CUL-H06179-37-994004

THE BRITISH JOURNAL OF PSYCHOLOGY

Edited by W. SLUCKIN

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37

VOLUME 63, 1972

CAMBRIDGE
AT THE UNIVERSITY PRESS
1972

PUBLISHED BY
THE SYNDICS OF THE CAMBRIDGE UNIVERSITY PRESS
Bentley House, 200 Euston Road, London, NW1 2DB
American Branch: 32 East 57th Street, New York, N.Y. 10022

Printed in Great Britain at the University Printing House, Cambridge

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Br. J. Psychol. (1972), 63, 1, *pp.* 1–7
Printed in Great Britain

CALCUTTA UNIVERSITY
105
'003
VOL. 63 NOS 1-4
FEB-NOV.
1972

ON MENTAL ENERGY

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The notion of mental energy as an account of why we are active at all is as old as the 'active principles' of Greek philosophy. It took hold on psychology with the instinct theories of Freud and McDougall and subsequently of the ethologists. The concept was adapted by the behaviour theorists and, under the influence of Cannon, by the energetics school. Currently the tradition is carried by arousal theory. The concept is criticized as being of uncertain ontological status, tending towards hypostatic explanation and as being an application of power engineering principles to a communication system. Current understanding of the reticular system offers little support for its proposed role as an energy source. It is argued that the central question of why we are active at all is an unnecessary one.

Our nature is one of Movement; to be completely still is to be dead. (Pascal, *Pensées*, 1661)

It is a salient characteristic of the pre-scientific mind that it peoples the world with irreducible agencies whose responsibility it is to crank the machinery of the universe and generate the dynamic phenomena of experience. In these busy agencies may originate our notions of cause and determinism. Typically it has been the activities and transformations of the world rather than its substance which have seemed problematical and thus have spawned irreducible 'active principles'. Above all, the growth and behaviour of living things have been attributed to an active principle. For Aristotle this principle was Nature, and phenomena were 'natural' which 'by a continuous movement, originating from an internal principle, arrive at some completion' (Aristotle, *On the Heavens*). More recently, natural science has not only blurred the distinction of substance and function (Cassirer, 1923) but has made of Aristotle's explanation an explicandum.

Such an attempt at explanation by rape rather than honest wooing still characterizes the psychology of motivation. Where the pre-scientific thinker falsely attributed a form of motivation to mindless processes the modern psychologist falsely explains behaviour in terms of a *motivational principle*. The apparent paradox in the latter accusation is resolved when this concept of motivation is examined in greater detail. A common element of the ancient and modern dilemmas is that the description also serves as the explanation. Accordingly the classification of the regularities of behaviour into dispositions has led to hypostasis and the dispositions have become the causal force underlying behaviour. Finally, a superordinate (and trivial) question as to why organisms are active at all has been met by an explanation in terms of a superordinate force of disposition, motivational energy.

It is not without significance, as Littman (1958) has pointed out, that motivational terms are replete with reference to force. Thus 'conation', from the Latin word for effort or impulse, was used by Huygens as the quantitative term for force, while Hobbes and Spinoza applied it to human motivation. So also 'motive power' is as much a power-engineering concept as a psychological one. Both the etymological and the conceptual origins of the term 'motivation' stem from a pre-Newtonian concept of principles of action. As Littman (1958) has indicated, 'Newton's first law wrested

motion from the grip which the concept of force had got on it and made of motion a physical passive...I suggest it is clearly unnecessary that there be psychological actives to set behavioral systems in motion; systems may be so constructed that they already have the properties of motion or activity.' This point of view is also reflected, for example, in Piaget's treatment of the dynamics of intelligence. As Flavell (1963) reviews it, 'For Piaget, then, the need to cognise is not fundamentally an extrinsic motive, separate from intellectual activity and pushing it, as it were from behind...Both biological and psychological organs are created through functioning and once created must continue functioning. The need to function cannot be separated from the functioning itself.' Both these writers are bleakly foreshadowed in Pascal's dictum which is taken here as a superscript.

The energy model of motivation deriving from 19th-century thermodynamics achieved its prototypic form through the instinct theories of Freud and McDougall. Freud's energy-conservation model is, as Bernfeld (1944) shows, an extension to motivation and psychopathology of ideas current in the Viennese circle of physiologists led by Freud's teacher, Brücke, and greatly influenced by Helmholtz. In the early 'Project for a Scientific Psychology' Freud (1895) provides his first formal statement of the energy model. He begins with a distinction of activity from rest in which activity is represented as a general quantity (Q), based on nerve action and subject to the laws of motion. Freud goes on to state the 'principle of neuronic inertia', which asserts the central notion that the nervous system acts to rid itself of activity. This principle is linked to the conclusions Freud was forming about the nature of hysteria which he had come to regard as a condition characterized by 'excessively intense ideas', conflicting, therefore, with the principle of inertia (Freud, 1895, p. 356).

Freud's treatment of the energy quantity (Q) as 'the impulsion which sustains all psychic activity' is in many ways similar to current notions of arousal, especially those qualified by drive-reduction postulates. 'Accordingly the precondition of sleep is a lowering of the endogenous charge in the ψ-nucleus, which renders the secondary function unnecessary. In sleep the subject is in the ideal state of inertia, with the store of quantity (Q) discharged' (Freud, 1895, p. 398).

Later versions of Freud's theory, which drop the physiological speculations of the Project, exhibit shifting interpretations of the energy concept. Energy is sometimes identified as the libido and sometimes distinguished as the force of self-preservation: 'this libido we distinguish from the energy which is at the basis of the psychic processes in general' (Freud, 1905). Furthermore, Breger (1969) has suggested that there is a shift in the development of Freud's theory from an open-system model in which environmental energy is acquired and discharged by the organism to a closed system model in which internally generated libido is shared amongst the activities of the organism.

While the physicalistic metapsychology of the Project continued to dominate the Freudian programme, it came into increasing conflict with his clinical observations. Klein (1969) has recently traced the distinction of Freud's energy discharge view from his hedonistic position in which sexual pleasure is sought in its various forms. In the latter sense, he points out, sexuality may be spoken of as *instinctive*, referring to a capacity, without implying the *drive* model. Freud became aware of this incon-

sistency in a form which was later to trouble drive-reduction theory when he asked why the experience of pleasure could result in a need for greater pleasure (Freud, 1905).

Several features of the Freudian model are prototypic. First is its continual resort to hydraulic analogy. This tends to illustrate the minimal technical advance of the model since Descartes. Concepts of flow, pressure and drainage abound in the deductions from the model. McDougall's model is even more overtly hydraulic, being replete with sluice gates, pipes, locks and even bubblings over (McDougall, 1923). As Hinde (1960) has shown, these properties are carried over into the ethological models of Lorenz (1950) and Tinbergen (1951) and, throughout, the power source has the property of a material substance flowing in an enclosed system rather than energy as strictly understood. A second characteristic exemplified in Freud's model is a set of deductions from the properties of the energy-like substance. These deductions all involve a form of the concept of pressure and they include the notions of repression, displacement, cathexis and catharsis. Repression refers obviously to containment and the build-up of pressure. Displacement describes the tendency of a blocked force to find a new outlet. Cathexis refers to the storage of pressure. Catharsis, of course, is the ancient Aristotelian joy of reducing pressure.

Since we have seen that demands made upon the properties of energy are very simple and rather remote from physical energy, we may fruitfully inquire more closely into the relation of mental to physical energy. Much of Hinde's (1960) analysis of these early models is devoted to their reality status. He shows that Tinbergen's and McDougall's models and the earlier Freudian ones have explicit physiological reference, but concludes that 'all have been influenced by the covert introduction of existence postulates concerning the explanatory concepts used' (Hinde, 1960). Hinde also argues that behavioural energy need not share any properties with physical energy since it is directed to an explanation of different phenomena. However, if we are not dealing with literal existence postulates (which cannot be explanatory of themselves) then deductive modelling is involved, in which case common properties must be shared, since this is the utility of models.

Hinde (1960) also raises the question of the number of forms of energy postulated. These early models are equivocal as to whether each syndrome of behaviour is fuelled by its own energy source. Only Tinbergen (1951) consistently maintains a unitary energy force. The number of energy sources postulated has shown a certain cyclical pattern. The instinct doctrine tended to lead to a proliferation of energy forms. As we shall see, the early behaviour theories opted for a restricted number of basic drives acting in a general and unitary manner. When the failure to deal with exploratory activities in terms of acquired drives threatened a new proliferation of basic drives the tradition passed to arousal theory and its single energy source.

More recently, Zeigler (1964) has questioned the application of the energy models to displacement behaviour in the ethological sense. The crux of Zeigler's criticism is that displacement activities need not be explained as overflow of aroused energy since the activities are not as irrelevant to the environmental situation as assumed by the energy theorists. Zeigler cites a variety of evidence to show that the cues for such displacement activities as feeding and grooming are almost always present. Such activities also have a low trigger threshold. Finally, they tend to occur when an

animal is in a state of equilibrium between competing response tendencies. Therefore neither the notion of overflow energy from a baulked primary activity nor that of cathartic relief is necessary to account for displacement behaviour.

Taylor (1964) has addressed himself to a quite different aspect of the hydraulic models in his treatment of the teleological characteristics of motivation. Illustrating his argument with reference to Tinbergen's theory, he inquires whether a theory which separates the arousal of behavioural energy from its direction (under stimulus control) can dispense with the notion of a goal or purpose. Taylor shows how the hydraulic models' failure to deal with response equivalence calls into question the notion of a directionless energy. If it is conceded that acts, rather than responses, are an unavoidable part of psychology's data language then the lack of any teleological property in the concept of motivational energy seems to render it superfluous at precisely the level of explanation to which it is normally applied.

With the descent of the instinct doctrine the tradition of energy models divided, being pursued on one hand by the S-R behaviour theorists and on the other by the first-generation arousal models of the energetics school. The behaviour theorists and subsequently the arousal theorists began by partitioning their theories into dynamic and static elements. In the classic Hullian version, learning leads to the acquisition of habit strengths ($_sH_r$) which eventuate in performance due to the action of D, a general energizing drive which 'seems also to have a generalized but possibly weaker tendency to facilitate action of *all* effectors, giving rise to a degree of undifferentiated motivation analogous to the Freudian libido' (Hull, 1943). The problem for the S-R theorist was that having imbued the organism with a structure of learned S-R associations they still had to get the creature to perform and to explain variations in performance to constant inputs. It seldom seems to have occurred to the S-R theorists that when the animal did not perform its instructed responses it was nevertheless active and behaving rather than deflated and inert. The theoretical distinction of active and passive processes is stated explicitly by Brown (1961): 'Thus, drives, motivations, conations, emotions and libidos function as the activating agents, while cognitive maps, associative tendencies, and habit strengths serve, in conjunction with external and internal stimuli, to determine the direction behavior will take.'

One of Brown's major reasons for invoking the concept of motivational energy is the disproportion between the energy of stimuli and responses. Curious though it may seem, this observation has frequently been interpreted as favouring the motivational energy principle. However, first, this disproportion is only surprising if we accept the venerable switch-board model where inputs are rerouted into outputs in a simple open-cycle system. Second, Brown's interpretation of the S-R energy disproportion is based on a confusion of levels of discourse reaching its peak with the energetics school. There is indeed a level at which the organism can be described as an energy system with the capacity to do work. But this is quite distinct from the level of information processing and decision which constitutes the psychological problem. For example, the energy of an information input is trivial and a communication system might even code inputs by energy decrements. Concepts such as Hull's stimulus intensity dynamism fail to distinguish a photon on the retina from a food pellet in the belly. It is this confusion of metabolism with mind that inspired Wiener (1948)

to comment: 'the conservation and degradation of energy are the ruling principles of the day...it is the metabolic balance which is the centre of attention...'. But, he argues, 'the bookkeeping which is most essential to describe their (neural) functions is not one of energy'.

Such confusion is especially typical of the energetics school of Freeman, Duffy and others, whose tradition stems from Cannon and runs in parallel with the S-R behaviourists. The extension of a metabolic model to cover brain function characterizes Freeman's (1948) approach. 'The function of stimulation', Freeman states, 'is to liberate energy within the organism.' The pressure analogy of the instinct models is carried over in such threat-laden statements as: 'now unless the excitation... aroused by stimulus is taken out of the system by expressed overt reaction...'.

Responses in the energy model function not to transform the organism's input but to prevent the model from exploding. The confusion of naming with explaining and the unacknowledged shift from a metabolic to a psychological language may be pursued through Freeman's book in such phrases as 'the habitual readiness (basic energy) level' and again, 'due to a superabundance of potential but unmobilized bodily energies, the neuromuscular system is alerted'.

Freeman's view of the stimulus as a step function event which 'happens' in a passive sense and by its advent produces disequilibrium in the energy system gives no account of the information function of stimuli and exhibits a failure to comprehend that stimuli are sought, that they test and terminate behavioural operations (Deutsch, 1960; Miller *et al.*, 1960).

Turning to Duffy's influential concept of energy mobilization, these criticisms continue to hold. It may be doubted whether Duffy appreciates the metaphorical status of the energy concept. She writes: 'The term energy mobilization means nothing different from energy release within the organism, or the conversion of potential energy to kinetic energy' (Duffy, 1951). While Duffy recognizes that the intensive aspect of behaviour derives not in work in any physical sense but in 'the degree of effort required by the situation as interpreted by the individual' (Duffy, 1951), she goes on seriously to propose the expression of psychological arousal in calories per minute (Duffy, 1951, footnote 7).

Again hydraulic ghosts haunt Duffy's model as when she attributes errors of commission in the C-type reaction to 'overflow energy' (Duffy, 1932). Decision processes do not arise in the energy model.

The arrival of contemporary arousal theory was attendant upon three major developments. First were certain difficulties encountered by the conservative S-R theories relating to the increasing tendency towards research on higher species and the consequent amazing discovering that animals were curious and exploratory and frequently preferred to pursue their own interests rather than those of the experimenter. Such motivation-sustaining propensities could scarcely be reconciled with the notion that animals acted to discharge energy and reduce drive. The counter-argument that a hitherto unsuspected drive to explore was being discharged was regarded as too convoluted by all but the most faithful. Second was the increasing currency of electrophysiological measurement of autonomic activity and the gross activity of the brain and skeletal musculature. This led to a continuum being posited from sleep to states of intense excitement. Thereby it was hoped to achieve a non-

circular definition of drive by independent physiological means. Finally, and most formatively, came the work upon the reticular system of the midbrain with the widely accepted interpretation of the reticular system as a global and unitary activator of the cortex and *sine qua non* of wakefulness and activity. The result of these developments was that mental energy regained its health and went to live in the reticular formation.

Hebb, who had argued in 1949 that the brain is built to be active and that motivation *qua* activity is autochthonous, reversed his position in his influential treatment of arousal (Hebb, 1955). Citing the early sensory deprivation studies as making trouble for his original treatment of motivation, he asked: 'If the thought process is internally organized and motivated, why should it break down in conditions of perceptual isolation?' Hebb's curious atavism appears to be based on a failure to see that to maintain that activity is intrinsic to the system is not to claim that it is independent of sensory feedback. To acknowledge this is merely to concede that behaviour is adaptive.

In many ways Hebb's (1955) position and that of Malmo (1959) is merely a restatement of the S-R drive doctrine giving anatomical names to hypothesized functions. Thus the relation of static habits to dynamic drive is translated into the action of the ascending reticular activating system (ARAS) upon the cortex.

It is not possible here to pursue the details of reticular studies since the mid-century (cf. Thompson, 1967). However, the picture as a whole suggests that the response of the system is more selective than had been thought, that the system possesses considerable internal differentiation and that it forms part of a complex of servo-loops within the midbrain and via cortex and peripheral autonomic and somatic pathways. Furthermore, it has become evident that behaviour arousal, EEG activation and autonomic activity are independent (Lacey, 1967). Finally, it seems possible that the reticular system discriminates its inputs. The fact that the components can talk to each other so freely equips it with the means to decode its convergent inputs. The incorporation of decision rules into such a system has led Kilmer, McCulloch and Blum to postulate a plastic model of reticular function (W. S. McCulloch, personal communication). These authors begin from the premise that 'throughout the life of the vertebrates, the core of the central nervous system, the reticular formation, has retained the power to commit the whole animal to one mode of behaviour rather than another'. They attempt to show by simulation how a structure, comprising similar modules, wide but shallow in computation and densely interconnected, could pass as an integral unit from mode to mode committing the system of which it is part to overall classes of activity. The assumption is made that these strategic decisions engender 'function setting' whereby the system puts out control signals which tune and set the filters on all inputs. All this is, of course, a far cry from the arousal theory of the ARAS as a unitary and unselective energizing force.

The metamorphosis of the energy concept in arousal theory may be assessed in Bakan's (1963) words: 'I tend to think of arousal as a source of energy; you turn on the tap, and it can go out different pipes.'

In conclusion, then, the notion of mental energy remains with us. Underlying the changing technological flavours of the energy model is a metaphysical mechanics embedded in our language and atavistic notions of cause and drawing sustenance

from our ability to anthropomorphize the engineering of power systems. The concept of mental energy survives in the belief that behaviour must have its *fons et origo*, a sort of psychological phlogiston. Rather, the problem for psychology is decision and choice and not 'why are we active at all?'

This research was supported by the University of Guelph Advisory Board, Grant 575–72. A version of this paper was presented to the Canadian Chapter, American Society for Cybernetics, at Toronto in May 1970.

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(*Manuscript received* 2 *December* 1970; *revised manuscript received* 19 *March* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 9–30
Printed in Great Britain

THE MODEL OF COGNITIVE ORIENTATION: TOWARDS A THEORY OF HUMAN BEHAVIOUR

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This article presents the main assumptions and processes of a model for the analysis and prediction of behaviour, mainly on the molar level. Analysis of findings concerning the orienting reflex shows that a cognitive determination of the input's denotative meaning ('meaning action') is an indispensable antecedent for the elicitation of the orienting reflex, adaptive and defensive reactions, and CRs. If these responses fail to cope with the stimulus situation, meaning is further elaborated ('meaning generation'). Interaction between the input's connotative meaning and the cognitive orientation components (beliefs about self, norms, goals and environment) produces a 'cognitive orientation cluster', manifested in a 'behavioural intent', which turns into behavioural output through the mobilization and adaptation of innate and learned 'plans'. Supportive empirical findings and general theoretical implications are discussed.

The resurgence of interest and research in cognition during the last decade has stimulated the development of an increasing number of theories and models dealing with the influence of cognition on behaviour. Common to all is the attempt to explain behaviour by means of cognitive processes. However, most of these theories and models deal only with limited aspects of cognitive processes and of the resulting behaviour. Examples include the various theories of balance and cognitive dissonance (Festinger, 1957; Heider, 1958; Newcomb, 1953), models of communication and attitude change (Hovland *et al.*, 1953; Osgood & Tannenbaum, 1955), of risk-taking (Kogan & Wallach, 1964) and decision-making (Kyburg & Smokler, 1964; Luce & Raiffa, 1957), theories of cognitive control (Gardner *et al.*, 1959; Klein, 1954; Rapaport, 1957), social learning (Rotter, 1960) and cognitive appraisal (Lazarus, 1966), models about cognitive structure, information processing (Crockett, 1965; Schroder *et al.*, 1967) and concept attainment (Bruner *et al.*, 1956), computer simulation models of cognitive performance (Newell *et al.*, 1958; Reitman, 1965) and other cognitive theories of behaviour (Harvey, 1963; Kelly, 1955).

Models such as these allow predictions of direction and/or intensity of specific forms of behaviour within the circumscribed domain of their application. On the other hand, a theory such as that suggested by Miller *et al.* (1960) seems indeed to apply to all forms of behaviour but until now it has not been subjected to experimental confirmation and has fulfilled only *post hoc* explanatory functions.

Thus those cognitive models which have withstood experimental confirmation in the prediction or modification of behaviour are too limited and one-sided to allow for their extension into general behaviour models, while the general behaviour models have not yet undergone experimental examination. However, when the limited models are considered together, they seem to demonstrate that cognitive processes allow for a better understanding and more precise prediction of most, if not all, forms of human behaviour than most other variables do. Yet it would be fatuous to expect that the attempt to combine the various models, which are based on an array of divergent theoretical assumptions, could lead to a fruitful integration. It seems more adequate to approach this task in a different way. Empirical and theoretical

developments already allow for the construction of a model which, on the one hand, would be general enough for integrating confirmed findings of cognitive psychology within the framework of one cognitive theory of behaviour, but on the other hand would be structured enough to allow for new experimental predictions and perhaps also for the manipulation and change of various forms of behaviour.

It is the purpose of the present article to describe the outline of such a model. We call it 'the cognitive orientation model of human behaviour' (CO model). The term 'cognitive orientation' is meant to suggest the assumed cognitive nature of orientative processes. It is also designed to express implicitly our three basic assumptions. These are: (*a*) the striving to establish cognitive orientation is a primary tendency of humans and perhaps also of the higher animals; (*b*) human behaviour above the level of spinal reflexes is steered or directed by cognitive orientation, its contents and/or processes; (*c*) information about a specific cognitive orientation allows one to predict the form and direction of ensuing behaviour, provided that the stimuli and the available forms of behaviour are known. As a corollary of the above, we also hypothesize that experimentally induced change of cognitive orientation will produce predicted change(s) in behaviour, i.e. in its form and direction.

COGNITIVE PROCESSES AND MEANING

Before an attempt is made to describe experiments confirming our basic assumptions and to discuss the CO model – its processes, implications and relation to other cognitive models – it is important to specify first the sense of the term 'cognitive'. During recent years the meaning of this concept has undergone a marked extension, so that it has come to cover all processes intervening between sensory input and motor output. Neisser (1967, p. 4), for example, writes: 'The term "cognitive" refers to all the processes by which the sensory input is transferred, reduced, elaborated, stored, recovered and used.' It seems to us that this as well as similar definitions are so general that it becomes hardly possible to specify with any degree of certainty which processes are not cognitive and which psychological theories should better not be termed 'cognitive theories'. From Neisser's definition it follows, for instance, that the theories of Hull, Freud, Skinner and Tolman should all be considered as cognitive. In order to avoid such blurring of basic distinctions we suggest defining as cognitive those processes which are involved in the production and processing of a certain kind of meaning. Production of meaning consists in turning neuronally coded stimuli into meaningful representations.

The definition of the term 'meaning' which we propose is the product of a series of studies in the domains of lexical (interpersonally shared) and symbolic (personal) meaning (S. Kreitler, 1965; S. Kreitler & Kreitler, 1968). According to this definition, meaning is a value or a pattern of values along one or more of a set of specified dimensions. The dimensions – many of which correspond to limited definitions of meaning suggested by other investigators – were found by categorizing a large number of response units, verbal and non-verbal, each referring to a definite aspect of a stimulus concept or term, yielded by subjects whose task was to communicate meanings in a standard experimental situation. The lexical meaning dimensions are: indication of the referent's (1) contextual allocation; (2) function, purpose or role;

(3) place or domain of existence or application; (4) causes for occurrence; (5) what it consists of or what it includes; (6) manner of occurrence; (7) sensory qualities, size, weight and quantity; (8) consequences which result from its existence or activity; (9) action(s) and potentialities for action(s); (10) history or past development; (11) temporal qualities; (12) similarity or contrast to other referents; (13) evaluation of the referent. Examples of the 10 symbolic meaning dimensions include 'an exemplifying instance', 'bodily expression and movement', 'sensation and feeling' and 'metaphor'.

The above definition of meaning as a pattern of values along specific dimensions enables one both to distinguish between meaningless and meaningful representations as well as to measure, i.e. specify and compare, the extent of connotation characteristic of meaningful representations. The suggested definition of meaning implies that a representation is meaningless when not even one value along any one dimension has been established with regard to it. In contrast, a representation would have a minimal degree of meaning when at least one value along at least one dimension has been established with regard to it. The degree of meaningfulness of a representation, and hence the extent of its connotation, increases with the number of different values and different dimensions of which its meaning consists. At first glance it might seem that the extent of connotation may increase indefinitely. However, it is plausible to assume that the maximum of meaningfulness is reached when the meaning of a representation includes an x number of values along all of the specified 23 dimensions (or more when further dimensions are discovered). Beyond a certain x number of values per dimension the meaning may become overdetermined and lose its specificity through overlapping with the meanings of other representations.

Since the meaning dimensions have been extracted from intentional communications by subjects, they seem to us to be applicable only on the level of perceptual, conceptual, and/or linguistic processes, i.e. on the level on which meaning values may adequately be designated as beliefs, concepts, or at least as denotative percepts ('denoceptions'). However, the principle underlying the suggested definition of meaning could be extended to the treatment of meaning on other levels, for example, the meaning of the patterns of electrical impulses in synapses. Such extensions would undoubtedly lead to the discovery of meaning values different from percepts, concepts or beliefs, and could thus require the establishment of meaning dimensions different from the ones defined by us. In general, the definition of functional levels in the organism in terms of values and meaning dimensions characteristic of each level could prove useful in clarifying the relations among the levels, possibly even the intriguing relations between so-called body and mind.

These remarks about the nature of meaning permit a further specification of the above-suggested definition of the term 'cognitive' as referring to processes through which meaningful representations of a certain type are produced and manipulated. The definition may now be amplified by adding that the relevant type of meaning is given when beliefs, concepts or denotative percepts ('denoceptions') take the form of values or patterns of values in a space defined by the lexical and sometimes also by the symbolic meaning dimensions. Accordingly, only representations of this nature and their processing would appear as the subject-matter of cognitive psychology.

By means of the amplified definitions of the terms 'cognitive process' and 'meaning', we hope to be able to show that it is both justifiable and necessary to assume the

existence of a primary tendency to establish cognitive orientation (assumption (*a*)), i.e. to make representations meaningful, and that the meaning of representations has a directive or steering role with regard to behaviour (assumption (*b*)).

TOWARDS THE LOWEST LEVEL OF COGNITIVE FUNCTIONING

McClelland (personal communication) once remarked that Skinner's great contribution arises from his daring endeavour to explore how far up one may get along the hierarchy of increasingly complex behaviours by using only the simplest conditioned responses as the primary building blocks. The contrary would be true for those who are seriously concerned with a cognitive theory of behaviour. Owing to the close interrelations between simple and complex forms of behaviour, it is incumbent on cognitive theorists to explore the lowest behavioural level on which cognitive processes are operative, if their hypotheses about higher behavioural manifestations are not to remain as precarious as a house without fundaments.

Our first assumption about the primacy of the striving to establish CO, and the left-hand part of our model (Fig. 1) reflect some ideas designed to contribute towards the erection of these fundaments and towards the bridging of the still wide enough gap between the simpler and more complex forms of behaviour. However, since the role of cognitive processes in the elicitation of unconditioned and conditioned reflexes was discussed elsewhere (Kreitler & Kreitler, 1970*b*), we shall limit ourselves here to summing up the main arguments, mainly in so far as they are relevant to the CO model of behaviour.

The major conclusions which support the primacy of the striving for CO (assumption (*a*)) and the crucial role of cognition in the elicitation of simple behaviours derive from an analysis of findings about the orienting reflex (OR). Since the OR was identified as the first reaction of the organism to any new stimulus, and since the evocation of the OR is a precondition for the acquisition of conditioned responses (Lynn, 1966), there is – at least at the present stage of research – no theoretical basis for the assumption that orienting tendencies are acquired through learning. Rather it has to be assumed that the OR is a primary unconditioned response. Since, however, the OR appears whenever orientation is missing or incomplete and is habituated through orientation, this assumption also implies that the tendency to be oriented is primary.

Yet the assumption that orientative tendencies are innate in humans and probably also in the higher animals does not necessarily imply that orientative behaviours are essentially cognitive, as is suggested in assumption (*a*). Had it been true that every perceivable change in a stimulus situation elicits an OR, Sokolov's (1963) hypothesis about the neuronal model could provide a satisfactory physiological account of orientative behaviours. There would not be any need then to suggest the involvement of cognitive processes in the elicitation of behaviour at this low level of complexity, and Berlyne's (1960) assumption that cognitive processes are a product of ORs and exploration could be accepted. However, ORs evidence certain peculiarities which undermine Berlyne's behaviouristic theory and necessitate a modification of the neuronal model hypothesis in a cognitive sense.

The term 'neuronal model' designates 'a certain cell system whereby the information is stored concerning the properties of a stimulus which has been applied many

times. The term suggests that the nervous system produces an exact model of the properties of external objects acting upon the sense organ' (Sokolov, 1963, p. 286). Hence, whenever an incoming stimulus does not coincide with a previously produced and still existing neuronal model, an OR is elicited. In the present context it would be futile to discuss whether the primitive kind of matching between the new stimulus and the neuronal model should be regarded as a cognitive process or could be accounted for in physiological terms, because a purely physiological interpretation of the OR is anyway ruled out by other findings. For instance, if in a series of stimuli which has been habituated one stimulus is replaced by a different stimulus, which is also known, the OR reappears. In an attempt to explain this phenomenon Mackworth hypothesizes that the 'brain is continually constructing and modifying models of incoming stimuli, and from these models it extrapolates into the near future' (Mackworth, 1968, p. 308). Moreover, it has often been shown that an OR occurs if the representation of the incoming stimulus differs to a sizable degree from the neuronal model, which contains the dimensions of the probable stimulus. The degree of discrepancy between the stimulus and the model determines the strength of the ensuing OR (Sokolov, 1963, p. 288). However, processes such as the plotting of a probabilistic neuronal model, determining the relevant limits of dissimilarity which change from one situation to another, and especially extrapolating into the future, cannot be explained – at least at the present stage of research – without taking recourse to a cognitive theory of stimulus elaboration.

Further facts may serve to highlight other weak points in the automatically functioning neuronal model as an explanation for the OR. Johnson (1968) demonstrated that a CS does not elicit an OR minutes and sometimes even hours after the occurrence of conditioning. The implication for the neuronal model hypothesis is that the neuronal model of the CS has to be preserved for hours and be kept continuously ready for quick matching. However, in the meantime, until the CS recurs, other stimuli were perceived and turned into familiar stimuli through the formation of their respective neuronal models. Thus when the original CS appears, it has to be matched against a considerable number of neuronal models, each of which should be highly differentiated and exact so as to enable the discrimination of the corresponding stimulus from the always present background noise (Mackworth, 1968, p. 318). It is hard to see how the neuronal model hypothesis can explain why the scanning of so many neuronal models and their unavoidable mismatch with the CS does not elicit ORs before the previously well-established CR reappears.

Another weakness of the neuronal model hypothesis becomes apparent when we consider the problem of significance of the stimulus or of certain stimulus features. Studies of semantic generalization (Luria & Vinogradova, 1959; Razran, 1939, 1961; Riess, 1940), some of which have used the OR as an indicator, show that generalization is greater to words with similar meaning and dissimilar sound than to words with dissimilar meaning and similar sound, unless the experimental instructions emphasize acoustic similarity. This means that the same neuronal model can be used selectively, for instance, once in order to match acoustic characteristics and at another time in order to match meaning characteristics of a stimulus. However, the neuronal model hypothesis does not provide for this kind of feature selection.

In view of these and other difficulties we (Kreitler & Kreitler, 1970*b*) suggested

that neuronal models of the kind outlined by Sokolov do not last longer than the actual stimulus situation and its iconic memory. That which is stored in long-term memory (Bartlett, 1932), and probably also in short-term memory (Neisser, 1967), are schemata which allow for the partial or complete reconstruction of neuronal models so that matching can be performed. Retrieval, i.e. scanning of stored schemata and reconstruction of neuronal models, and even the matching of a reconstructed neuronal model against the representation of a changed stimulus situation, are highly selective processes. The assumption of selective matching implies that the search for similarities and dissimilarities is restricted to specific features of the stimulus situation or, to use Bruner's (1957) terminology, it is performed in accordance with certain categorization principles. The same may be assumed to hold with regard to scanning of stored schemata, and to a certain extent also for the reconstruction of neuronal models.

The principles guiding the selective retrieval and matching are implied in the above-mentioned dimensions of meaning. Since each meaning dimension reflects a specific aspect of the stimulus-referent, we venture to hypothesize that the meaning dimensions are in fact rules which determine that 'categorization process' (Bruner, 1957) out of which all perceptual experience derives. Each of these rules could function as an 'address' which directs scanning and limits matching to the values of the applied meaning dimension(s). Since a match between the values of the input stimulus and the partly or fully retrieved, namely reconstructed, neuronal model results in the determination of denotative meaning, we suggest the term 'meaning action' for the selective scanning, the retrieval and the matching process, even when the derived meaning serves only as a signal for a defensive reaction. Moreover, since the same principles guide both the scanning and reconstruction of neuronal models as well as the matching against the stimulus representation, these various processes could be carried out concomitantly and could sometimes be concluded even before the full-fledged neuronal model is attained (Norman, 1968, p. 530). The meaning dimensions most likely to be involved in the determination of a minimal denotative meaning are the lexical dimensions of 'sensory qualities', 'contextual allocation', 'similarity or contrast', 'action and potentialities for action' and 'evaluation'. Of course, it is unnecessary to assume either that all of these dimensions are applied for the determination of meaning in every case of a simple response or that the dimensions are of equal importance in identifying the stimulus for the purposes of a simple response. However, following Neisser's (1967) findings, it is plausible to conclude that the application of these different meaning dimensions for the immediate recognition of stimuli is simultaneous rather than sequential.

Thus it is the role of 'meaning action' to establish meaning values of a kind and to an extent which would enable the elicitation of a defensive or adaptive reflex or a CR. In case 'meaning action' fails in this task, an OR is released. This would occur when, in the extreme case, the only unambiguous meaning value which could be established is 'contrast' or 'dissimilarity', or when the determined meaning values do not have that signal function which could release an innate or conditioned response. Through its motor and autonomic components which facilitate the acquisition of further information, the OR can lead to the formation of new representations of the stimulus situation, which are subjected to a further 'meaning action'. This process could result

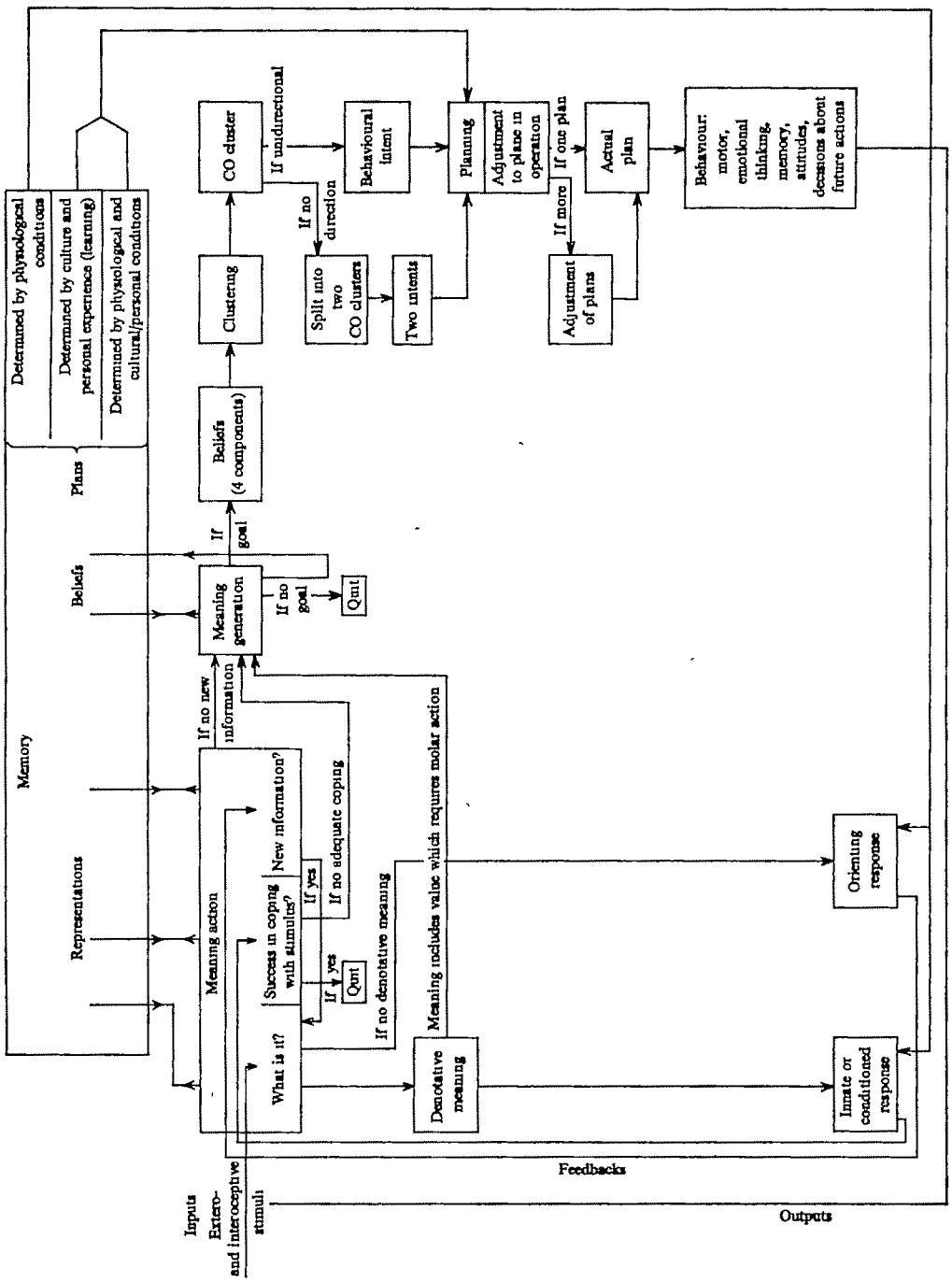

Fig. 1. A flow chart of the CO model of human behaviour.

in denotative meaning which proves sufficient for the elicitation of an innate or conditioned response, although this is not necessarily the case. Accordingly, the operational definition for the sufficiency of denotative meaning is the habituation of OR and/or the elicitation of innate or conditioned responses other than the OR. The left-hand part of Fig. 1 may serve to clarify the described sequence as well as the conditions under which it is terminated or continued on a higher functional level.

The above considerations clarify the cognitive nature of those processes which enable the organism to react with an OR or another unconditioned or conditioned response on this relatively simple level of behaviour. Since the determination of meaning is the very essence of cognitive orientation, it is clear that the establishment of at least a minimal cognitive orientation necessarily precedes behavioural and specifically motor output, namely enables its occurrence and directs its course, as suggested in assumption (*b*). Moreover, since 'meaning action' is the set of processes involved in the determination of denotative meaning, it is likely that 'meaning action' constitutes the most basic unit of cognitive functioning.

THE COMPONENTS OF COGNITIVE ORIENTATION

Reflexes are sometimes, but by no means always, the end-point of a behavioural sequence designed to cope with a stimulus. They often prove to be insufficient, so that molar behaviour is mobilized. We assume that this occurs under the following three conditions: (*a*) when, in spite of improved perception produced by OR, no new information about the stimulus object and stimulus situation is yielded, while the existing information is insufficient to bring about the establishment of denotative meaning and thus also the inhibition of the OR; (*b*) when denotative meaning has been established and a reflex or a CR has been triggered off, but feedback information reveals that this coping process has not led to an adequate mastery of the situation; (*c*) when the pattern of denotative meaning includes a value which expresses the requirement for molar behaviour, e.g. when the meaning value is a signal for an instinctive sequence of behaviour, or when it represents information from former experiences that molar behaviour is necessary for coping with that stimulus.

It would be plausible to assume that the transition from the level of reflexes and CRs to the level of molar behaviour is a gradually unfolding process and not an all-or-nothing event. The reasons for this suggestion are, first, that observation does not allow the drawing of sharp distinctions between innate or conditioned reflexes and molar behaviour, and secondly, that the difference between denotative meaning, which determines innate and conditioned reflexes, and connotative meaning, which determines molar behaviour, is merely a difference in degree or quantity. The transition from one behavioural level to the other, which occurs when one of the mentioned conditions is present, consists in the continuation and expansion of 'meaning action'. In the course of this process – which we call 'meaning generation' in order to distinguish it from 'meaning action' – more denoceptions as well as increasingly complex representations stored in long-term memory are elicited. 'Meaning generation' leads to the determination of meaning values which differ from denoceptions in that they appear in larger units, i.e. beliefs. This view also implies that 'meaning generation', in contrast to 'meaning action', is a process more characteristic of humans than of animals.

The term 'belief' is defined as a meaningful unit of two or more grammatically related meaning values. When expressed in verbal terms it usually takes the form of a proposition or a negation and may be defined – along with Abelson & Rosenberg (1958), Bem (1970, p. 4), Fishbein (1967), Rokeach (1968, p. 113) and others – as referring to the relation between at least two cognitive elements, e.g. an object and a characteristic of it, or an object and another object. It should, however, be noted that the related elements are meaning values, single or in patterns, and that due to the grammatical connexion, the meaning of the whole unit surpasses the meanings of its constituents. The term 'belief' is used here to denote the basic unit of cognitive contents on the molar level, regardless of the source, veridicality, interpersonal sharedness, rationality, and degree of distortion of reality in these contents.

The above discussion implies in general terms that molar behaviour is directed by beliefs. Operationally this implication leads to the expectation that behaviour may be predicted from the beliefs relevant for it. However, in so far as attitudes are conceived as reflecting cognitive contents, most of the evidence refutes the hypothesis that cognitive contents are related to behaviour (e.g. Bray, 1950; Brody, 1965; Corey, 1937; Fishman, 1961; Friedmann, 1958; Holman, 1956; Kutner *et al.*, 1952; Lakie, 1964; Linn, 1965; Zunich, 1961). The different hypotheses suggested for explaining this unexpected low or missing relationship (e.g. Campbell, 1963; Ehrlich, 1969) tend to emphasize the role of factors other than attitudes in the co-determination of behaviour. However, since the standard studies on attitudes and behaviour suffer from restrictions in themes, in the sampling of cognitive contents, and in the range and form of the assessed behaviours, there can be little doubt that they are hardly adequate to test the limits of the hypothesis that cognitive contents direct behaviour. Therefore the first empirical step incumbent upon us was to clarify whether there exists at all a correspondence between beliefs and specific forms of behaviour. In principle, the problem can be approached through two lines of research. The first consists in starting with a certain act of behaviour and looking for the belief cluster whose meaning corresponds to that behaviour, i.e. whose beliefs point in the direction of that behaviour, whereas the second consists in starting with a belief cluster and examining whether the behaviour implicated by it actually occurs. In our earliest studies we focused on the first line of research and explored the relation between beliefs and steady or recurring behavioural forms or tendencies. In the first of these studies (Kreitler & Kreitler, 1965), conducted with 40 schizophrenic hospitalized subjects and a matched control group of 40 normals, we found that while single beliefs hardly differentiated between the groups, there was a series of belief clusters which appeared in at least 80 per cent of the schizophrenics and in no more than 10 per cent of the normals. These clusters referred to themes such as good and evil, ambition and humility, freedom and fulfilment of duties, etc., and could be related to characteristic schizophrenic symptoms. Similarly, in another study with 40 professional burglars, 24 intentional murderers and an adequate control group of normal non-criminals we found a series of belief clusters which clearly differentiated the criminal subjects from the others, and were meaningfully related to the criminal acts.

Apart from demonstrating relations between cognitive contents and behaviours, these studies helped to generate a procedure for sampling belief types, so that in ensuing studies belief clusters could be used successfully for predicting behaviour.

The observation that beliefs in belief clusters which correlated highly with behaviour seemed more heterogeneous than beliefs in belief clusters which did not correlate with behaviour gave rise to the idea that it might be useful theoretically and methodologically to distinguish between different types of beliefs, which have to be combined in clusters so as to yield significant correlations with behaviour. The categorization of beliefs we finally adopted derives from two sources: first, an examination of the contents of beliefs in the clusters which were correlated with behaviour in the above-mentioned studies, and secondly, correlational studies (unpublished) of beliefs on various themes such as achievement or success and failure. The categorization itself is based formally on two identifying features, the first relating to the subject of the belief, i.e. self or non-self, and the second relating to the kind of relation between subject and object, i.e. factual, desirable ('ought', 'should'), and desired (wished for). Since in our studies beliefs about non-self referring to the 'factual' and 'desired' were highly correlated, while 'desirable' for self could be easily deduced from 'desirable' for non-self and 'desired' by self, the six possible types of belief which could be formed by all the combinations of the two features were tentatively reduced to four types. These are: (*a*) *beliefs about self* (BS), which formally consist of the meaning value 'I' as a subject linked to another meaning value by a factual relation, and express information about oneself or any aspect of the self in the present or the past, e.g. one's habits, actions, abilities, feelings, sensations, etc. (e.g. 'I never lie'); (*b*) *general beliefs* (GB), which formally consist of a meaning value that is a non-self linked as a subject to another meaning value by a factual relation, and express information about people, objects, situations, events or any aspect of these (e.g. 'People lie a lot'); (*c*) *norms and rules* (N), which formally consist of a meaning value that is a non-self linked as a subject to another meaning value by a positive or negative 'should' relation, and express generalized rules and standards of an ethical or non-ethical nature, applied to any person, object, event, etc. (e.g. 'People should not lie'); (*d*) *goals* (Go), which formally consist of the meaning value 'I' as a subject linked to another meaning value by a relation of desire or avoidance, and express future actions, objects, situations, etc. desired or rejected by the self (e.g. 'I want to lie less').

It may be noted that these four types of beliefs proved sufficient in order to classify all items of many currently used personality inventories and attitude scales, the majority of which include beliefs of only one or at most two types. Moreover, the classification itself includes most, if not all, content-types of beliefs distinguished by other investigators. For example, Rokeach (1968, p. 113) distinguishes between descriptive-existential beliefs (GB), evaluative beliefs (mostly GB) and prescriptive-exhortatory beliefs (N), and places a special emphasis (*ibid*, p. 165) on cognitions about own behaviour (BS) as against cognitions about significant others and non-social objects (GB); Fishbein (1967) distinguishes between beliefs about the components, qualities and interrelations of objects (GB), beliefs about what should be done with the object and what it should be allowed to do (N), and beliefs about the object's relation to personal goals (Go); Parsons *et al.* (1951, pp. 162–4) classify cultural elements into cognitive (factual), affective (relevance to goals) and evaluative (normative); Kluckhohn (1951) differentiates between values (N), goals and existential (factual) propositions; while Lazarus (1966) distinguishes – as many others

do – between beliefs about the general environment (GB) and beliefs about oneself (BS).

However, the main evidence in support of this quadripartite classification of beliefs derives from a series of studies (Kreitler & Kreitler, 1969, 1970*a*, 1971) which deal with predicting behaviours, manifested under controlled laboratory conditions, on the basis of cognitive contents. For the purpose of illustrating the methodology and theoretical implications of the studies, some findings from one study will be presented in greater detail.

The study (Kreitler & Kreitler, 1970*a*) dealt with using cognitive contents for predicting long-term and short-term behaviours, attitudes and commitments to action reflecting achievement. In the present context we shall concentrate on short-term achievement behaviour. The behaviour was assessed through the accomplishments of subjects on three performance tasks, each administered under a different set of experimental conditions. The tasks were all overlearned routine tasks which enabled the manifestation of individual differences in effort and achievement striving rather than in training and speed of learning. The standard instruction to the subjects was 'to solve the items as correctly and as quickly as possible'. The first task (duration 15 min.) consisted of Düker's two-step one-cipher arithmetic exercises, and was administered under neutral conditions. The second task (8 min.) consisted of subtraction exercises in two 10-digit rows, and was administered after a feedback of success ('post-success') for a previous similar task. The third task (8 min.) consisted of addition exercises in three six-digit rows, and was administered after a feedback of failure ('post-failure') for a previous similar task. The major performance variable was an index constructed so that each exercise solved correctly was given a two-point weight and each exercise solved incorrectly was given a one-point weight.

The subjects' cognitive orientation was assessed by means of questionnaires, which included beliefs relevant to achievement. The beliefs were selected from a larger pool of beliefs which included, first, statements about the meaning of 'achievement' made by subjects, of the same population as the experimental subjects, in the course of a pretest in which they were required to communicate the meaning of 'achievement' according to the method developed by S. Kreitler & Kreitler (1968); and secondly, beliefs constructed by the experimenters, each of which included at least one of the recurring meaning values of 'achievement' (e.g. effort, luck, prestige) used by at least 50 per cent of the subjects in the pretest. The selection of the beliefs for the questionnaires was made initially in line with the judges' evaluation of the clarity, non-suggestibility and orientational direction of the available statements, and later in accordance with findings about the discriminative power of the items and the reliability of each of the four parts of the questionnaire.

In their final form the four questionnaires included beliefs referring to the same set of themes, e.g. the conditions under which achievement and success occur, the role and evaluation of effort, etc., and differed only in the formulation of the beliefs and in the instructions to the subjects. In each questionnaire half of the beliefs were presumed to be orienting towards achievement and half towards non-achievement. These two types were presented to the subject in groups of two, four, six or eight beliefs. The subject was requested in the N part to choose out of 16 statements eight which 'express your opinion about how people should behave'; in the GB part – out of

30 statements 15 which 'describe how people actually behave'; in the BS part – out of 14 statements seven which 'describe best how you are and how you usually behave'; and in the Go part – six statements which 'express your goals and wishes and how you would like to be'.

In each of the CO components the subject got one score which represented the number of beliefs orienting towards achievement which he checked in that component. The correlations between the four scores are either insignificant or low ($r_{N\&GB} = 0{\cdot}31$, $r_{N\&Go} = 0{\cdot}34$, $P < 0{\cdot}05$). This finding supports the relative independence of the four components and suggests that they tap different aspects of cognitive contents.

Table 1. *Comparisons of performance on tasks under three experimental conditions by subjects classified according to CO scores*

(The reported significance levels of t values are one-tailed.)

Task	CO score	n	Index of performance: Mean	Index of performance: SD	Significance of differences
Neutral	(a) 4	3	133·66	27·58	$F = 10{\cdot}85$***
	(b) 3, 2	24	102·83	37·08	$t_{a,b} = 1{\cdot}34$, $P < 0{\cdot}10$
	(c) 1, 0	23	95·78	30·46	$t_{a,c} = 1{\cdot}97$*
Post-success	(a) 4	3	46·33	3·05	$F = 4{\cdot}91$*
	(b) 3, 2	23	32·84	8·77	$t_{a,b} = 2{\cdot}60$**
	(c) 1, 0	23	29·69	9·08	$t_{a,c} = 3{\cdot}10$**
Post-failure	(a) 4	3	41·66	4·04	$F = 3{\cdot}01$*
	(b) 3, 2	24	27·72	9·12	$t_{a,b} = 2{\cdot}58$**
	(c) 1, 0	23	27·45	10·95	$t_{a,c} = 2{\cdot}19$*

* $P < 0{\cdot}05$. ** $P < 0{\cdot}01$. *** $P < 0{\cdot}001$.

The assumption that all four components are necessary for predicting behaviour led to combining the four scores into one predictor which presumably reflects the *cognitive orientation cluster*. This predictor was defined as the number of components in which the subject's score lies above the group's median, so that its range is from a minimum of 0 to a maximum of 4. The predictor is correlated with each of the components (for all correlations $P < 0{\cdot}01$).

Table 1 presents the findings about the positive relation between the CO cluster referring to achievement and short-term achievement behaviour under the three experimental conditions. In order to be able to appreciate the significance of these findings, the two control analyses performed on the same data should be mentioned. The first control analysis was designed to check whether all four components are indeed necessary for predicting behaviour. The analysis consisted in comparing the performance of groups formed on the basis of CO clusters reflecting median splits in one component (four comparisons), in two components (six comparisons), and in three components (four comparisons) at a time. Out of these 42 comparisons (14 for each experimental condition) only one (N & GB for the neutral condition, $F = 4{\cdot}90$; d.f. $= 1{\cdot}46$; $P < 0{\cdot}05$) yielded significant findings, which can be attributed to chance. The second control analysis was designed to examine whether the predictive utility of the CO cluster is due to the fact that it is based on a quadripartition of the beliefs or merely to the fact that it reflects a large number of beliefs. The analysis consisted in

comparing the performance of groups formed on the basis of a median split in the sum of all beliefs pooled together, regardless of the distinctions among the four components. None of the three comparisons yielded significant results.

The described experimental procedure is based on several assumptions and raises some problems. First, the CO cluster, the major cognitive predictor in our studies, is not assessed directly but through a combination of the beliefs checked by subjects in the questionnaires. This procedure derives from our present hypothesis about the CO cluster. The CO cluster is assumed to be a product of the four CO components which combine according to the directionality inherent in the orientative values of the respective beliefs. Hence it is a structure composed of beliefs in multiple interrelations. Since interrelationships among beliefs, structural qualities, and effects of interaction are difficult to verbalize, the CO cluster eludes direct observation and possibly even awareness. Moreover, the CO cluster is assumed to be an *ad hoc* construction of very short duration so that the individual can hardly be expected to attend to it and communicate it. Accordingly, the CO cluster has at present the status of a hypothetical construct, whose function is to generate a *behavioural intent*, and whose conscious product – if any – could take the form of beliefs about self, e.g. 'In situation *X* I decided to do *Y*'. The closest experimental approximation for the CO cluster is some kind of combination of the four CO components. Indeed, the major evidence for CO clusters derives from the fact that in all our experiments up to date behaviour correlated with combinations of the four CO components but not with the overall sums of beliefs supporting the relevant behavioural alternative.

A second assumption underlying the described experimental procedure is that the degree to which a CO component contributes to a particular CO cluster is reflected in the number of beliefs in that component which support the relevant behavioural alternative. In the described experiment there were only two alternatives, but the method could be extended also to cases where more alternatives are available, as, for example, in the study which dealt with the relative frequency of the three defence mechanisms: rationalization, denial and projection (Kreitler & Kreitler, 1969). However, the described procedure may be revised if further studies confirm our hypothesis that beyond a certain optimal number of beliefs supporting any behavioural alternative, the addition of further beliefs may not change the dominance of the behavioural alternative. Another possibility which awaits experimental testing is that the relevant units within the components are not single beliefs but groupings of beliefs supporting the relevant behavioural alternative, whereby a grouping of beliefs is defined as all beliefs in the component which share at least one of the meaning values that have been found as relevant to the predicted behaviour.

The third assumption underlying our experimental procedure for the time being is that the four components contribute equally to the directing of behaviour. However, multiple regression analyses on the data of our experiments revealed that the relative weights of the four components change from one experimental situation to another. For example, GB seems to play a greater role with regard to achievement behaviours (Kreitler & Kreitler, 1970*a*) than with regard to defensive behaviours (Kreitler & Kreitler, 1969). Moreover, the assignment of weights to the CO components is related to a further problem posed through the interrelations among these components. Although the correlations between the components are generally low

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enough to support their distinctness (Kreitler & Kreitler, 1969, 1970*a*, 1971), factors such as young age (Kreitler & Kreitler, 1967) or an emotion-laden theme (Kreitler & Kreitler, 1971) tend to diminish the differentiation among some components. However, research findings are as yet too limited to allow even a tentative hypothesis about whether the distinctness of the components and their differential weights remain constant across types of behaviour, developmental stages or specific structures of CO clusters. This problem, like the ones raised above, can be resolved only on the basis of further research.

BEHAVIOURAL INTENT, PLANS AND OUTPUT

The foregoing discussion has clarified that the nature and function of a CO cluster may best be understood through its relation to beliefs, on the one hand, and to the behavioural intent, on the other hand. It has been hypothesized that an input which could not be adequately coped with on the reflex level is transmitted to the molar level and appears here in the form of one or more beliefs about the environment and oneself. By means of 'meaning generation' this input brings about the retrieval of further relevant beliefs about environment and norms as well as about oneself and goals. If the retrieved beliefs do not include goal(s), probably no action is undertaken beyond perception and possibly memory registration. If one or more goals are elicited, the four CO components undergo a clustering process which consists in testing the beliefs (or groupings of beliefs within the components) from an orientative viewpoint and combining them according to their implied directionality. The CO cluster is the product of this clustering process. If, however, the relevant beliefs have undergone clustering before, as may be the case with regard to recurrent and significant situations, the orientative implications of the beliefs and the interconnexions among the beliefs may be assumed to be stored in memory, and could be retrieved when necessary. Under these conditions the process of clustering is materially shortened or even largely dispensed with, so that a greater speed and even automation of reaction can be attained. Yet, regardless of whether the CO cluster is newly formed or retrieved from memory as largely preformed, its orientative effect results from the combination of the directionalities inherent in the four CO components, and becomes manifest in the form of a *behavioural intent*.

This whole process, which ends with the emergence of a behavioural intent, should be considered as a feedback loop, although in this as in other cases Fig. 1 illustrates for the sake of simplicity merely the sequential steps. However, sometimes two or even more behavioural intents emerge. This may be assumed to occur when two components support one behavioural alternative, e.g. approach, while the other two, equally strong, support another behavioural alternative, e.g. avoidance. Under these circumstances no orientative CO cluster and therefore no dominant behavioural intent can be formed. For the purpose of coping with the stimulus, feedback information of this constellation leads to a new sorting-out of beliefs according to the behavioural alternatives which they support – for example, into approach-oriented and avoidance-oriented – in a manner which enables the formation of two CO clusters and thus also of two competing behavioural intents. We suggest denoting this situation by the name '*intentional conflict*', in order to distinguish it from other conflict types to be described later.

At this point, it would be useful to recall the contention of Miller *et al.* (1960) that cognitive constellations, such as Tolman's 'cognitive map' (1948) or Lewin's (1935) vectorially derived intention, are not sufficient to account for the course of an action. This contention is supported by the findings of Leventhal *et al.* (1965), who showed that fear alone was not a sufficient cause for behavioural change, while fear coupled with a specific plan of action was.

Miller *et al.* suggest that every action is controlled by a plan, i.e. 'any hierarchical process in the organism that can control the order in which a sequence of operations is to be performed' (Miller *et al.*, 1960, p. 16). We regard the concept of 'plan' as useful in this context. However, while Miller *et al.* apply it for the explanation of the whole of behaviour, we adopt it in order to describe and explain a specific stage, i.e. the transformation of a behavioural intent into an overt act of behaviour.

It is plausible to assume that three kinds of *plans* or plan schemata are stored in memory: (*a*) physiologically determined plans, e.g. innate reflexes or the terminal stage of instinctive behaviour (Schiller, 1957; Tinbergen, 1951); (*b*) plans which are acquired through learning or formed in the course of personal experience, e.g. culturally determined or personally elaborated sets of instructions for the implementation of behavioural intents, e.g. habits and skills, plans such as, 'when you are hungry, then do x and y', rules of logical thinking, schemata for problem-solving, strategies for concept-formation, or the hypothesized plans of Miller *et al.* (1960) for the adaptation of stored plans to new situations, etc.; (*c*) plans which are determined both by physiological factors and learning, e.g. plans for the determination of perceptual constancy in Piaget's sense, defence mechanisms (Rapaport, 1957), conditioned reflexes, etc.

The execution of plans is obviously subject to feedback mechanisms in the course of performance. However, feedback controls of performance do not suffice to account for the smoothness of most behavioural acts in spite of the infinite variations among situations and the necessarily restricted number of different plans. Therefore a process of adaptation of plans prior to action must be assumed. We suggest calling this process '*planning*' (Fig. 1). It includes combination, fragmentation and other adjustments of plan(s) in view of both the behavioural intent and the demands of the current situation, and it continues until a viable plan is actualized.

In most cases each new behavioural act replaces a former behavioural act. However, since the formation of CO clusters is presumably a much faster process than are performance and feedback control of plans being executed, it is plausible to assume that intentions may arise and plans may be actualized during or before the completion of previously actualized plans. Two frequent procedures for dealing with such conflict situations between intents and performed plans are: (*a*) delaying the performance of the new plan until the completion of the present plan or activity (Feather, 1960); (*b*) interruption of the plan which is in the course of performance for the sake of the new plan (Mandler & Watson, 1966; Zeigarnik, 1927).

However, conflict situations of a more serious type arise when there are two plans which seem adequate for the implementation of one behavioural intent, or when two equally strong intents attain simultaneously the stage of 'planning'. The latter seems to be a necessary but not sufficient condition for the actualization of defence mechanisms.

When the plans available to subjects under given conditions are known, or are experimentally controlled by restricting behaviour alternatives to a given set, it is possible to predict on the basis of CO clusters which plan would reach the stage of performance. For example, in one study (Kreitler & Kreitler, 1969) CO clusters enabled the prediction of the relative frequency with which three available defence mechanisms, namely rationalization, denial and projection, would be manifested, whereas another experiment (Kreitler & Kreitler, 1971) demonstrated that CO clusters predict whether a subject would choose to go on working on a task similar to or different from a task in which he previously experienced success or failure.

The actualization of one or more plans becomes manifest in the form of behavioural *output*. In view of the functions of the model in prediction and explanation of behaviour, we group output behaviour into types according to the aspect dominant in each. Of the two common approaches to classifying behaviour, which reflect either an emphasis on the presumed psychological function dominant in the behaviour or an emphasis on features of the situation in which the behaviour occurs, we prefer the first as closer to psychological theorizing on intra- and inter-individual processes. Accordingly, the major types of output with which the model deals are motor behaviour, emotional behaviour, thinking, memory, attitudes, decisions about future actions, etc.

However, behaviour of each of the types mentioned may be regarded not only as output but also as a possible further input, and may thus contribute to the formation of CO clusters, plans, etc. Hence, in so far as the behavioural output serves as further input into the system, the model is self-generating. The interrelations between output and input and the problems created through these interrelations may be illustrated with regard to attitudes. Considered as output, attitudes may be predicted by the CO model as other behaviours are. However, unlike other behaviours, attitudes are manifested in the form of Beliefs about Self, particularly about the relation of the self to other people, objects, or situations. As such they are stored in memory and may take on the functions of beliefs which influence the formation of further CO clusters. Since, according to our current prediction technique, beliefs fulfil the role of independent variables, the attempt to predict which of a set of already formed attitudes would be expressed under given conditions leads to confounding the dependent and independent variables. As long as this problem is not solved, the CO model can be used only for the prediction of *ad hoc* newly formed attitudes.

IMPLICATIONS FOR MOTIVATION, THINKING AND PSYCHOPATHOLOGY

In view of a future cognitive theory of human behaviour, it seems useful to discuss at least some of the many problems raised by the proposed assumptions and CO model. The first problem bears upon the relation between our assumption of a primary tendency for the establishment of cognitive orientation and the various theories about exploratory behaviour, epistemological drives and particularly Jones' (1966) concept of an information drive. Jones (1966) has shown that information-seeking behaviour manifests all those features which serve as criteria for the identification of an information drive in the Hullian and Freudian senses. Like hunger and thirst, this drive follows the principles of homeostasis and of drive summation, is elicited by lack of

information, is reinforced by information, seems to be innate, and may even be distinguished from a need for stimulation since it is not reduced by merely changing stimuli devoid of information. However, it is evident that the assumption of exploratory, epistemological or information drives necessarily involves the assumption of a tendency to establish cognitive orientation, i.e. to turn representations of stimuli into meaningful representations. For the meaning of a representation must be established before it can be identified as new, or as conflicting with other representations or as having a certain information value.

More crucial than the above issue is the problem of the relations between CO and drives in general. Since the CO model is claimed to be a model of behaviour but deals only with cognitive processes, the wrong impression might arise that no role is attributed to drives and emotions.

The most prominent theories of drives can be classified into three groups according to which characteristic of drives they regard as central, i.e. internal stimulation, peripheral or central (e.g. Dollard & Miller, 1950; Harlow, 1953; Hull, 1951; Young, 1961), goal directedness (e.g. Lewin, 1935; McClelland, 1951) or the mode of performance, i.e. consummatory responses or instinctive behaviour sequences (e.g. Sheffield *et al.*, 1951; Tinbergen, 1951).

The CO model uses all the three variables mentioned and could thus contribute to all three hypotheses about drives. When considered only as reactions, e.g. to noxious inputs, proprioceptive activity is treated in the model as output. However, when it is considered as a link in a reaction-chain or as a spontaneous cyclical phenomenon, i.e. as both a stimulus and a reaction – which is the case in most physiological drive theories – it is treated in the model as input. Since interoceptive stimulation elicits orienting responses just like exteroceptive stimulation, we assume also that proprioceptive stimuli can become effective only through 'meaning action' or, if necessary, through 'meaning generation'. The findings of Schachter & Singer (1962), Schachter (1964, 1967) and Nisbett & Schachter (1966) clearly demonstrate the role of cognitive processes in determining the effects of internal stimulation on behavioural manifestations of drive and emotion. Further, the conception of goal-directed or goal-elicited behaviour is represented in the CO model by the belief component of 'Goals'. The innate action sequences which ethologists and others (Rapaport, 1957) regard as characteristic of drives appear in the CO model as innate plans.

Accordingly, we would propose using the concept of drive in order to designate the following two sequences of processes: (*a*) the representation(s) of proprioceptive activity, with or without the representation of a single stimulus, are subjected to 'meaning action' and 'meaning generation', through which a Goal is activated, for whose attainment there exist plans of a special kind. These are mainly innate plans, which may also be modifiable, to a certain degree, by learning, e.g. food-seeking behaviour, mating ritual, etc.; (*b*) the representation(s) of proprioceptive activity and of a signal stimulus, after being subjected to 'meaning action', effect directly, i.e. without the activation of beliefs, the elicitation of a special kind of plan. The second sequence would apply mainly to drive behaviours in infants and perhaps also in animals.

Similarly, we would propose using the term 'emotion' to designate the following two sequences of processes. (*a*) Representation(s) of proprioceptive activity, which may

have been elicited as reaction to a signal stimulus, or the former plus the representation of a signal stimulus, are subjected to 'meaning action' and 'meaning generation' through which Beliefs about Self are activated. These point in the direction of behaviour for whose performance there exist plans of a special kind, i.e. innate but not exclusively so. (*b*) The representation(s) of proprioceptive activity and of a signal stimulus, after being subjected to 'meaning action', effect directly the elicitation of a special kind of plan, without the prior activation of beliefs. Sequence (*a*) would describe emotional behaviour mainly in human adults (Schachter, 1964; Schachter & Singer, 1962), while sequence (*b*) describes better emotional manifestations in infants and the higher animals.

Whether Goal in the case of drives, or Beliefs about Self in the case of emotion, would produce a behavioural intent in the corresponding direction so that the adequate plan attains performance depends on the constellation of the CO cluster and its relations to another CO cluster which may have been formed at the same time. The suggested definitions of drive and emotion apply to the designation of the mentioned processes or aspects of these also when the corresponding plans do not attain performance. As in other behaviour domains, the following possibility has to be taken account of: a CO cluster, all of whose beliefs point in one direction, has been formed, but no adequate plans are available for the implementation of the behavioural intent.

The next problem to be discussed concerns the still only partly clarified relation of the CO model to specific thinking acts. The treatment of thinking as a special kind of output is not meant to suggest a fundamental distinction between thinking and other behaviours, but only to emphasize the distinction between cognitive processes in general and specific thinking processes and products, such as problem solving or concept formation, which are an integral part of those cognitive processes that direct behaviour. The distinction turns out to be purely pragmatic when one considers that 'output' is only the product or completion of an operational unit, and thus can be treated also as a new 'input', sequentially or even simultaneously.

On the basis of several pilot studies we venture the conclusion that the CO model can be applied successfully to the prediction of whether or not subjects will try under given circumstances to solve a problem, how long they will persist, and which in a series of problems they will choose. Prediction follows the same procedure as in the case of other behaviours, whereby the various thought processes are considered as plans or as given abilities, and are accordingly treated as constants. However, the application of the CO model to the study of thinking processes proper requires the hypothesis that the mentioned output, e.g. the decision to solve a certain problem, be turned into an input through its subjection to 'meaning generation'. Currently performed experiments on the meaning of specific problem-solving situations suggest that the elicited meaning values correspond to the four CO components. Goal Beliefs appear in the form of a conception about the presumable solution, similar to Selz's 'anticipatory schemata'; General Beliefs appear as methods or means for the solution of problems of this or similar types; Beliefs about Norms take the form of logical rules, which, according to Piaget, fulfil in thinking the role of moral rules in behaviour; and Beliefs about Self are manifested as attitudes towards certain solutions, methods and rules, many of which resemble aspects of cognitive style, such as levelling which is reflected in the belief 'I don't care for small differences'.

Grouping within the components, formation of a CO cluster, the resulting strategy and its implementation by a plan would be the further stages of the cognitive process whose product(s) could be predicted on the basis of beliefs.

It is notable that the suggested conception of the thinking process enables a tentative rapprochment between the various theories of thinking, by reformulating their main domains in terms of the CO model. For example, association theory would appear to deal mainly with the processes of recall and retrieval, which are crucial for 'meaning action' and 'meaning generation'. Gestalt psychology and studies on organization in memory (Mandler, 1967) and thinking appear to deal mainly with 'clustering' processes. The various computer models of thinking seem to concentrate on the processes characteristic of the operation of general beliefs and beliefs about rules as regulated by feedback effects of a set goal. Bruner *et al.* (1956) deal mainly with strategies and plans, and the approaches of cognitive control and cognitive style concentrate on some aspects of strategies but emphasize particularly plans. Finally, Freud (1933, p. 124) and his followers, who define thinking as a trial and error in fantasy for the purpose of satisfying drives, would consider beliefs about self, i.e. attitudes in terms of the model, and the translation of strategies into plans, as the basic features of thinking.

Finally, we turn to some psychopathological implications, extending those bound with the self-generating features of the model as found in the studies on schizophrenics (Kreitler & Kreitler, 1965). The CO model helps to formulate several hypotheses about constellations which could lead to abnormal development and symptoms. Some of these are: CO clusters which direct information-getting behaviours of a kind that provide only specific types of new information but not others; restricted 'meaning generation', for example, through the application of only a limited number of specific meaning dimensions; highly non-veridical beliefs which enter into CO clusters; deviant clustering such as occurs in schizophrenic contaminations or through overemphasis of beliefs of one component only; 'intentional conflicts' which consist in the simultaneous elicitation of antagonistic behavioural intents resulting from antagonistic CO clusters; conflicts formed through the generation of several apparently adequate but mutually exclusive plans for the implementation of one behavioural intent; use of pathogenic plans, e.g. projection or regression; and finally, formation of CO clusters which clash with plans in the course of performance or already performed ones.

Each of the above and other constellations could generate pathogenic effects, but need not necessarily do so. For example, post-decisional conflict (Festinger, 1964), which in terms of the CO model would be a contradiction between an already performed plan and a subsequently formed CO cluster, could produce cognitive dissonance only in those individuals or cultures whose beliefs about norms include a rule requiring consonance among beliefs or between behaviour and beliefs. But no attempt at dissonance reduction would be undertaken if, for instance, each of the dissonant elements is supported by strong groups of beliefs cherished in the culture (Kreitler & Kreitler, 1968). In more general terms, information about CO processes and formal constellations does not suffice for the understanding or prediction of behaviour or development, if the orientative contents of beliefs and their clustering are ignored. It is meaning and its orientative impact rather than merely 'meaning action' or 'meaning generation' which forms the nucleus of the CO model. The attempt to

reduce it through formalization to an explanatory or predictive schema while ignoring the content of the cognitive elements contradicts the spirit of the model.

Naturally, the concept of meaning should not be restricted only to that which is expressed or expressible in beliefs. We regard beliefs as the imprecise but so far the main manifestation of meaning values empirically accessible. The CO model in no way implies the assumption that all or most processes of cognitive orientation are conscious or even accessible to awareness, any more than it implies rationality or veridicality of beliefs or meaning values. The capacity of the individual to become aware of the contents and process of cognitive orientation is severely limited, on the one hand, by impaired or missing verbalization of meaning values and the speed of the process, and on the other hand, by the defence mechanism, namely plan of 'repression'. However, these restrictions upon awareness in no way restrict the fruitful application of the CO model to the investigation, prediction and, hopefully also, modification of behaviour.

This article was written while the first author was a USPHS Visiting Scholar at the Educational Testing Service, Personality and Social Behavior Research Group, and the second author was a Visiting Lecturer at Princeton University. The authors wish to express their gratitude to Dr Walter Emmerich and to Dr Nathan Kogan of the Educational Testing Service for their helpful comments.

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(*Manuscript received* 3 *November* 1969; *revised manuscript received* 16 *March* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 31–40
Printed in Great Britain

UNDERWATER DISTORTION: AND PLAIN DISTORTION

By JAMES G. TAYLOR*

In a paper on underwater distortion of size Ross *et al.* (1970) report results which they claim to be at variance with a theory they attribute to Taylor. The so-called theory is actually a deduction from Taylor's theory of perception and, when it is correctly stated, it is confirmed by the reported results. The use of objective methods for the study of perceptual adaptation is criticized, and it is argued that subjective reports give a more accurate account not only of perceptual adaptation but of the adaptation of the behavioural system that is disrupted by any transformation applied to the retinal input.

In an article on adaptation to size distortion under water (Ross *et al.*, 1970) the authors report an experiment designed to test a theory that they attribute to me, and claim that the results do not confirm that theory. I shall show: (*a*) that what they call 'Taylor's theory' is not an independent theory at all but a deduction from a larger theory; (*b*) that by neglecting the theory from which the deduction is drawn they have failed to understand the deduction itself; and (*c*) that when the deduction is correctly stated the reported results confirm it.

I begin by pointing out a small error. I am included among authors who have used terms such as 'situation-contingent', 'situation-specific' or 'situational' to refer to a type of adaptation 'in which a perceptual compensation appropriate to an optical distortion is immediately employed in the correct situation' (p. 371). I have never used such terms, and certainly not in the work (Taylor, 1962) cited by the authors.

The authors write: 'According to Taylor's theory, the subject learns to associate an optical distortion with certain cues, such as wearing prism spectacles or using a face-mask under water, and learns to apply an appropriate behavioural correction when these cues are present' (p. 371). This statement needs to be corrected in several ways to make it agree with anything I have said, and the most important correction is that what the authors are referring to is not an independent theory of situational adaptation but a deduction from a general theory of perception. The distinction is by no means trivial, for if a deduction is cited without reference to the theory from which it is derived, there is a serious danger that it will be misunderstood and misrepresented. My view is that perceptual adaptation can be understood only within the framework of a general theory of perception, and no doubt the majority of workers in this field would agree. But the trouble is that they seldom, if ever, reveal the basic theoretical concepts on which they rely. A serious consequence of this concealment is that their deductions from those concepts are not recognized as such and are elevated to the status of independent theories. What is worse, from my point of view, is that when I draw deductions from a theory of perception that has nothing in common with earlier theories, and clearly label them as such, they read my deductions as independent theories resting on the same axiomatic foundation as their own, and apparently do not consider it necessary to read and digest the theory from which they are derived.

To make the remaining corrections in the statement quoted above it is necessary to offer a brief statement of the behavioural theory of perception (Taylor, 1962) and

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some of the deductions drawn from it. The essence of the theory is that perception is a state of multiple simultaneous readiness for responses to the environment acquired in the course of the subject's life. What determines those responses is the properties of the environment that are relevant to the subject's life needs, and not the intrinsic character of the sensory input.

The relevant deductions are concerned with the effects of applying a temporally continuous transformation to the retinal input. The first is that initially there will be a change in the perceptual field, but that the character of this change is not necessarily entirely dependent on the optical properties of the transforming device. For example, if one looks through spectacles containing wedge prisms with their bases left, or right, one will report that both the horizontal and vertical dimensions of objects appear to be changed. Yet analysis of the path of a ray of light through the prism shows that the horizontal component of its direction is changed, but its vertical component remains unchanged (Taylor, 1966). The discrepancy is deducible from the behavioural theory.

A second deduction is that if the transformation is applied continuously to the retinal input, and if the subject can re-acquire all the appropriate responses to the environment, then normal perception will be restored. It can further be deduced that when the normal retinal input is restored, behaviour appropriate to the transformed input will tend to continue briefly, and perception will once more be distorted. Such perceptual distortions are commonly called after-effects and their existence has been frequently confirmed since Kohler (1951) first described them in detail.

The deduction that Ross *et al.* have mistaken for a theory was made near the beginning of an experiment in which Dr S. Papert wore spectacles that reverse right and left. The deduction was that if he wore the spectacles not continuously but only for half of each day, the duration of the behavioural after-effects, both of periods of wearing the spectacles and of periods of not wearing them, would suffer a progressive diminution to zero, and that consequently no perceptual change would occur when the spectacles were put on or removed, and no perceptual after-effect would be detected. If this prediction were confirmed, as in fact it was, it was evident that there must be some signal to cue in the appropriate behavioural system every time a change was effected. The most obvious signal was the pressure of the spectacles on the nose and other parts of the head; and that is the only one I mentioned. If I had thought it important to enumerate all the signals I would have pointed out that the visual field of the spectacles is very much narrower than the normal field, but I considered it enough to mention one signal that was certainly adequate for the purpose.

Ross *et al.* have not only mistaken a deduction for an independent theory; they have not even stated the deduction correctly. One of the statements they attribute to me is that 'the subject learns to associate an optical distortion with certain cues'. This seems to imply that the subject is aware of the distortion, in which case, of course, no additional cue is required to bring the appropriate behavioural system into action. In other words, what these authors are asserting is that I have postulated an entirely unnecessary mechanism. But in fact I said nothing about associating the optical distortion with the pressure cue. It would have been nonsensical to do so, because towards the end of the experiment Papert reported that there was no difference between the right–left ordering of the perceptual field with and without the spectacles. In other words, there was no perceived distortion to be associated with the pressure

signal. What I did say was that the presence or absence of pressure was a two-valued parameter that served to bring the appropriate system of behaviour into operation.

The authors go on to say that the subject 'learns to apply an appropriate behavioural correction when these cues are present'. This distorts what I said, in two ways. First, the behaviour evoked by the transformed input cannot be described as a 'correction'. At the beginning of the experiment responses are misdirected and have to be corrected, but at the end there is no trace of any tendency to move in the wrong direction; the responses are correct *ab initio*. Secondly, correct responses are conditioned not to the pressure cue but to the transformed retinal input. What is conditioned to the two values of the pressure parameter is two complete response systems which differ with respect to input but are identical with respect to output. It is absurd to say that one of these is a correction of the other, and still more absurd to say that the subject learns to associate the pressure cue with one of those identical behavioural outputs but not with the other.

The authors then state what they think I would have said about underwater distortion. 'If, for example, divers associate the distortion with underwater swimming, they should not show situational adaptation when looking into the diving tank through a porthole.' I would certainly never have made any such prediction. Immersion of the body in water can constitute one of the components of the two-valued parameter referred to above, but a more important component is the knowledge that the subject's eyes are in air, that the test object is in the diving tank and that there is an interface of glass between the air surrounding the eyes and the water containing the test object. This knowledge is available to the subject both when he is himself immersed and when he is looking into the tank through a porthole, and I would therefore predict that experienced divers would make virtually the same judgements of size of an immersed object in the two conditions. This is precisely what the authors found, but they interpreted their results as lending 'little support to Taylor's (1962) theory'.

At the end of their article the authors attempt to explain why experienced divers make reasonably accurate judgements of the size of immersed objects seen from outside the water. They think the 'correction' may be intellectual, since it occurs whether the diver is in or out of the water, but they do not specify the kind of intellectual process that would do the trick. So let us try to give some body to this hypothesis. We can rule out at once any process requiring knowledge of the laws of refraction and the use of a table of sines. If the subject knows in general that objects immersed in water are magnified, he may interpret the instruction to adjust the length of the variable line until he 'thinks' it is 12 in. by making it *appear* to be 12 in. long and then enlarging it by what he conceives to be a suitable factor. But as it does not require a long experience in diving to learn the appropriate factor of magnification, there is no reason to expect novice divers to be less successful in this exercise than experienced divers. Since they were in fact less successful, the authors should have rejected the hypothesis outright.

'Alternatively the correction may be perceptual, and may be switched on whenever the diver experiences "face-mask distortion", however the distortion arises' (p. 372). With some necessary changes in terminology, this is precisely the interpretation of

the events that I would offer. The first change requires the elimination of the word 'correction', since no corrective process is evident in the responses of trained subjects. Secondly, if perceptual errors have been eliminated, it is evident that a contradiction in terms is involved in saying that the distortion is 'experienced'. It is plainly nonsensical to say distortionless perception is triggered off by the perception of distortion, but it is not at all nonsensical to say that it is triggered off by some stimulus that unambiguously signals the fact that the test object is immersed in water while the eyes are in air, separated from the water by glass.

That the authors have fallen into a logical trap is revealed by their statement that: 'There is sufficient information available from small eye and head movements when outside the porthole to identify the nature of the distortion.' Eye movements alone do not reveal distortion unless the distorting device is incorporated in a scleral contact lens, and head movements reveal it only before the errors of perception have been corrected. When the subject has learned to respond correctly to the transformed retinal input, the perceptual field is no longer unstable under movements of the head, as Stratton (1897) reported long ago.

If Ross and her collaborators had understood my theory of perception, they might have attacked me on apparently much stronger grounds by referring to their experimental findings. One of the deductions from my theory is that normal perception will be fully restored if the subject has an opportunity to correct *all* the behavioural errors induced by the transforming device. But their results show that their divers (11 men with diving experience ranging from 1 to 15 years) saw an 11-in. test object as being 12 in. long whether observed through a face-mask or through a porthole. In other words, perceptual adaptation, as measured, is incomplete. This finding is repeated with monotonous regularity in practically all the adaptation studies, and if it is valid it constitutes powerful evidence against my theory. It is obvious, for example, that people with more than one year of diving experience have had ample opportunity to correct all the behavioural errors induced by underwater distortion and their underwater perception should therefore be fully corrected.

I maintain that it is in fact fully corrected and that the discrepancy arises from restrictions artificially imposed on the subjects for the purpose of measurement. I have shown elsewhere (Taylor, 1965) that the most powerful stimulus for controlling responses adapted to size and distance is the rate at which the visual angle subtended by an object increases as one moves towards the object. That is, for the purpose of perceiving size and distance the most important information is not the visual angle as a function of distance but the first derivative of that function. Whatever the size of the object may be, the derivative increases with positive acceleration as the distance decreases. Thus if three objects at distances of 100 m, 10 m and 1 m subtend visual angles of 10°, a decrease of 50 cm in the distances will increase the angles to 10° 3′, 10° 32′ and 19° 50′ respectively.

Now when psychologists undertake to measure perceived size or distance they invariably immobilize their subjects and many of them use bite-boards, under the mistaken impression that they are thereby eliminating possible sources of contamination. In fact, immobilization is the surest way to contaminate the results, since it cuts out the most important source of information. The rate of increase of the visual angle under water is slightly greater, with a higher acceleration, than it is in air, but a

more important fact is that the visual angle subtended by an object varies with rotation of the head, whether left–right or up–down.

This fact seems to have been consistently ignored by psychologists working in this field, and that is all the more surprising because the relevant physical principles are quite simple. When two substances having different refractive indices are separated by an interface, a ray of light crossing the interface is refracted, that is, it changes direction. In the present case the two substances are water and air. The refractive index of air is unity and that of water may be taken to be $n = \frac{4}{3}$. The change of direction is given by the difference between the angle of incidence, i (the angle between the incident ray and the normal to the interface at the point of incidence), and the angle of refraction, r (the angle between the emergent ray and the same normal). When the light passes from water to air, the angle of refraction is given by the equation

$$\sin r = n \sin i.$$

From this it is easy to calculate the visual angle subtended by an object that subtends an angle x at a point on the interface in front of the eye. If the object is straight ahead of the observer, the angles of incidence of rays from its two extremities are each $i = \frac{1}{2}x$. The required visual angle is then $2r$. A small correction is required to take account of the distance between the node of the eye and the interface, but this diminishes to a limiting value of zero as the distance of the object increases. I shall ignore it in my calculations.

The change in the visual angle that results from rotation of the head may be illustrated by a numerical example. In what follows I am assuming that the interface is a plane. If an object subtends an angle of 5° at the interface, the visual angle is 6° 40′ when the object is straight ahead of the diver. If the head is now rotated, carrying the face-mask with it, through 5°, 15°, 25°, 35° and 45°, the visual angle increases to 6° 41′, 6° 52′, 7° 18′, 8° 29′ and 15° 10′. Since the refracted ray is coplanar with the incident ray and the normal to the interface at the point of incidence, these changes occur only in the dimension of the object that is in the plane of rotation. The dimension at right angles to that remains invariant. We cannot, however, deduce the form of the perceptual distortion directly from those calculated values. Although I have never experienced underwater distortion through a face-mask, I would venture to deduce, from the behavioural theory of perception, that the expanded dimension, as perceived, is less extended than the figures would suggest, and that there is some diminution of the dimension that remains optically unchanged. Even when the object is straight ahead the perceived expansion is less than the optical expansion. In the first experiment reported by Ross novices judged a length of 9·6 in. at a distance of 3 ft. to be 12 in. The optical distortion would make it 12·9 in. However that may be, it is clear that the normal stability of the perceptual field under rotation of the head is destroyed. One of the most frequently reported results of experiments involving transformations of the retinal input is the gradual disappearance of perceptual instability under movements of the head and body, and there is no reason to expect that this does not occur in experienced divers. Hence it would seem that the simplest way to discover whether perceptual adaptation has taken place is to ask the divers whether the shapes and sizes of immersed objects change or remain invariant when the head is rotated.

Specialists in the field of perceptual adaptation will no doubt protest that this is a proposal to return to the primitive and unreliable method of introspection in place of the more sophisticated objective techniques that have been developed in recent years. I propose, however, to argue that subjective reports give more accurate information about perceptual changes than we can ever get from objective methods, provided we do not include among our subjects people whose integrity we have reason to suspect.

Suppose that Ross and her colleagues were to set up an experiment to investigate perceptual instability under water, using the apparatus they have described. They might make, say, five sets of measurements, with the subject's head directly facing the apparatus, and with the head rotated 15° and 30° to the right and to the left. They would undoubtedly select subjects with varying amounts of diving experience, from novices to experts. One might expect that in the case of the novices analysis of variance would reveal significant differences between the angles and that this would agree with subjective reports of instability under rotation of the head. One might also expect that in the case of the experts no significant differences would be found between the angles, and that this, too, would be in agreement with the subjective reports.

Let us assume now that there is a group of subjects with more than novitiate but less than expert experience, and that each of them has reported a slight residual instability at angles of 30° or more. If these are treated as a separate group for statistical purposes, it may be expected that the variability of the responses, both inter- and intra-subject, will have the effect that no significant differences between the angles will be revealed. If that should happen, the investigators, convinced as they are of the superiority of objective methods, would be constrained to conclude that there was no remaining instability and that the subjective reports were unreliable. Most investigators avoid this dilemma by the simple expedient of not asking for subjective reports, or of not reporting any that are spontaneously offered by their subjects, justifying the omission by the low scientific repute in which subjective reports are currently held. The dilemma, nevertheless, exists and it demands a resolution.

My answer is that the subjective report is much more reliable than any objective measurements can ever be, because it results from an incomparably more sophisticated statistical operation than any that can be applied to the objective data. The essence of the argument is to be found in my discussion of the autokinetic phenomenon (Taylor, 1962, 7·13 and 7·14, pp. 151 ff.). I had argued that the perceived position of an object reflects a state of readiness for a response such as reaching for the object, or aiming at it. But investigations of aiming behaviour reveal a very much higher range of variability than is to be found in measurements of perceptual thresholds. My answer to this is that if a faint light is exposed in a dark room, its perceived position is no less variable than an aiming response would be. The variability is substantially reduced if a second light is added, and quickly drops to threshold level as the number of lights is increased.

This decrease in instability is explainable in terms of the behavioural theory, which identifies perception with a state of multiple simultaneous readiness for responses to many features of the environment. It should be noted that multiplicity and simul-

taneity are essential ingredients of the theory. This means that readiness for responses to points A, B and C are not unrelated to one another, and in particular the potential errors of readiness for a response to A are subject to restriction by the simultaneous readiness for responses to B and C. The process is expounded at length in the sections referred to above, but here it will suffice to say that it involves the averaging not of the errors to which a single response is subject but of the random errors simultaneously affecting responses to a whole set of adjacent points. It is evident that a momentary process cannot average the errors to which a single response is subject over a period of time, but it can average errors occurring simultaneously. The effect of this averaging process is that the range of error to which any one readiness for response is subject is drastically reduced. Its standard deviation is probably equivalent to the standard error of the mean error of all the responses. Thus if a single act of perception involves readiness for n responses and if σ is the standard error of any one response, then the standard error of the perceived position of any object in the perceptual field would be $\sigma/\sqrt{n}$.

In comparison with this the so-called objective methods of measuring perception are crude and unreliable, because they necessarily depend on a single overt response repeated over a period of time, whereas the perceptual experience summarizes a large number of response tendencies at one moment of time. In addition to this they obscure the perceptual facts by lumping together measurements taken from a number of subjects. Perception is after all a private and personal phenomenon and it is to be expected that there will be significant differences between individuals. Such differences are inevitably hidden in an average drawn from a number of subjects.

In conclusion I would submit that to use objective methods for the study of perceptual adaptation is to put the cart before the horse. As a behaviourist I would maintain that the essence of adaptation to a transformed input is the restoration of behaviour accurately adjusted to the relevant properties of the environment. What is in question here, however, is not just a few isolated responses but a whole system of closely related responses. Ordinary methods of behavioural investigation are incapable of dealing with such systems, since they must of necessity be directed to one response at a time. But if my theory of perception is correct, it means that perceptual experience is a sensitive instrument that reveals the state of the system as a whole. On the other hand, the adaptation specialists use behavioural techniques not for the purpose of behavioural analysis, but to enable them to make statements about subjective phenomena which in any case they hold in low esteem. There is surely something radically wrong with this approach.

It is worth noting that the scientific respectability of subjective reports is currently being restored by ergonomists. In a study of the optimum heights of domestic working surfaces Saville (1969) recorded subjective reports of preferences and in addition used three objective techniques, viz. electromyography of postural muscle activity, photography of movements of body segments, and continuous recording, by means of a stability platform, of displacements of the centre of gravity of the subject's body. He found the subjective reports to be no less reliable than the objective measurements. In a study of the discrimination of the heights of chair seats Kirk *et al.* (1969) used no objective techniques at all, but the old psychophysical method of right and wrong cases. When the height of the chair was varied by steps of half an inch they

found a distressingly low incidence of 'wrong cases', and they could determine difference thresholds and points of subjective equality with the standard height only when the steps were reduced to ¼ in. Their conclusion was that subjective reports provided a reliable basis for this kind of ergonomic investigation.

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(*Manuscript received* 23 *November* 1970)

HELEN E. ROSS* and P. LENNIE† write:

Cues for perceptual adaptation

It was clearly foolhardy of us to attempt to summarize Taylor's sophisticated views in a passing paragraph; but we are glad to have given him the opportunity to explain some of them in more detail. We cannot, in a short space, discuss all the issues he raises, but we would like to answer a few points.

Taylor seems to say that the completely adapted diver responds to some cue other than sensory information about the nature of the distortion, and suggests that the cue is the knowledge that he is looking from air into water through a flat glass interface. How does the diver 'know' this? Suppose we attempt to trick him by using glycerine instead of water, or giving him a slightly curved face-plate. Will he respond appropriately for the usual situation or for the new one? He will probably notice immediately that something is wrong, as subjects do at the start of most adaptation experiments. Experiments on learning to switch from one type of distortion to the opposite have produced varied results. Ebenholtz (1967) and Lazar & van Laer (1968) failed to find either positive or negative transfer. McKay (1970) and unpublished results by Lennie (both of whom, unlike the previous authors, allowed time for after-effects to dissipate between sessions) showed some evidence for positive transfer – subjects learned to adapt quickly to the reversed distortion. Lennie, however, found that *on the first trial only* there was negative transfer – subjects initially perseverated in their previous (now inappropriate) responses. There may be cases,

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then, in which subjects respond out of habit to what they believe to be the nature of the distortion. Normally, however, the subject relies on veridical information about the distorting system, which he obtains in a variety of ways. The most obvious sources of information are the visual changes brought about by head and body movements, mistakes in eye–hand coordination, and changes in the apparent size and shape of familiar objects (such as one's own body). We cannot exclude the possible role of eye movements, or, more precisely, the effects of changes in eye position: as the eye moves behind a flat glass plate, the pupil accepts a slightly different bundle of rays from the same point of an object in water; as the angle of refraction varies non-linearly with the angle of incidence, there will be a change in the optical location of the point, giving a small deformation with eye movements, particularly when looking to the periphery of the face-mask. The distortion is gaze-contingent, in the same way as that produced by wedge prisms (see review by Epstein, 1967, ch. 9). (Incidentally, underwater psychologists are well aware of the increased distortion in oblique viewing. We mention it briefly in the paper which Taylor criticizes (Ross *et al.*, 1970, p. 365); Kinney *et al.* (1970) and Christianson (1968) mention it; and Ross (1970) reports adaptation to the curvature which it produces.)

Taylor seems overconcerned with the limiting case of the perfectly adapted subject who perceives no distortions and makes no behavioural errors, when switching between normal and distorted vision. He seems to have little to say about the partially adapted subject, who is frequently aware of distortions. It is doubtful whether cases of perfect and instantaneous adaptation occur, at least in diving. Taylor thinks this is due to their immobilization during tests; but it is not clear why this should be deleterious to experienced divers, if they are relying on 'knowledge of the situation'. In fact, for safety reasons, we never immobilize our subjects. In the present experiment they sat on a stool with their heads free (Ross *et al.*, 1970, p. 367). Changes in buoyancy when breathing make it impossible for a diver to sit still, and considerable head and body movements occur. Free-swimming divers also normally continue to report perceptual distortions, even after years of diving experience. Cases of complete and instantaneous adaptation *may* occur, but they are not common.

Models for perceptual adaptation should be able to account for both normal and distorted vision, and for all degrees of adaptation (e.g. Dodwell, 1967). It seems reasonable to assume that the brain always applies some kind of transformation to the retinal image, and checks whether any discrepancies arise from that transformation. If there are any discrepancies, the type or degree of transformation will normally be changed. Our results suggest that the experienced diver learns approximately what type of transformation to apply when looking into water, but does not get it completely right immediately. Further adaptation normally occurs rapidly during the first minute in water, and thereafter more slowly.

We cannot rule out the possibility of intellectual corrections for size estimates. Some subjects maintain that they do consciously correct, and there is no reason to disbelieve them. This could account for some of the difference between novice and experienced divers, because the 'appropriate factor of magnification' is not as simple as Taylor implies. It varies with the angular size of the object; with the distance of the object from the face-plate and of the eye behind the face-plate; with the apparent distance of the object; with the way in which the individual subject

trades off size-distortion against distance-distortion; and with the extent to which he has perceptually adapted (Ross, 1967; Ross *et al.*, 1970; Franklin *et al.*, 1970). It is conceivable that the experienced diver learns what type of error he normally makes in a given situation, and is thus better able to correct his judgements than a novice.

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(*Manuscript received* 15 *January* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 41–46
Printed in Great Britain

A CONFIRMATION OF THE INERTIAL-ψ EFFECT IN SEQUENTIAL CHOICE AND DECISION

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The experiment was designed to test the hypothesis, derived from earlier investigations, that people tend to overestimate compound probabilities, in the rough sense that they think they have a better chance of success than is actually the case. Fifty boys, aged 14+, were presented with various $m \times n$ arrays, where m represents the number of stages and n represents the number of alternatives at each stage. There was one correct but unknown alternative at each stage and, in order to win a prize, the subject had to guess them all in turn, only one guess being allowed at each stage. The task of the subject was then to equate what he believed was his chance of winning the prize with one of a set of lotteries. The chance of success in these lotteries ranged from 0·9 to 0·0001. The hypothesis was confirmed and, in addition, what seems to be a 'law' of sequential choice (or decision) seemed to emerge. The 'law' is such that if the number of stages in the array is held constant, the relative overestimation of the chance of guessing correctly at all stages varies directly with a power of the number of alternatives per stage. If, however, the number of alternatives is held constant, the relative overestimation varies exponentially with the number of stages. This 'law', which we call 'the inertial-ψ effect', appears in almost identical form in two independent experiments. It may prove to have a very general application in the characterization of human judgement and its fallibility in many spheres of life.

A recent experiment (Cohen *et al.*, 1971) has revealed what seems to be a new phenomenon in the way people deal with a chain of choices or decisions, when there is an element of chance in each link of the chain. The experiment may be illustrated as follows. Imagine an array which consists of rows of receptacles. One receptacle in each row contains a ticket. The subject's task is to assess his chance of guessing the location of the ticket in all the rows, and to match his assessment with one of a set of different lotteries. The outcome was a systematic overvaluation, by the subjects, of their chance of locating all the tickets. Furthermore, variation in the number of rows (with stages constant) produced a decidedly different effect from variation in the number of stages (with rows constant).

The experimental situation may be placed in a broader setting if we conceive of the rows as 'stages' and of the receptacles as 'alternatives' per stage, thus representing a common type of sequential choice or decision which may occur in daily or professional life. The general case allows for variation of a stochastic character in the array, in that the number of alternatives, and therefore the probability of a correct choice, need not be the same at each stage.

The particular problem which the experiment attempted to resolve arose in the context of studies of visual search (Cohen & Cooper, 1962) from which it appears that items in a display which to the experimenter are equiprobable are not necessarily equiprobable to the subject. Thus if subjects are presented with a page on which 2^{10} small circles are irregularly scattered, and their task is to locate a target circle, 50 per cent believe the target is among the 20 per cent of the circles clustering near the centre of the page. Again, in a $k \times k$ array (e.g. 20×20) of identical squares, some 60 per cent of nearly 1000 subjects located the target square in the top left quadrant. A similar non-equiprobable effect appears in a series of concentric circles with the

target to be located in one of the inter-circle zones, which all have the same area (Cohen *et al.*, 1969). From these investigations we predict that, in the kind of sequential choice situation described at the beginning of this section, the subject's estimate of his chance of guessing correctly at all stages of the array would be exaggerated, as judged by the compound mathematical probability of a correct guess at all stages.

In view of the implied claim as to the stability of the 'relative overestimation', we felt it was necessary to repeat the experiment. We have done this, and employed a somewhat modified procedure. We have also extended the range of the inquiry by using a method of direct choice, in contrast to an indirect one with a lottery as intermediary.

METHOD

Apparatus and procedure. The apparatus consisted of a board on which were set m rows, each containing n receptacles, where m and n each took the values 2, 3, 4, 5 or 8. All the receptacles were empty apart from one in each row which contained a ticket. The number of separate stages thus ranged from 2 to 8, with the chance of guessing correctly at each stage ranging from 2^{-1} to 2^{-3}. The compound probability of guessing correctly at all stages is $p = n^{-m}$.

The subjects were clearly given to understand that in order to win a prize they had to guess, in turn, which receptacle in each row contained a ticket, only one guess per row being permitted. Their task was to equate what they believed to be the chance of winning the prize with one of a set of lotteries.

Table 1. *Choice of lotteries*

Lottery	No. of tickets	Probability of winning
1	11	0·91
2	12	0·83
3	14	0·71
4	16	0·63
5	20	0·50
6	25	0·40
7	33	0·30
8	50	0·20
9	10^2	0·10
10	10^3	0·01
11	5×10^3	0·002
12	10^4	0·001
13	5×10^4	0·0002
14	10^5	0·0001

There were 14 lotteries in all, with 10 winning tickets in each and with the probability of winning ranging from 0·91 to 0·0001, as shown in Table 1. In the previous experiment, referred to in the introduction, the number of lotteries and the range of probabilities were the same as in the present experiment, except that 10 of the lotteries had 100 tickets in each and the remaining four had 500, 1000, 5000 and 10,000 respectively; in lotteries 1 to 4, the chances of success were precisely 0·9, 0·8, 0·7 and 0·6 respectively.

The subjects were 50 grammar-school boys aged 14–15 years, whose 'general intelligence' may be assumed to be at least that of the average adult.

The number (5) of values taken by the rows, and the number (5) taken by the receptacles in each row, allowed for 25 different situations. Each of the 50 subjects was assigned to 5 of these situations by means of randomized Latin squares. Each $m \times n$ situation was thus assessed by 10 subjects.

The amount of information necessary to locate the target is $m \log_2 n$ 'bits', where m is the number of rows and n the number of receptacles. The subject's choice of lottery will be treated as an indirect estimate of his psychological probability (ψ) of guessing correctly at all stages of an array, and therefore of winning a prize.

Results

The estimated chances (in terms of choice of lottery) of guessing correctly at all stages of a given array are shown in Table 2. The first point to note is that all values of ψ in Table 2 are overestimates, as judged by the corresponding compound probabilities p ($= n^{-m}$). Our prediction, and the results of the previous experiment, are thus confirmed. The magnitude of the relative overestimation is shown in Table 3 by the ratios, ψ/p, of psychological to compound probabilities.

Table 2. *Psychological probabilities (ψ) based on choice of lottery*

(Means of 10 observations.)

No. (n) of alternatives per stage	No. (m) of stages in array					
	2	3	4	5	8	Mean
2	0·37	0·40	0·39	0·39	0·43	0·40
3	0·37	0·23	0·32	0·26	0·13	0·26
4	0·19	0·26	0·29	0·24	0·12	0·22
5	0·31	0·16	0·19	0·09	0·28	0·21
8	0·10	0·16	0·08	0·13	0·04	0·10
Mean	0·27	0·24	0·26	0·23	0·21	

Table 3. *Ratios, ψ/p, of psychological to compound probabilities*

No. of alternatives per stage	No. of stages in array				
	2	3	4	5	8
2	1·5	3·2	6·2	12·6	108
3	3·4	6·4	26·7	65·0	929
4	3·0	16·3	72·5	240·0	8×10^3
5	7·8	20·0	95·0	281·0	1×10^5
8	6·3	80·0	333·0	4×10^3	7×10^5

There is a decline in the values in Table 2 as we move from the first row to the last. The subjects therefore (so we infer) are aware that the more alternatives per stage, the smaller their chance of guessing correctly. However, they take much less account of stages than of alternatives per stage. As an analysis of variance reveals, the inter-alternative variance (but not the inter-stage variance) is significant ($P < 0{\cdot}01$) by comparison with the residual variance.

In Fig. 1 the logarithm of relative overestimation, $\log_{10} \psi/p$, is plotted against the amount of information, $m \log_2 n$ 'bits', required to choose the correct alternative at all stages. The relation is linear and is given by the equation:

$$\log_{10} \psi/p = 0{\cdot}26m \log_2 n - 0{\cdot}33, \tag{1}$$

which may be rewritten as:

$$\psi/p = \exp\,(0{\cdot}86m \log_e n - 0{\cdot}76), \tag{2}$$

from which we infer that if the number of stages is held constant, ψ/p is directly proportional to a power of the number of alternatives per stage. If, however, the number of alternatives is held constant, ψ/p varies exponentially with the number

of stages. This is the basic outcome of the experiment. Eqns. (1) and (2) are almost identical with those arrived at in the original investigation.

The inference may also be made – if it were not already self-evident – that a grasp of the multiplicative character of a compound probability is far from being in any sense a 'primitive' property of mental processes in relation to the external world.

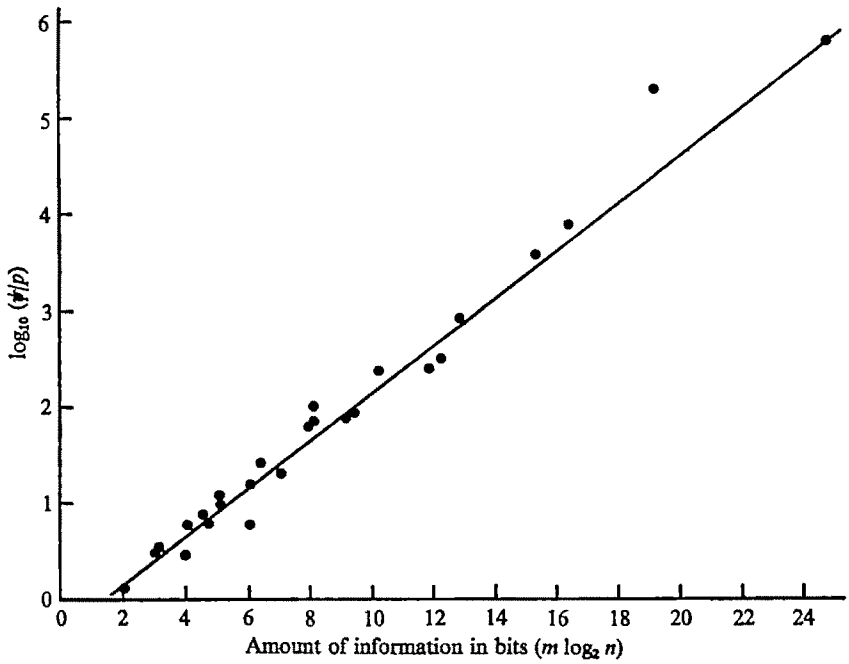

Fig. 1. Log relative overestimation plotted against amount of information in bits.

This may go to explain why many people, otherwise competent, experience particular difficulties in the study of statistics. If there is any intuition of this multiplicative element, it only exists in an attenuated form. The attenuation is of the order of six-sevenths of the number of stages, within our experimental limits.

Replacing p by n^{-m} in eqn. (2) gives

$$\psi = e^{-0\cdot76}\, n^{-0\cdot14m}, \qquad (3)$$
$$e^{-0\cdot76} = 0\cdot47,$$

from which we can predict the mean value of ψ, given m and n.

Direct evaluation

The method described above is an indirect evaluation of ψ. After making this indirect assessment, we made a more direct assessment by offering each subject a choice between two alternative arrays, as shown in Table 4. The subjects for this part of the experiment did not have to evaluate the same arrays as they had in the earlier part.

The preference, in all three cases in Table 4, for the array with the smaller compound probability of success is consistent with the corresponding indirect evaluations

in Table 2. Furthermore, again as in Table 2, the preference is most marked in the first of the three choices and least marked in the third choice.

Table 4. *Number of subjects favouring alternatives I and II respectively*

I			II		No. choosing	
m	*n*	*v.*	*m*	*n*	I	II
8	2		2	8	11	6
5	3		3	5	10	7
5	4		4	5	9	7

Twenty subjects were then presented with the choice shown in Fig. 2(*a*), and another 20 with the choice shown in Fig. 2(*b*).

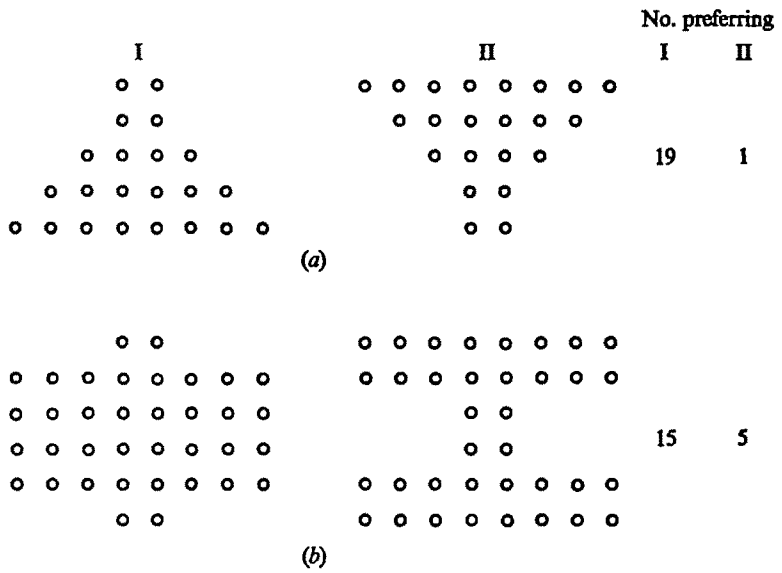

Fig. 2. Equiprobable pairs of arrays, and number of subjects preferring each. (The top rows in Figs. 2(*a*) and 2(*b*) are the rows at which the subject begins.)

In each choice alternatives I and II are equiprobable, in the sense that the chance of guessing correctly is the same in both situations. In so far as a belief in luck plays a part, this belief might be more decisive in I than in II, for one needs less luck, so to speak, to begin the former than the latter. Also, inasmuch as the situations have a game-like character, the utility of play in I exceeds that of II, because of the greater likelihood of remaining in the 'game' for a longer period. For the same reasons it is easy to understand why the pyramidal array (I in Fig. 2*a*) should be preferred to the cross-shaped (I in Fig. 2*b*), as may be inferred from the numbers choosing each one.

Discussion

The results of the experiments prompt several reflexions. Let us assume that our experiment simulates a variety of situations in daily life, as in administrative, medical, industrial, military and other professional contexts, where an individual has to make a number of consecutive choices or decisions, with an element of chance

entering at each stage. We may expect the decision-maker to err in his strategy or judgement by the overvaluation of his chance of making a correct choice at every stage. In general, the more stages, the greater the error of judgement.

Secondly, it is perhaps easier to understand the difficulties which many people experience in the grasp of statistical concepts which involve compound probabilities. In view of the fact that highly intelligent people, even those with some training in statistics, are liable to gross overestimation, it follows that the idea of compound probability is far from being 'primitive' or intuitive.

In accord with this view is the fact that the subtlest intellects in ancient Greece, greatly intrigued as they were by the idea of the Possible, especially in Stoic philosophy, never grasped the elements of combinatorial analysis, and this in spite of their understanding of disjunctive propositions of the type 'Either A or B or C...is true, and then arguing from the falsity of B, C...to the truth of A (Sambursky, 1956). Even Aristotle evidently had only the simplest notion of probability, and Cicero (*De Div*, XIII) who was born three centuries later, flatly denied that, if 400 dice were thrown, it would be possible 'by any chance in the world for them all to come up the face six'. The first developments in combinatorial analysis had to await Tartaglia and his contemporaries in the 16th century.

To conclude then: the phenomenon which our experiments appear to have identified, and which we have designated the 'inertial-ψ effect', may turn out to be a characteristic feature of human judgement, while pin-pointing one of its Achilles heels.

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(*Manuscript received* 5 *March* 1971)

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Printed in Great Britain

PEAK SHIFT, BEHAVIOURAL CONTRAST AND STIMULUS GENERALIZATION AS RELATED TO PERSONALITY AND DEVELOPMENT IN CHILDREN

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Fourteen children, aged 7–11 years, were selected from an initial sample of 190 to give a range of scores on the JEPI. They were also evaluated by a Teacher's Rating Scale for neuroticism. The children were trained in a discrimination learning task which yielded measures of peak shift, behavioural contrast and stimulus generalization. Peak shift and behavioural contrast were positively correlated. Neuroticism was positively correlated with both one measure of peak shift and stimulus generalization. Extraversion was negatively correlated with a second measure of peak shift and positively correlated with generalization. With increasing developmental maturity (chronological age, reading age and relative advance of reading age), both peak shift and behavioural contrast tended to be reduced.

Nicholson & Gray (1971), using an operant discrimination learning procedure, have demonstrated the phenomena of behavioural contrast (Reynolds, 1961) and peak shift (Hanson, 1959) in normal children. These phenomena have been treated as arising from the emotional consequences of frustrative non-reward (Terrace, 1966), and Gray & Smith (1969) have proposed an explicit mathematical treatment of them along these lines. This interpretation suggests that they may be used as measures of individual differences in susceptibility to frustration (Gray, 1972). Susceptibility to frustration might be expected to vary with (1) the general organization of personality and (2) age and developmental characteristics. With respect to the former, Gray (1970) has suggested that susceptibility to frustration should be related to the personality dimensions (Eysenck, 1967) of neuroticism (N) and introversion–extraversion (E), being greater with increasing N and with decreasing E. With respect to the latter, evidence reviewed by Lawson (1965) suggests that susceptibility to frustration decreases with age.

In the present experiment, then, we sought to relate these phenomena – peak shift and behavioural contrast – to scores on the Junior Eysenck Personality Inventory (Eysenck, 1965) and on a specially constructed Teacher's Rating Scale; and also to chronological age (CA) and reading age (RA). Peak shift was studied in more detail than behavioural contrast, because it yields scores on a definable physical scale, i.e. the stimulus dimension which is used for discrimination training and subsequent generalization testing, whereas the use of behavioural contrast as a measure of individual differences raises problems of correct scaling. The specific predictions tested (based on the assumption that peak shift and behavioural contrast increase in magnitude as susceptibility to frustration gets greater) were that both peak shift and behavioural contrast will increase (1) with increasing neuroticism; (2) with decreasing extraversion; (3) with decreasing developmental maturity. The detailed theoretical background to the first two of these predictions is set out by Gray (1970, 1972).

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The present experiment also had two subsidiary aims. The first was to confirm that behavioural contrast and peak shift can be found in children when a longer pre-discrimination training period is used than the one involved in our earlier experiment (Nicholson & Gray, 1971). The second was to see whether minor variation in the type of reinforcement employed (tokens, sweets, or tokens which could be exchanged for sweets) affected the results obtained.

METHOD

Subjects

Subjects were selected from an initial sample of 190 boys and girls at the Edward Field County Primary School, Kidlington, Oxfordshire: this is a large school, and the children there represent a wide range of backgrounds and abilities. The initial sample completed the Junior Eysenck Personality Inventory (JEPI: Eysenck, 1965), obtaining a mean N score of 14·43 (S.D. = 4·81) and a mean E score of 17·01 (S.D. = 3·65). Twenty subjects (10 boys and 10 girls) were selected so as to give a wide dispersion of scores along both the neuroticism (range of N scores, 6–23) and the extraversion (E scores, 8–23) dimensions. Test–retest reliabilities were obtained from a second testing of 43 of the initial sample (including the 20 experimental subjects) 6 months later, at the end of the experiment, and were found to be 0·64 for N and 0·77 for E. The ages of the sample chosen for the experiment ranged from 7 years 11 months to 11 years 5 months; reading ages on the Schonell Attainment and Diagnostic Tests of Reading English (Schonell & Schonell, 1960), which were administered by the school during the period of the experiment, were also available.

In addition, a specially constructed rating scale was filled in by the children's teachers. This Teacher's Rating Scale was made up of 12 adjectives, chosen from those used by Eysenck & Rachman (1965) to describe neuroticism, on which the teacher was asked to rate the child on a five-point scale, from 'not at all' to 'very'. The adjectives chosen were balanced between neurotic introvert traits and neurotic extravert traits and between the stable and unstable poles of the neuroticism dimension. The traits assessed, all expressed here in the neurotic direction, were: aggressive, unstable, restless, rigid, anxious, impulsive, touchy, moody, unsociable, pessimistic, excitable and unselfconfident. The test–retest reliability of this scale, obtained from a different sample of 70 pairs of judgements of six different teachers on 34 children (Nicholson, unpublished) is 0·81. We shall refer to the score obtained from this scale as TRS-N.

Of the 20 subjects in the experimental sample, three failed to complete the experiment: one left the school and two others failed to learn the discrimination. Thus complete data were available for 17 subjects.

Apparatus

The apparatus has been fully described by Nicholson & Gray (1971). Essentially it consisted of an operant conditioning cabinet containing a lever, pressing of which sometimes resulted in the delivery of a reinforcement into a tray, and a screen on to which discriminative stimuli (a drawing of a space rocket at various angles of orientation) could be projected.

Procedure

Shaping and pre-discrimination training were essentially as described by Nicholson & Gray (1971). By the end of this phase of the experiment, during the daily 30 min. experimental session all subjects were responding regularly on a VI 1 min. schedule in the presence of the stimulus subsequently used as the positive discriminative stimulus (S^D), i.e. the rocket orientated at 32·5° to the right of vertical. Care was taken to ensure that asymptotic response rate had been reached (i.e. response rate did not increase from one session to the next) before commencing discrimination training.

The 17 subjects were divided into two groups. Group 1 (n = 11) were given one pre-discrimination generalization test after asymptotic response rate had been reached. Group 2 (n = 6) were given two pre-discrimination generalization tests, one after three training sessions, the procedure used by Nicholson & Gray (1971), and a second after reaching asymptotic response rate. For both groups, asymptotic response rate was reached in three to nine sessions.

In the pre-discrimination generalization tests, 12 test stimuli were used, ranging from 18·5° to 161·5° in 13° steps (including the S^D). These were presented three times each in random order over one experimental session, and were interspersed with 24 additional presentations of S^D; only during the latter was the VI 1 min. schedule in operation. Each stimulus presentation lasted 30 sec.

The next phase of the experiment consisted of intradimensional discrimination training between S^D and S^Δ (the rocket orientated at 32·5° to the left of vertical). VI 1 mm. continued to operate in S^D; no rewards were delivered during S^Δ. There were an equal number of 30-sec. presentations of S^D and S^Δ, randomly intermixed, in each session. The number of training sessions varied from two to nine for different subjects. By this time, discrimination ratios (i.e. the ratio of response rate in S^D to that in S^Δ: Dinsmoor, 1951) ranged from 28:1 to 3:1, median 13:1.

The final phase of the experiment consisted of a post-discrimination generalization test, conducted over two sessions. The same 12 test stimuli (including S^D and S^Δ) as in the pre-discrimination generalization test were presented seven times each in random order, without reinforcement, and interspersed with 18 additional presentations of S^D (with VI 1 min. in operation) and 18 additional presentations of S^Δ without reinforcement. Stimulus presentations again lasted 30 sec.

For all of group 2 and five subjects in group 1, the reinforcement consisted of poker chips which were simply returned to the experimenter at the end of the session. Three subjects in group 1 received a sweet in the place of every seventh poker chip, and the other three subjects in this group were allowed to exchange their poker chips for sweets (one for every seven chips) at the end of each session.

Results

Although data were available for 17 subjects, preliminary analysis indicated that three subjects (two from group 1, one from group 2) constituted exceptions to several strong relationships that appear to exist between variables for the other 14 subjects. It seemed that inclusion of these three subjects in our analysis of the results would obscure rather than uncover the relationships they demonstrated, so that the main Results section is based on the data for 14 rather than 17 subjects. An appendix at the end of the Results describes the ways in which these three subjects stand out, and explains our reasons for excluding them.

Examination of the results revealed no systematic effect on any measure of variation in the mode of reinforcement. This variable will therefore be ignored.

1. *Peak shift*

Examination of the data on peak shift failed to reveal any systematic differences between groups 1 and 2. In this section of the results, therefore, we deal with all 14 subjects together.

Only one of these subjects showed a peak shift as classically described (e.g. Hanson, 1959), i.e. a shift in the *mode* of the post-discrimination stimulus generalization curve about S^D. But Terrace (1966), observing that only 50 per cent of Hanson's (1959) subjects displayed this mode shift, has pointed out that the distribution of area above and below S^D on the stimulus continuum may prove to be a more sensitive measure of the effects of discrimination training than the location of the peak. All Hanson's subjects showed these 'area shifts'. Grusec (1968) describes a method of treating data, in which the generalization curve is treated as a frequency distribution and the mean of this distribution is calculated in units of the stimulus continuum to produce a 'mean shift' (Nicholson & Gray, 1971) from S^D. Applying this technique to the present data, we considered response rates at S^D and at the three stimulus values on either side of it. After discrimination training, all 14 subjects showed mean shifts away from S^D

in the direction opposite to S^{Δ}. The mean Grusec value taken over the group of 14 for the pre-discrimination generalization curve (taking the final pre-discrimination test in the case of group 2) was $-0{\cdot}5°$ (S.D. $1{\cdot}5°$), this slight displacement being in the direction of S^{Δ}. After discrimination training, the corresponding mean Grusec value was $+3{\cdot}9°$ (S.D. $2{\cdot}4°$), the displacement now being in the direction opposite to S^{Δ}. The difference of $4{\cdot}4°$ between pre- and post-discrimination tests is significant at the 0·001 level ($t = 5{\cdot}29$, with 12 d.f.).

It could be argued, however, that this mean shift measure does not reflect the same processes as Hanson's (1959) original peak shift measure, since a mean shift can be produced by a depression of responding at stimuli located between S^{D} and S^{Δ} as well as by an elevation of responding beyond S^{D} on the opposite side of the stimulus continuum from S^{Δ}, whereas the peak shift is unaffected by the former of these two possible effects of discrimination training. For this reason, it was decided to examine the relationships between the mean shift, calculated as above, and measures of responding (*a*) at the three stimulus values beyond S^{D} in the direction opposite to S^{Δ}, i.e. the stimuli at which an elevation of responding would contribute to a classical peak shift; and (*b*) at the three stimuli located between S^{D} and S^{Δ}. To eliminate the effects of individual children's absolute response rates on these measures, response rates at each of these six stimuli were expressed as a simple proportion of response rate at S^{D}, separately for each child, and summed over the two sets of three stimuli involved in (*a*) and (*b*) above.

Table 1. *Product-moment correlations between behavioural measures* ($n = 14$)

(With 12 d.f., $P < 0{\cdot}05$ for $r = 0{\cdot}458$ and $P < 0{\cdot}005$ for $r = 0{\cdot}661$.)

	Mean shift 1	$S^{D}-S^{\Delta}$, average response rate 2	Beyond-S^{D}, average response rate 3	Generalization 4
1	—	−0.22	+0·26	+0·14
2	−0·22	—	+0·87	+0·86
3	+0·26	+0·87	—	+0·90
4	+0·14	+0·86	+0·90	—

It can be seen from Table 1 that, as would be expected, the mean shift is greater, the lower the response rate between S^{Δ} and S^{D} and the higher the response rate beyond S^{D}. However, these relationships are weak, and fail to reach statistical significance. Furthermore, the correlation between mean shift and response rate between S^{Δ} and S^{D} is not higher than that between mean shift and response rate beyond S^{D}, so that it seems reasonable to conclude that the mean shift is not primarily a measure of the depressant effects of exposure to S^{Δ} on responding in the presence of stimuli close to S^{Δ}. It seems probable, therefore, that the mean shift measure reflects essentially the same phenomenon as Hanson's (1959) original mode shift. It is, however, possible that the classical mode shift is most closely related to the response rate in the area beyond S^{Δ}, since this is the area in which mode shift is observed: we shall return to this possibility in the Discussion.

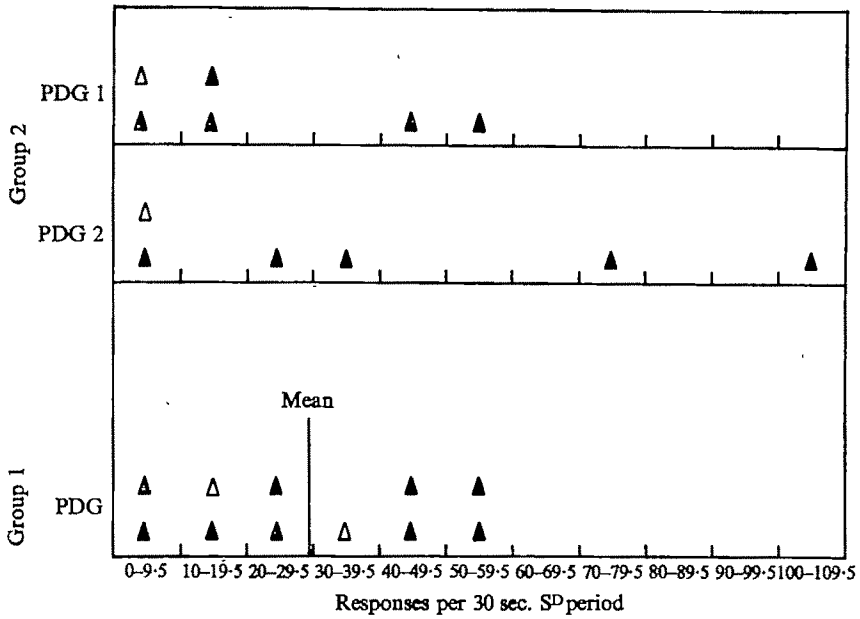

Fig. 1. Distribution of S^D response rates during pre-discrimination generalization (PDG) testing. The distribution of group 2's first PDG is indistinguishable from that of the group 1, whereas the distribution of group 2's second PDG contains two subjects whose response rates exceed the group 1 mean by more than two standard deviations. The open triangles represent the three excluded subjects (see Appendix).

It can also be seen from Table 1 that the correlation between the response rates on either side of S^D is exceptionally high: $r = +0{\cdot}87$ ($P < 0{\cdot}001$). This indicates that the tendency to generalize responding to stimuli similar to S^D is a reliable characteristic of a given child. By summing the two measures of responding involved – the S^Δ-to-S^D response rate and the beyond-S^D response rate (both expressed as proportions of S^D response rate) – we can obtain a measure of this tendency towards stimulus generalization (the fourth column in Table 1). It can be seen from Table 1 that this tendency is essentially independent of the mean shift.

2. *Behavioural contrast*

We deal first with group 1. In the case of seven of these nine subjects, response rate during S^D rose from the pre- to the post-discrimination test. The individual increases in response rate ranged from 6 to 226 per cent of the pre-discrimination base-line, and the individual decreases were 35 and 39 per cent. Taking all nine subjects together, there was a mean increase of 10 responses per 30-sec. period (i.e. an average increase of 49 per cent).

Turning to group 2, we may compare the subjects' post-discrimination S^D response rate with S^D rate recorded in either the first or the second pre-discrimination generalization test. By the first of these procedures, four of the five subjects show a post-discrimination response rate increase, whereas by the latter all five subjects show a response rate *decrease*. At first sight, this would appear to indicate that the response rate increase demonstrated by the first procedure is merely an artifact of increased

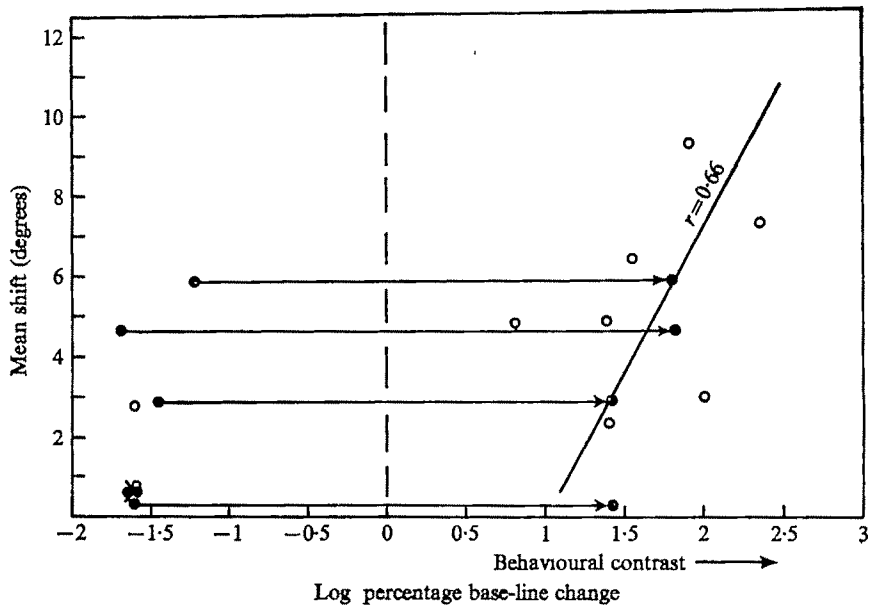

Fig. 2. Scattergram of the relationship between mean shift (in degrees) and behavioural contrast (log percentage base-line change). Group 1 subjects shown by open circles (○), group 2 subjects by closed circles (●). The arrows indicate changes in contrast as based on each of the two pre-discrimination generalization (PDG) sessions, arrows pointing towards contrast based on PDG 1. Although all 14 subjects are represented, the correlation coefficient of $r = +0{\cdot}66$ between behavioural contrast as measured from PDG 1 and mean shift is calculated only for those 10 subjects judged to have displayed contrast (see text).

exposure to the situation, as suggested by Russell (1960). But two features of the data militate strongly against this conclusion.

In the first place, as shown in Fig. 1, the distribution of S^D response rates in group 2 after three sessions is indistinguishable from the response rates in group 1 after the five to eight sessions required to reach the criterion of asymptotic responding (see Method). The distribution of group 2 S^D response rates in the *second* pre-discrimination generalization test, in contrast, contains two subjects whose response rates fall outside the group 1 distribution, being more than two standard deviations above the mean of the latter. This suggests that the first pre-discrimination generalization test itself resulted in an elevation of response rates in group 2. It seems possible that the non-rewarded probe components of the generalization test were responsible for the elevation and that the rise in response rate between pre-discrimination generalization tests 1 and 2 in this group was in fact a form of behavioural contrast.

The second feature of this data which suggests that group 2, like group 1, shows a true contrast effect emerges when we consider the relationship between behavioural contrast and peak shift.

3. *Peak shift and behavioural contrast*

Fig. 2 plots the relationship between the mean shift and a measure of behavioural contrast, the latter calculated as the logarithm of the percentage change from the pre-discrimination base-line. Subjects from group 2 are represented by two points, one

showing the results of the calculation of behavioural contrast when the data from the first pre-discrimination generalization test were used, the other showing the corresponding value when the data from the second pre-discrimination generalization test were used. It is clear from Fig. 2 that, if the data from the first pre-discrimination generalization test are used, a strong relationship exists between the extent of behavioural contrast and the magnitude of the mean shift in all subjects who showed behavioural contrast, with the exception of one subject from group 1 whose rate increase was only 6 per cent, much less than the increases seen in other members of this group. Subjects from the two groups are affected equally by this relationship. But if the data for group 2 are calculated from the final pre-discrimination generalization test, the relationship between behavioural contrast and peak shift is destroyed. It seems evident, therefore, that the correct base-line from which to calculate behavioural contrast in group 2 is the first pre-discrimination generalization test. It is also probable that the subject in group 1 who showed only a 6 per cent rate increase was not displaying a contrast phenomenon, but merely a chance increase in responding. If, on these assumptions, we calculate the correlation between log behavioural contrast and peak shift for the 10 subjects showing behavioural contrast, irrespective of group and using for group 2 the first pre-discrimination generalization test as base-line, we obtain $r = 0{\cdot}66$ ($P < 0{\cdot}05$), i.e. the greater the peak shift, the greater is the behavioural contrast.

If these arguments may be taken as indicating that it is correct to calculate behavioural contrast for group 2 from the base-line of the first pre-discrimination generalization test, we may conclude that, out of all 14 subjects, on the assumption that the subject who showed a 6 per cent increase was not showing behavioural contrast, 10 subjects did display contrast. A t test on the log percentage change scores for all 14 subjects from pre- to post-discrimination generalization tests shows a significant increase ($t = 2{\cdot}49$; d.f. $= 12$; $P < 0{\cdot}05$).

4. *Personality and developmental measures*

The intercorrelations between personality and developmental measures are presented in Table 2; this includes a relative reading age score (RA-CA), derived by subtracting chronological age (CA) from reading age (RA).

An important feature to note in Table 2 is the significant negative correlation ($r = -0{\cdot}63$) between the JEPI-N score and the TRS-N score. This indicates that children whose self-rating on neuroticism was low were rated by their teachers as

Table 2. *Product-moment correlations between personality and developmental measures* ($n = 14$)

(With 12 d.f., $P < 0{\cdot}05$ for $r = 0{\cdot}458$ and $P < 0{\cdot}005$ for $r = 0{\cdot}661$.)

	TRS-N	JEPI-N	JEPI-E	JEPI-L	CA	RA	RA-CA
TRS-N	—	−0·63	+0·17	+0·58	−0·62	−0·58	−0·48
JEPI-N	−0·63	—	−0·26	−0·93	+0·34	+0·61	+0·49
JEPI-E	+0·17	−0·26	—	+0·09	−0·28	−0·17	−0·12
JEPI-L	+0·58	−0·93	+0·09	—	−0·74	−0·70	−0·58
CA	−0·62	+0·34	−0·28	−0·74	—	+0·91	+0·70
RA	−0·58	+0·61	−0·17	−0·70	+0·91	—	+0·93
RA-CA	−0·48	+0·49	−0·12	−0·58	+0·70	+0·93	—

highly neurotic. This negative relationship was reflected in the correlations which emerged between each of these variables and all the other variables in this table. It is important, therefore, to decide which is the more likely to provide a true measure of neuroticism. The correlations between the two measures of N and the JEPI Lie (L) scale provide a rather clear answer to this question: there is a strong negative relationship between JEPI-N and L ($r = -0{\cdot}93$) and a positive correlation between TRS-N and JEPI-L ($r = +0{\cdot}58$). Thus it seems evident that the TRS provides a direct assessment of neuroticism and that, as a result of the tendency of neurotic children to present themselves in a favourable light (as indicated by their high L scores), the JEPI-N score is actually, at least in this sample, an *inverse* measure of neuroticism. We shall proceed on this assumption in our further analysis of the results.

Although no scale corresponding to the TRS-N was used in the present experiment to evaluate subjects on extraversion, the insignificant correlation between JEPI-E and JEPI-L suggests that the JEPI may be more reliable in its assessment of extraversion than in its assessment of neuroticism. The correlations between both TRS-N and JEPI-N, on the one hand, and JEPI-E, on the other, are low and insignificant.

If we consider the three developmental variables, it is clear from Table 2 that these are unrelated to JEPI-E. They are strongly related, however, to both measures of neuroticism: the older the child, the more advanced his reading age, and the more relatively advanced his reading age, the less neurotic is he rated by his teacher, the more he rates himself as neurotic, and the lower his scores on the Lie scale of the JEPI.

5. *Correlations between behavioural and personality/developmental measures*

In this section we consider the interrelations between the behavioural measures derived from the discrimination task (i.e. the measures listed in Table 1), and the personality and developmental variables.

The relevant first-order correlations are presented in Table 3.

Owing to the existence of complex correlations between the behavioural measures (Table 1) and also between the personality/developmental measures (Table 2), it becomes necessary to use partial correlations to examine more carefully the meaning of the first-order correlations obtained between these two sets of measures (Table 3).

Table 3. *Product-moment correlations between behavioural, personality and developmental measures* ($n = 14$)

(With 12 d.f., $P < 0{\cdot}05$ for $r = 0{\cdot}458$.)

		Mean shift 1	$S^D - S^\Delta$ average response rate 2	Beyond-S^D average response rate 3	Generalization 4
TRS-N	5	+0·37	+0·31	+0·52	+0·59
JEPI-N	6	−0·29	−0·28	−0·35	−0·28
JEPI-E	7	−0·35	+0·04	−0·17	+0·10
CA	8	−0·18	−0·13	−0·26	−0·24
RA	9	−0·36	−0·08	−0·28	−0·24
RA-CA	10	−0·37	−0·01	−0·25	−0·15

Table 4. *Partial correlation coefficients between behavioural, personality and developmental measures* ($n = 14$)

(Variables to the right of the dot are held constant. First-order correlations shown at bottom of each block of partial correlations.)

		Mean shift 1	$S^D - S^\Delta$ average response rate 2	Beyond-S^D average response rate 3	Generalization 4
TRS-N	5	51·2 = +0·47*	52·1 = +0·43	53·1 = +0·47*	54·1 = +0·58*
		51·3 = +0·60*	52·3 = −0·34	53·2 = +0·53*	54·2 = +0·67*
		51·23 = +0·90**	52·13 = +0·85**	53·12 = +0·31	54·3 = +0·34
		51·4 = +0·36	52·4 = +0·48*	53·4 = −0·03	54·23 = +0·52*
		+0·37	+0·31	+0·52*	+0·59*
JEPI-N	6	61·2 = −0·38	62·1 = −0·37	63·1 = −0·30	64·1 = −0·36
		61·3 = −0·22	62·3 = +0·05	63·2 = −0·22	64·2 = −0·08
		61·23 = −0·50*	62·13 = −0·46	63·12 = +0·41	64·3 = +0·09
		61·4 = −0·26	62·4 = −0·08	63·4 = −0·23	64·23 = +0·07
		−0·29	−0·28	−0·35	−0·28
JEPI-E	7	71·2 = −0·37	72·1 = −0·04	73·1 = −0·09	74·1 = +0·16
		71·3 = −0·32	72·3 = +0·39	73·2 = −0·41	74·2 = +0·13
		71·23 = +0·18	72·13 = +0·26	73·12 = −0·21	74·3 = +0·59*
		71·4 = −0·37	72·4 = −0·09	73·4 = −0·60*	74·23 = +0·52*
		−0·35	+0·04	−0·17	+0·10
CA	8	81·2 = −0·22	82·1 = −0·18	83·1 = −0·22	84·1 = −0·22
		81·3 = −0·12	82·3 = +0·20	83·2 = −0·30	84·2 = −0·25
		81·23 = +0·21	82·13 = +0·26	83·12 = −0·29	84·3 = −0·02
		81·4 = −0·15	82·4 = +0·15	83·4 = −0·10	84·23 = −0·10
		−0·18	−0·13	−0·26	−0·24
RA	9	91·2 = −0·39	92·1 = −0·17	93·1 = −0·21	94·1 = −0·20
		91·3 = −0·31	92·3 = +0·34	93·2 = −0·43	94·2 = −0·34
		91·23 = +0·04	92·13 = +0·15	93·12 = −0·19	94·3 = +0·03
		91·4 = −0·34	92·4 = +0·25	93·4 = −0·15	94·23 = −0·11
		= −0·36	−0·08	= −0·28	= −0·24
RA-CA	10	101·2 = −0·38	102·1 = −0·10	103·1 = −0·17	104·1 = −0·11
		101·3 = −0·33	102·3 = +0·44	103·2 = −0·49*	104·2 = −0·27
		101·23 = +0·27	102·13 = −0·40	103·12 = −0·43	104·3 = +0·17
		101·4 = −0·35	102·4 = +0·24	103·4 = −0·27	104·23 = +0·02
		−0·37	−0·01	−0·25	−0·15

* $P < 0·05$. ** $P < 0·005$.

Table 5. *Partial correlation coefficients between personality and behaviour variables when developmental variables are held constant* ($n = 14$)

(Variables 8, 9 and 10 are CA, RA and RA-CA. Variables to the left of the dot are held constant. First-order correlations shown at bottom of each block of partial correlations.)

		TRS-N 5	JEPI-E 7
Mean shift	1	15·8 = +0·33	17·8 = −0·42
		15·9 = +0·21	17·9 = −0·45
		15·10 = +0·24	17·10 = −0·43
		+0·37	−0·35
Generalization	4	45·8 = +0·58*	47·8 = +0·03
		45·9 = +0·57*	47·9 = +0·06
		45·10 = +0·59*	47·10 = +0·08
		+0·59	+0·10

We present in Tables 4 and 5 the results of such of these calculations (out of a total of more than 200) as proved to be of interest. Table 4 shows the correlations between some of the personality variables and the behavioural variables with other behavioural measures held constant; Table 5 shows the same correlations with developmental measures held constant. These tables bring to light a number of intriguing relationships.

First, there is the extremely high correlation ($r = +0{\cdot}90$; $P < 0{\cdot}005$) between the TRS-N score and mean shift when response rates both at stimuli between S^D and S^Δ and at stimuli beyond S^D are held constant. By this technique, we have, in effect, a pure measure of central tendency in the generalization distribution around S^D and this proves to be, in our sample, an almost pure measure of neuroticism as gauged by the TRS. It is also clear that the first-order correlation of $+0{\cdot}37$ between TRS-N and mean shift cannot be due to depression of response rate at stimuli close to S^Δ since both (*a*) the first-order correlation between TRS-N and S^D-S^Δ response rate is positive ($+0{\cdot}31$) and (*b*) this correlation becomes even more positive ($+0{\cdot}85$; $P < 0{\cdot}005$) when both mean shift and response rate beyond S^D are held constant. The high positive first-order correlation ($+0{\cdot}52$) between TRS-N and beyond-S^D response rate appears to be mediated by the relation of TRS-N to other variables, since this correlation is reduced to $+0{\cdot}31$ when both mean shift and S^D-S^Δ response rate are held constant and to $-0{\cdot}03$ when generalization is held constant. Generalization displays a strong positive relationship to TRS-N both at the first-order level and in the partial correlations. Furthermore, neither the correlation between TRS-N and mean shift, nor the correlation between TRS-N and generalization, changes when the former is held constant for generalization and the latter for mean shift. This indicates that the relationships between each of these behavioural measures and TRS-N are independent of each other.

Turning to the JEPI-E score, there is a first-order correlation between this and mean shift which is in the predicted direction ($r = -0{\cdot}35$), but which fails to reach significance, and even this correlation is destroyed when we hold constant for response rates on both sides of S^D. However, an examination of the relations between JEPI-E and the other behavioural measures shows that extraversion is significantly negatively

Table 6. *First-order product-moment correlation coefficients between behavioural contrast (log percentage change in base-line) and variables* 1–10 *compared with the correlation coefficients between mean shift and the remaining variables*

		Behavioural contrast ($n = 10$)	Mean shift ($n = 14$)
Mean shift	1	+0·66*	
$S^D - S^\Delta$ rate	2	+0·09	−0·22
Beyond-S^D rate	3	+0·39	+0·26
Generalization	4	+0·37	+0·14
TRS-N	5	+0·36	+0·37
JEPI-N	6	+0·31	−0·29
JEPI-E	7	−0·31	−0·35
CA	8	−0·62*	−0·18
RA	9	−0·69*	−0·36
RA-CA	10	−0·42	−0·37

* $P < 0{\cdot}025$.

(−0·60) correlated with beyond-S^D response rate when generalization is held constant, and significantly positively (+0·59) correlated with generalization when beyond-S^D response rate is held constant.

The partial correlations, holding constant for behavioural measures, did not reveal anything of statistical significance with respect to the developmental variables, with the exception of the correlation between RA-CA and beyond-S^D response rate holding constant for S^D-S^Δ response rate (−0·49, $P < 0·05$). This correlation indicates that beyond-S^D response rate decreases as the child's reading age becomes advanced relative to his chronological age. The moderately high first-order correlations (−0·36 and −0·37) between RA and RA-CA, on the one hand, and the mean shift, on the other, were both destroyed when response rates on both sides of S^D were held constant (becoming +0·04 and +0·27 respectively).

Turning to the partial correlations contained in Table 5, the main points to note are the reduction in the correlation between TRS-N and mean shift, and the increase in the correlation between JEPI-E and mean shift, when these are held constant for the three developmental measures. The correlation between JEPI-E and mean shift now approaches significance ($P = 0·05$ at $r = 0·48$, compared to correlations ranging from −0·42 to −0·45). The reduction in the TRS-N × mean shift correlation is of less importance than would at first appear. It will be recalled that this correlation, when developmental measures are not held constant, is greater when response rates on both sides of S^D are held constant (Table 4: $r = +0·90$). Now, as can also be seen in Table 4, the correlations between the developmental measures and mean shift (for which the Table 5 partial correlations hold constant) actually reverse sign when themselves held constant for response rates on both sides of S^D. Thus, for the pure measure of central tendency in the distribution of response rates around S^D, the developmental measures do *not* contaminate the mean shift × TRS-N correlation. No important findings emerged from the partial correlations between developmental and behavioural measures, when these were held constant for the personality measures.

Finally, in this section, we deal with the correlations between behavioural contrast (log percentage change in base-line) and the remaining measures for the n of 10 subjects who in fact displayed contrast. These are presented in Table 6. For comparison, this table also presents again the first-order correlations between mean shift and the remaining measures, both behavioural and personality/developmental, for the n of 14. The most striking feature of this table is the overall agreement in the pattern of correlations between each of these two measures, log behavioural contrast and mean shift, and the other variables. Major departures from this similarity are encountered only for JEPI-N and CA. It is also noteworthy that the correlations between the developmental measures and behavioural contrast are higher than the corresponding correlations for mean shift, and in two cases (CA and RA) attain statistical significance: the direction of correlation is the predicted one, greater maturity going with less contrast. Given the smallness of the sample, partial correlations were not attempted for behavioural contrast.

APPENDIX

At the beginning of the Results section, we gave notice of our intention to exclude from our main analysis data on three subjects who constituted exceptions to several of the relationships we have described. If we include these subjects in our analysis, three relationships break down: these are the first-order correlations between mean shift and RA-CA, mean shift and TRS-N, and finally mean shift and behavioural contrast. Three of these relationships involve mean shift, a measure on which all three subjects stand out: one subject was the only child who actually displayed a mean shift in the direction *towards* S^{Δ}, the second subject displayed the smallest mean shift of the whole population, while the third subject showed the largest mean shift of all. Although the exclusion of subjects is always a dangerous procedure, the fact that correlations between mean shift and measures of personality (TRS-N), development (RA-CA) and behaviour (behavioural contrast) are all affected suggests strongly that, for reasons at present unclear, these subjects are genuinely different from the rest of the sample.

DISCUSSION

The major findings of this study are concerned with the relations between the personality and developmental measures, on the one hand, and the behavioural measures on the other. However, before we discuss these, there are a number of points which need to be made about each of these sets of measures in isolation.

Behavioural measures

Although neither the peak shift nor the behavioural contrast effects observed in the present study were as large as those obtained in our earlier experiment (Nicholson & Gray, 1971), it is clear that the basic findings of that experiment are confirmed: children, like pigeons (Terrace, 1966), display these phenomena when exposed to a similar discrimination learning situation. It can also be affirmed on the basis of the present results that the occurrence of behavioural contrast in children is not merely an artifact of continued response rate increase with continued exposure to the learning situation (see discussion of this point in second and third sections of the Results).

Our data are able to throw light on the meaning of the mean shift measurement introduced by Grusec (1968). As we have already pointed out in the first and fifth sections of the Results, there is good reason to believe that mean shift is not primarily a measure of response rate depression around S^{Δ}, so that it can with some legitimacy be used in place of Hanson's (1959) original mode shift measurement, over which it has the considerable advantage of being a continuous variable. It is of course unlikely that mean shift and mode shift behave in entirely similar ways, and only data which provide information on their covariation in the same population of subjects (not available in the present experiment, given the absence of mode shifts) would be able to show how similar their behaviour is. (We are at present analysing data from a new population which appear likely to provide an answer to this question.) What is quite clear from the present data is that, given the strong relationships which have emerged both between mean shift and behavioural contrast and between mean shift and the personality and developmental measures used, mean shift does behave in ways which would be expected also to be true of mode shift. The covariation between the magni-

tudes of *mean* shift and behavioural contrast observed in the present experiment, for example, is not surprising in the light of Guttman's (1965) and Terrace's (1966) data on the close relationship between the occurrence of *peak* shift and of behavioural contrast in the pigeon. The correlations between mean shift and the personality/developmental measures (which we deal with below) are predictable on the view that *mean* shift is a measure of susceptibility to frustration, while the view that *peak* shift is due to the frustrating effects of non-reward has a considerable degree of empirical support (Bloomfield, 1967; Gray & Smith, 1969; Terrace, 1966). But the correlation between behavioural contrast and beyond-S^D response rate of $+0{\cdot}39$ serves as a reminder that the latter may also be closely related to peak shift.

The method we developed in the present experiment to measure behavioural contrast—log percentage base-line change—was successful in yielding meaningful relationships, not only with mean shift, but also with the personality/developmental measures. The diminished size of the behavioural contrast effects obtained in the present experiment (second section of Results) as compared with our earlier experiment (Nicholson & Gray, 1971), in which rate increases ranging from 125 to 1000 per cent were observed, is probably due to the fact that in the present instance we observed contrast after intradimensional discrimination training (and the consequent distortion of the generalization curve as reflected in the mean shifts) whereas in our earlier study contrast was measured after presence/absence discrimination training, the intradimensional training being presented only later.

Personality measures

A comparison of our results using the JEPI with the standardization data contained in the manual for the JEPI (Eysenck, 1965) shows substantial agreement for the correlations between the N, E and L scales and for the trends of each of these with age. The exceptions are as follows. The trends for E with age given by Eysenck (1965) show a slight tendency for this to rise over the years 7–11, whereas our own data show a slight, and insignificant, tendency in the opposite direction ($r = -0{\cdot}28$). The correlation between L and N in our data takes the same direction as in the standardization sample, but it is, for the sample of 14, much stronger ($r = -0{\cdot}93$ as compared with app. $-0{\cdot}3$). However, for our larger sample of 43 (which included the sample of 14) the correlation between N and L dropped to $-0{\cdot}24$. It would seem, therefore, that in this respect our sample does not differ from the standardization sample, but that selection of extreme subjects has inflated the correlation between N and L. Much the same appears to be true for the trend in L scores with age. The standardization data (Eysenck, 1965) show a definite decline in L scores between ages 7 and 11, and this also appears in our correlation of $-0{\cdot}74$ between L and CA in the sample of 14. In the sample of 43, however, this correlation fell to $-0{\cdot}42$. Even so, it is clear that, in the 7- to 11-year age group, the N scale from the JEPI cannot be regarded as a valid index of neuroticism. As we have seen (fourth section of Results), in our own small sample of 14 the JEPI-N score is apparently *negatively* related to true neuroticism. In contrast the Teacher's Rating Scale appears to be an instrument of considerable potential, as well as of good reliability (test–retest reliability = 0·81 and inter-rater concordance = 0·83; Nicholson, unpublished).

Correlations between behavioural and personality/developmental measures

We come finally to the interrelations between the personality/developmental variables and the behavioural measures. Among the latter, we include the tendency to generalize. The very high correlation (+0·87) between response rates on either side of S^D when these are expressed as a proportion of the absolute S^D response rate shows that the tendency to generalize is a highly consistent aspect of the individual child's personality. Indeed, if we hold constant for mean shift, the resulting partial correlation between these two proportionate response rates reaches the extremely high value of +0·98. In terms of the model for discrimination learning which underlies our thinking, the tendency to generalize can only reflect the activity of the 'reward mechanism' (Gray & Smith, 1969). It might therefore be possible to use the magnitude of this tendency to measure individual susceptibility to reward, in the same way that we propose to use mean shift and behavioural contrast as measures of susceptibility to frustration. As we shall now see, such an approach is able to make very good sense out of our data. In this connexion, it should be mentioned that the theory of neuroticism and introversion/extraversion proposed by Gray (1970) treats neurotics as having high sensitivity both to reward and to frustrative non-reward and extraverts as having high relative sensitivity to reward and low relative sensitivity to non-reward. Thus we would expect our measure of generalization (on the assumption that this reflects sensitivity to reward) to increase with both neuroticism and extraversion.

If we start with neuroticism, and take the TRS-N score as our defining criterion for this, we may summarize our results as follows: increasing neuroticism leads to increasing mean shift (when this is treated as a pure measure of central tendency by holding constant for response rates on both sides of S^D: $r = +0{\cdot}90$; $P < 0{\cdot}005$); possibly to increasing behavioural contrast ($r = +0{\cdot}36$; non-significant); and to increasing generalization ($r = +0{\cdot}59$; $P < 0{\cdot}05$). All these findings are in agreement with our predictions.

Turning to extraversion, and using the JEPI-E score as our criterion, there is a tendency for increasing extraversion to be associated with decreasing mean shift, but this tendency is reliable neither in terms of statistical significance nor when partial correlations are used. Increasing extraversion is, however, associated with decreasing proportionate response rates to stimuli beyond S^D ($r = -0{\cdot}60$; $P < 0{\cdot}05$ when generalization is held constant); it tends also to be related to decreasing behavioural contrast ($r = -0{\cdot}31$; non-significant); and it is significantly related to increasing generalization ($r = +0{\cdot}59$; $P < 0{\cdot}05$) when beyond-S^D response rate is held constant. These results, too, are in line with expectation.

Given these findings, it will perhaps help to make the complex picture revealed by the partial correlation coefficients intuitively more obvious if we consider the different influences at work on children of different personality types during generalization testing in our situation. Consider first a highly neurotic child. He has a strong tendency to generalize (sensitivity to reward) and also a strong tendency to shift his responses away from S^D in the direction opposite to S^Δ (sensitivity to non-reward). These two tendencies will *summate* at stimuli beyond S^D but *oppose* each other at stimuli between S^D and S^Δ. Consider next an extreme extravert. He also has a strong tendency to generalize, but a weak tendency to shift his responses to stimuli beyond S^D. These

tendencies will (relative to what should occur in an extreme introvert) *oppose* each other at stimuli beyond S^D and *summate* at stimuli between S^D and S^Δ.

One odd feature of the results is that the tendency to shift responses away from S^D is reflected among neurotics most clearly as a pure mean shift, whereas among introverts it is reflected as an increase in the proportionate response rate at stimuli beyond S^D. This is perhaps an accidental feature of our results, but it may indeed reflect a difference in the way neurotics and introverts respond to frustrative non-reward.

A greater tendency to generalize has been noted previously among neurotics (Mednick & Freedman, 1960), but always in experiments involving generalization of expectation of punishment. As far as we are aware, this is the first report of a correlation between neuroticism and generalization of expectation of reward.

Turning to the developmental measures, these too behaved in the predicted manner. In agreement with the evidence reviewed by Lawson (1965) suggesting that susceptibility to frustration declines with age, our data showed that mean shift, beyond-S^D response rate and behavioural contrast all declined with CA, with RA and with RA-CA, though only the correlations between CA and behavioural contrast and RA and behavioural contrast ($r = -0{\cdot}62$ and $-0{\cdot}69$, respectively; $P < 0{\cdot}05$) attained statistical significance.

It is of interest to note that behavioural contrast proved to be the most sensitive measure with respect to the developmental variables, though it was less sensitive than mean shift and beyond-S^D response rate for the personality variables. It should be noted that, as already discussed in the fifth section of the Results, the partial correlations indicate that the relations between, on the one hand, *personality* and our behavioural measures, and on the other, *development* and our behavioural measures are essentially independent of each other.

This research was supported by a grant from the Social Science Research Council of Great Britain. We are extremely grateful to the Acting Headmistress and the staff of the Edward Field County Primary School, Kidlington, Oxfordshire. We would also like to thank P. Bryant, P. Dean and M. Goodale for their helpful advice and criticism.

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(*Manuscript received* 1 *February* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 63–72
Printed in Great Britain

WORD-ASSOCIATION RESPONSE COMMONALITY AND THE GENERATION OF ASSOCIATIVE STRUCTURES

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Subjects were selected as high or low in the number of primaries given in a word-association task under both standard instructions and with instructions to give popular associations. Low-commonality scorers produced words with a larger number of interconnexions than high-commonality scorers, but did not produce more different words on a later serial-association task. Commonality score under popular set did not seem to affect the production of associations. The associations of high-commonality subjects seemed to consist more frequently of the repetition of the original stimulus words and the original responses to those words as responses later in the task compared to low-commonality scorers.

Previous research relating associative commonality, the tendency to give popular responses in a free word-association test, to personality and ability variables has produced such conflicting results that one is led to consider that commonality will appear as a significant individual difference only in situations which evoke the intra-verbal habits of a subject (Jenkins, 1960). It may be that any differences between high- and low-commonality subjects appear only in conditions where the generation or utilization of verbal relationships is required.

Jenkins (1960) suggests that low-commonality subjects have 'weak' connexions between words while high-commonality subjects have 'strong', stable connexions. Evidence for this comes from studies by Van der Made-Van Bekkum & Prevo-Molenaar (1968), who found that when subjects were given a word-association test (WAT) and were then asked to recall their associations, all subjects gained in commonality score, but low-commonality subjects (on the WAT) gained more than high-commonality. This seems to indicate that the WAT responses of the low-commonality subjects are uncommon, even for them, and are likely to be forgotten and replaced by other responses.

Other data suggest that low-commonality subjects are capable of making the associations which are common in the verbal community. Their performance in the context of a WAT may indicate that the relationships between words are weaker than for high-commonality subjects. It may also suggest that they perceive the WAT as a situation in which to be original.

Jenkins (1959) found that low-commonality subjects gained in commonality score when instructed to give the responses which they considered would be given by most subjects. Low-commonality subjects gained more than high-commonality subjects. Overall there was a correlation of 0·67 between commonality scores under the two sets of instructions when the tests were separated by one month. Horton *et al.* (1963) also found a gain in commonality score under 'popular' set. Van der Made-Van Bekkum & Van der Kam (1966) obtained a significant correlation of −0·34 between commonality scores under free-association instructions and gain score. Data from the Jenkins (1959) experiment would suggest that any effect is not due to ceiling effects.

Wynne *et al.* (1965) found that instructions to give a popular response on a WAT produced an increase in the number of non-antonym popular responses while, if any-

thing, there was a decrease in the number of antonym primaries. This seems contradictory to the results of Kjeldergaard (1962), who found that instructions to give 'opposite' responses increased the number of primaries, and that this was especially effective for low-commonality subjects. Wynne *et al.* (1967) suggest that this apparent discrepancy may be a result of the additional association practice in the Kjeldergaard (1962) study. In that study subjects were given free association and then 'opposites' instructions. In the Wynne *et al.* (1965) study subjects experienced only one condition. In the Wynne *et al.* (1967) experiment subjects experienced a WAT under several conditions and in these casès there was an increase in antonym primary responses.

Such evidence does suggest that when asked to do so, low-commonality subjects are capable of emitting the verbal connexions which they possess and which mirror the associations existing in the verbal community. Further evidence for this comes from experiments in communication when subjects, required to transmit to another subject enough information, in the form of word associations, to enable that other person to choose a correct alternative, use common associations and can solve the task (Feffer & Suchotliff, 1966; Rosenberg & Cohen, 1966).

The difference between high and low commonality of subjects on a WAT may be not so much one of ability as of motivation. Low-commonality subjects may see the WAT as a challenge to be original. When required to produce the common associations available to them, they are capable of doing so. The evidence is conflicting as to whether low-commonality subjects are superior, inferior or equal to high-commonality subjects in the utilization rather than the production of common and uncommon associations as, for example, in studies of learning (Jenkins, 1960; Nunnally *et al.*, 1967).

As Brown (1960) has pointed out, however, there is still the possibility that low-commonality subjects are of two distinct types. There may be those who take the WAT as an invitation to be original. There may also be those who do not know the normatively common associations. The present study explores such a possibility.

Baldridge & Hustmyer (1965) and Fosmire (1965) both used a variation of WAT, which can be termed 'serial discrete association', in which subjects give a single association to each stimulus word, each response word being added to the end of the original list of stimulus words and in turn becoming a stimulus word. With this technique Baldridge & Hustmyer (1965) claimed to have found key words with emotional significance appearing recurrently in the associations.

Fosmire (1965) used the technique to study differences between high- and low-commonality scorers in their ability to generate clusters of words and he found low-commonality subjects to be less constrained by the normative associative strength of the S–R pairs and to be more likely to produce large and complex sets of words, closely interrelated with each other. He also found that words which had a larger number of associations tended to be more strongly affective in character when produced by low- as against such words produced by high-commonality subjects.

The experiment reported here is a partial replication of Fosmire's (1965) study in an attempt to support his results and is also an attempt to test Brown's (1960) notion of two types of low-commonality scorer, subjects giving associations under standard and popular sets.

Method

The general procedure used by Fosmire (1965) and by Baldridge & Hustmyer (1965) was employed.

Subjects were administered a 50-item word association task (WAT) under two conditions. In the first administration the subjects were asked to give the first association that came to mind for each stimulus word. After completion of this form an identical one was passed out and the subjects this time were asked to give what they thought were common responses to the test.

Ninety-nine subjects were tested under both sets of instructions. The bivariate distribution was plotted in order to select subjects who were in the four response categories, formed by a median split on the distributions under both sets of instruction: High Commonality (HiCo) under both sets of instructions (HH); HiCo under standard instructions and Low Commonality (LoCo) under popular set (HL); LoCo under standard and HiCo under popular (LH); LoCo under both instruction sets (LL).

It was expected, and it became obvious from the plot, that few subjects fell into the second category (HL). Only seven subjects were above the median under standard instructions and below the median under popular set. Therefore only subjects from the other three categories were tested in the second task.

A median split was made on each commonality dimension and subjects above and below the median score were selected to fall into the appropriate classifications. Twelve subjects were tested from each of the three categories. These 36 subjects were then tested individually on the serial discrete association task.

Task. In a second session, subjects were presented with a list of stimulus words (S_1) and had to give a single response (R_1) to each stimulus. On the next trial each subject was presented with his own R_1 words as the stimulus words (S_2). These S_2 words elicited R_2 words which were in turn used as the S_3 stimulus list. All subjects went through the following series of trials. $S_1 \rightarrow R_1$; $R_1(S_2) \rightarrow R_2$; $R_2(S_3) \rightarrow R_3$; $R_3(S_4) \rightarrow R_4$; $R_4(S_5) \rightarrow R_5$. There was a 2 min. break between stimulus presentations.

The list used in this study was constructed from words in the Kent–Rosanoff (K–R) list in order to maintain greater continuity with previous work on commonality, despite the fact that the K–R list is composed of a set of high-frequency nouns which tends to elicit, coordinate or contrast responses.

An attempt was made, however, to sample words from the K–R list according to the characteristics of the distribution of response probabilities. By selecting words with differently shaped associate hierarchies it was hoped, first, to allow greater generalization from the results with the present list and, second, to observe possible differences between HiCo and LoCo subjects in the utilization of response words in the generation of associative clusters.

Thirty words were selected from the K–R list according to criteria suggested by Bilodeau (1966). He has demonstrated that words in the K–R list can be divided into three categories according to the distribution of responses listed in the normative tables. These three categories are:

(*a*) Words with a low probability (p) of the primary (P_1) and secondary (P_2) responses and a high p for responses at and below the tertiary response (P_{3-n}).

(*b*) A moderately high value of P_1 and P_{3-n}, with P_2 at a maximum.

(*c*) A high value of P_1, with low values of P_2 and P_{3-n}.

Table 1. *Stimulus words with Thorndike–Lorge frequencies*

Category A	T-L f	Category B	T-L f	Category C	T-L f
Baby	AA	Bath	46	Bed	AA
Butterfly	22	Carpet	24	Blossom	43
Cabbage	16	Cottage	46	Boy	AA
Cheese	45	Joy	AA	Butter	AA
Doctor	AA	Mutton	10	Eagle	38
Earth	AA	Ocean	AA	King	AA
Memory	A	Religion	A	Lamp	A
Music	AA	River	AA	Man	AA
Street	AA	Stem	39	Scissors	8
Wish	AA	Thief	28	Table	AA

Bilodeau and his co-workers have used these sets of words in free and stimulated recall experiments and have found the nature of intrusions in recall to be predictable from the response hierarchies produced by these words.

The words in the present experiment were selected from the 1960 Minnesota norms of Palermo & Jenkins (1964) to match as closely as possible the values reported in Bilodeau. The P_1, P_2 and $P_{(3-n)}$ values from the 1964 norms were plotted and essentially the same shape of plot was found as for the 1954 norms. The probabilities or response categories for the samples of words in the present experiment were:

(a) $P_1 = 0{\cdot}14$; $P_2 = 0{\cdot}11$; $P_{3-n} = 0{\cdot}76$.
(b) $P_1 = 0{\cdot}30$; $P_2 = 0{\cdot}20$; $P_{3-n} = 0{\cdot}50$.
(c) $P_1 = 0{\cdot}73$; $P_2 = 0{\cdot}05$; $P_{3-n} = 0{\cdot}22$.

The 30 words used in the present experiment, in their appropriate categories, together with the Thorndike–Lorge frequencies, are presented in Table 1. The 30 words were arranged in four different random orders, with the limitation that no two consecutive words were from the same category. Different orders were used to avoid some of the criticisms that can be levelled at much word-association research (Jung, 1966) because of the repeated use of fixed orders of presentation of items.

Subjects were assigned to WAT forms at random and were tested individually. The experimenter was not aware of the commonality status of the subjects while testing.

The subjects had of course responded to a K–R WAT in order to be assigned to the experimental condition. The nature of the serial-association task is different, however, and as the subjects were tested four months after the first administration of the K–R WAT there seemed little possibility of any serious contamination of the responses in the experimental session. Sixteen of the words used in the serial-association task were included in the K–R WAT used for selection of subjects.

RESULTS

Preliminary WAT administration. In the first session carried out to assign subjects to experimental conditions, the mean Commonality (Co) score under standard instructions was $18{\cdot}14 \pm 6{\cdot}93$ and under popular instructions the mean was $25{\cdot}51 \pm 6{\cdot}45$ ($n = 99$ in both cases). Jenkins (1959), using a 100-word form of the K–R WAT, obtained mean scores of $38{\cdot}1 \pm 9{\cdot}6$ and $46{\cdot}7 \pm 8{\cdot}9$ under standard and popular sets when these conditions were separated by 5 min., the nearest condition to the present. The mean scores are very similar, allowing for length of list. The correlation between Co scores under the two conditions in the present experiment was 0·596, appreciably higher than the correlation of 0·48 obtained in Jenkins' (1959) study. As in the Jenkins experiment LoCo scorers showed the greatest gain scores under popular set.

The WAT was also scored for the number of opposites given to Opposite-Evoking-Stimulus (OES) (Carroll *et al.*, 1962) and for the number of primaries given to non-OES (Co ($\overline{\text{OES}}$)). The mean Co ($\overline{\text{OES}}$) scores were $8{\cdot}96 \pm 3{\cdot}09$ and $13{\cdot}03 \pm 3{\cdot}21$ in the standard and popular set conditions respectively. The mean OES scores were $9{\cdot}12 \pm 4{\cdot}71$ and $12{\cdot}53 \pm 4{\cdot}35$ for the two conditions. The data thus show an increase in both non-antonym primaries and in antonym responses under popular instructions. These data show something of the same effect as that present in Wynne *et al.* (1967), except that there is an increase in the antonym responses as a result of popular instructions.

The correlation between Co ($\overline{\text{OES}}$) and OES under standard instructions was 0·55 and under popular instructions 0·45.

The correlation between the Co ($\overline{\text{OES}}$) scores under the two instruction sets was 0·40, while the correlation between the OES scores was 0·61, indicating that subjects

show rather more consistency in rank order when asked to give populars and the stimuli are opposite-evoking.

Because of the greater likelihood that Co ($\overline{OES}$) scores are tapping individual differences in associative behaviour over and above the over-learned cultural responses to OES (DiVesta & Bernheim, 1967), the subjects were categorized into the various groups described earlier on the basis of the Co ($\overline{OES}$) scores.

The median scores under standard and popular conditions were 8 and 13 respectively. Subjects with scores above and below these medians were assigned to the three experimental categories, HH, LH and LL, at random until there were 12 subjects in each group.

Serial-association task. Associative clusters were drawn for each subject, a cluster being any group of words related by manifest association (Fosmire, 1965; Kiss, 1968). For example, 'king' and 'queen' comprise a cluster if, given as a stimulus word, each elicits only the other word.

Two measures were taken from this first clustering analysis, the number of clusters produced and the number of different words produced. These data were analysed in a mixed model 3×4 analysis of variance with three categories of subjects and four word orders. The second factor was taken as a random factor.

The only significant effect was between categories of subjects selected on the basis of their Co scores ($F = 7{\cdot}99$; d.f. $= 2, 24$; $P < 0{\cdot}025$). The word order and groups $\times$ word order interaction terms were not significant. In this serial-association test the initial order of stimulus words seems to have no effect on the kind of associative structure produced.

The mean scores were $16{\cdot}42 \pm 5{\cdot}07$, $16{\cdot}50 \pm 2{\cdot}65$ and $21{\cdot}25 \pm 4{\cdot}20$ for LL, LH and HH groups respectively. It thus appears that what is important is the Co score produced by the subject under standard instructions. The LoCo subject who does not produce the popular associations when instructed to do so (LL group) does not appear to differ from the LoCo scorer who can (LH). Brown's (1960) hypothesis suggesting the existence of a difference between these groups does not seem to hold, at least for this task. Both of these groups do differ, however, from the HH group which seems to produce more, i.e. smaller, associative clusters in the serial-association task. The LL and LH groups do seem to differ in the spread of cluster scores. The LL group has a higher standard deviation than the LH and this may indicate that the LL group was still somewhat heterogeneous, still perhaps containing subjects who were capable of giving popular associations but who persisted in taking an 'original' set in the WAT under all instruction conditions.

The analysis of the number of different words produced no significant differences between commonality groups or word orders. The Co $\times$ word order interaction was also insignificant. The mean scores for the three groups in number of different words produced were LL $103{\cdot}00 \pm 22{\cdot}32$; LH $107{\cdot}42 \pm 23{\cdot}54$; HH $92{\cdot}58 \pm 18{\cdot}86$. Note that the total number of different words a subject could produce, if he produced a different response to every stimulus, equals $30 + (5 \times 30)$ or 180 words. The minimum number of words which could be produced was 31, if the subject gave the same response to all stimuli.

Two further measures of clustering behaviour, the size of the largest and of the smallest cluster produced by the subjects, were analysed.

The mean sizes in number of different words for largest and smallest cluster respectively were: LL, $28{\cdot}16 \pm 23{\cdot}64$ and $2{\cdot}67 \pm 1{\cdot}07$; LH, $27{\cdot}08 \pm 14{\cdot}51$ and $2{\cdot}50 \pm 1{\cdot}17$; HH, $14{\cdot}75 \pm 14{\cdot}23$ and $2{\cdot}00 \pm 0{\cdot}00$. The HH group's largest clusters were significantly smaller than those produced by the LL and LH group. Note again the difference in variance of the LL compared to the other two groups.

There was a further difference between the groups, LoCo subjects producing fewer clusters composed of two words, i.e. clusters composed only of the original stimulus word and the first response. Analysis of variance showed a significant commonality groups effect ($P < 0{\cdot}05$). HH subjects produced an average of $6{\cdot}17 \pm 3{\cdot}59$ clusters of size two, LH, $2{\cdot}50 \pm 2{\cdot}24$ and LL, $3{\cdot}58 \pm 4{\cdot}29$. LoCo subjects thus seem to make greater use of interrelationships between words to produce clusters of moderate size.

Again, however, a difference emerges between the LL and LH groups in the standard deviations of scores for the size of the largest clusters. The LL group does not react to the task in the same way as the LH group who are much more consistent in their responses as a group.*

Inspection of the data in the present experiment indicated no difference between the affective tone of the clusters produced.

It will be remembered that the initial stimulus lists were composed of words with different response distribution characteristics. The possibility existed of the words in different categories producing different kinds of clustering effects and being utilized in different ways by the HiCo and LoCo subjects in the formation of clusters.

A count was made of the number of stimulus words from each category which appeared in each of the clusters produced by the subject, and also of the size of cluster containing each stimulus word, averaged within the three stimulus classes. These methods do produce non-independent scores.

Inspection of the mean scores of the first index showed no difference between the groups in the number of clusters per stimulus category other than the already noted difference between the HH group and the others. No difference appeared in the use made of the different category words.

The second index, mean cluster size per stimulus category, showed a similar pattern of results, except perhaps that the LL group tended to have larger clusters associated with A words than with B and C words.

The mean scores appear in Table 2.

In a previous analysis it was shown that HH subjects had more clusters of size *two*. No difference emerged, however, between the groups in the proportion of these small clusters which contained the various category stimulus terms. Table 3 gives the number of clusters of size *two* for the three commonality groups, together with the proportion of those containing stimulus words for each category.

There seems to be no difference between commonality groups in the utilization of words from different categories (the difference between the LH and HH groups in the proportion of clusters of size *two* produced by category C stimulus words is not statistically significant, $z < 1$) although stimulus categories do differ in the extent to

* Although the variances of the three experimental groups appear to differ, Hartley's F_{max} test indicates that in no case is the heterogeneity sufficiently large for us to reject the hypothesis of homogeneity of variance. The differences in mean value on the measures cited above can be taken as reliable.

which they are utilized by all subjects. Category C words seem to appear more frequently in small clusters, which might be expected from the normative data, which shows a high probability of emission of the dominant responses. What is interesting is that LoCo subjects seem to make no better use of the category A words than HiCo subjects.

Following DeBurger & Donahoe (1965), a measure of the associative proximity of the response words to the stimulus words was obtained by means of an associative index. This was constructed by giving a number from 1 to 7 to each response dependent upon its frequency of occurrence in the first six associates in the norms. Responses not appearing among the first six associates were given a value of 7.

These results are presented in Fig. 1 for the HH and LL groups, together with the results from the DeBurger & Donahoe paper for their chain and repeated association conditions.

The LL group begin by giving less common associates than the HH group and continue to give, within each response list, less common associations. These data also indicate that the serial-association task, by returning to the associate of a word after having associated to other stimuli, appears to produce a greater tendency for subjects to remain in the 'associative proximity' of the original stimulus words than does the production of chain or repeated associations.

Some previous research suggests that HiCo subjects are better able to recall the associates they give to stimulus words than are LoCo subjects. It could be that the HiCo subjects have smaller clusters of associations because they are better able to recall the associations they have given before to a particular stimulus word. To some extent, of course, the LoCo subjects must also 'remember' the responses they made. No subject gave the maximum number of different words possible, which would indicate that all subjects keep referring to a limited set of stimulus words. The LoCo subject in this study appears to give associations which refer back to other associations in other parts of the response list. In a sense he is 'recalling' previous associations. The difference may be that the HiCo subject recalls associations that have

Table 2. *Mean number of clusters containing A, B and C category words and mean size of clusters per stimulus category for commonality groups*

Commonality group	Number of clusters			Size of clusters		
	Stimulus category			Stimulus category		
	A	B	C	A	B	C
LL	7·18	7·25	7·59	12·63	10·33	10·45
LH	7·93	7·25	7·75	9·50	9·88	9·61
HH	9·00	8·08	8·08	5·56	6·13	6·13

Table 3. *Mean clusters of size two and the proportion stimulated by stimulus category for each commonality group*

Commonality group	Number of clusters	Proportion stimulated by category		
		A	B	C
LL	3·58	0·25	0·24	0·51
LH	2·50	0·20	0·20	0·60
HH	6·17	0·30	0·20	0·50

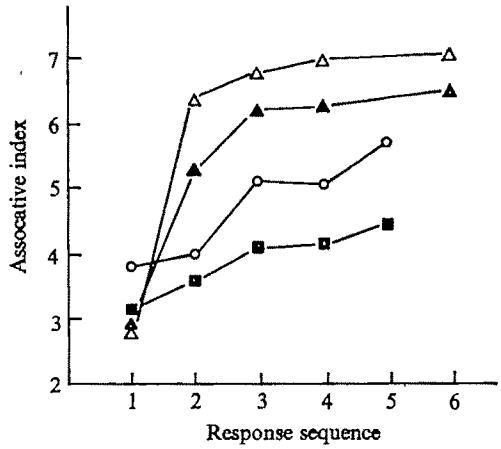

Fig. 1. Association index of responses as a function of ordinal number of the response sequence for HH and LL groups on discrete serial-association task. ■, HH; ○, LL; △, chain; ▲, repeated.

been made to the particular stimulus word, whereas LoCo subjects recall associations that have been made to other stimulus words; they produce associates which are common to several stimuli.

A count was made of the times the original stimulus words reappeared as responses anywhere in the total association time, and a count was then made of the number of times that the subject's *original* response to the stimulus words was repeated. This analysis indicated that the HH group produced the original stimulus words, on average, 28·2 times, compared to the LL group's reproduction rate of 19·3 and the LH rate of 16·7. The HH group then followed the original stimulus with the first response 84·4 per cent of the time compared to a reproduction rate of 65·8 per cent for the LL and of 57·1 per cent for the LH groups. The difference between groups on the number of stimulus words reproduced is significant ($F = 3{\cdot}46$; d.f. = 2, 33; $P < 0{\cdot}05$) and the difference between the percentage of reproduction of original responses is also significant ($F = 3{\cdot}38$; d.f. = 2, 33; $P < 0{\cdot}05$), following an angular transformation.

DISCUSSION

The results of this study suggest the existence of differences between HiCo and LoCo subjects on a different form of WAT. LoCo subjects seem to generate larger clusters of associations than do HiCo subjects, although there is no difference in the total number of words produced, the HiCo subjects seeming to produce more clusters composed only of the original stimulus word and the initial response. These differences appear to relate to the commonality scores of the subjects when they have taken the normal discrete response WAT under standard conditions. Differences do not emerge between HiCo and LoCo subjects when these scores have come under 'popular' instructions. For subjects who were low scorers under free instructions, no difference emerges on the serial-association task between HiCo and LoCo scores under 'popular' set.

These results replicate and extend those reported by Fosmire (1965). The differences were obtained using groups which were not formed from the extremes of the commonality dimension but which were composed of subjects above and below the medians of the distributions and selected at random. The use of non-OES words probably improved the discrimination of the measure of commonality. The results do not support Brown's (1960) hypothesis that there are two kinds of LoCo subjects, at least on this task, although the variance of the LoCo groups differed sufficiently in some cases to suggest that the 'popular' instructions given for the discrete WAT had not eliminated all idiosyncrasies of response in the subjects.

The subjects in this study seemed to use words differently in the generation of structures, the nature of the response distribution shown by the stimulus words in the normative tables being a determinant of use. Of particular interest, however, was the failure to find an interaction between commonality and stimuli. No difference emerged between HiCo and LoCo subjects in the use of the various words.

No difference was observed in the affective tone of the clusters, as was found by Fosmire (1965). The present result could have been due to the smaller number of stimulus words used in the present experiment, giving less opportunity for 'central' focus words to appear. It could also be due to use of a different set of words. Baldridge & Hustmyer (1965) do state, however, that the nature of the original stimulus words is largely immaterial.

The data also seem to indicate that HiCo subjects are both more likely to reproduce one of the original stimuli as a response, and on the reappearance of this word as a stimulus later in the task, repeat the original response given to it.

Of course, the fact that HiCo subjects recall S–R bonds more effectively than LoCo subjects does not explain the present results. They may recall because the S–R relationships are stronger for these words. Recall may be an index of the strength of a connexion; it is not a explanation for the connexion. The analysis of reproduction rate does not explain why LoCo subjects produce larger clusters than HiCo subjects. The LoCo subjects are less likely to 'retrace' a path back to an original stimulus and if they do give the word as a response, they are less likely to 'recall' the original response. Previous results would suggest that even if asked to do so, LoCo subjects would be less able to recall the S–R link than HiCo subjects (Van der Made-Van Bekkum & Prevo-Molenaar, 1968), although it would be of interest to do such an experiment using cued *v.* non-cued recall as did Jung (1967) with the successive WAT. Previous research results do not indicate, however, why LoCo subjects do not arrive back in a position where they are able to reproduce an earlier association, why they fail to repeat words as responses in the same way as do HiCo subjects.

The explanation must be looked for elsewhere, perhaps in the test attitude LoCo subjects take in such a situation. The LoCo subject may feel under less pressure to give the first response he thinks of, as suggested by Arthur & Freemantle (1966). This would account for the fewer number of clusters of size *two* found in this study. It does not square, however, with the absence of differences between groups in the use of category C words. Further work using this task and the successive WAT may be fruitful on this point.

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(*Manuscript received* 13 *March* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 73–84
Printed in Great Britain

THE SIZE CONSTANCY PROBLEM IN SCHIZOPHRENIA: A COGNITIVE SKILL ANALYSIS

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An attempt was made to reconcile discrepant findings in the size constancy performance of poor premorbid, process, non-paranoid schizophrenic men. It was postulated that an impairment of basic cognitive skills, and particularly of conservation, differentiates schizophrenics and normals, and under-constant and over-constant non-paranoid schizophrenics, and that this and an associated primitive visual centration effect may lead to paradoxical over-constancy for some schizophrenics. The results confirm that schizophrenics may be both under- and over-constant, and that they exhibit significantly low levels in primitive cognitive operations. The latter abnormalities are particularly marked in over-constant schizophrenics, for whom there is strong evidence of centration with a distant standard. Contradictory findings of over- as well as under-constancy in non-paranoid schizophrenics may thus be a function, apart from other determinants, of differences in the availability of conserving operations, and differences in centration strategies, where the latter would be influenced by characteristics of the experimental display.

For more than a decade researchers have attempted to identify and analyse the size constancy characteristics of schizophrenic patients, in the hope that this might yield knowledge of aetiological significance. Many inconsistent and contradictory results have been obtained. At various times these were explained by reference to poor sampling from heterogeneous subgroups, or to drug action, to differences in premorbid characteristics, or attention strategies. A number of excellent reviews have attempted to place this area of investigation into perspective (e.g. Silverman, 1964*a*; Venables, 1964; Yates, 1966*a*). Unfortunately, many of the studies were not capable of comparison by virtue of different research methods or differences in experimental displays. In addition, the attempt to relate differences in under-constancy and over-constancy of schizophrenics to degrees of chronicity or types of premorbid adjustment (e.g. Silverman) virtually leaves the question of determining processes unanswered. To some extent this deficiency applies to attempts to bring signal detection methodology to bear on the over-/under-constancy problem. While this method (Price & Eriksen, 1966) facilitates the assessment of perceptual sensitivity and the independent assessment of certainty of judgement, it basically only confirms that non-paranoid schizophrenics give unreliable and highly variable judgements with low degrees of certainty. Other experimenters, including the writer, have preferred to elicit maximum active participation from subjects in the response situation, i.e. to use the method of adjustment in preference to other psychophysical methods, and to search for the determinants of over- or under-constancy in perceptual/cognitive processes which developmentally are fundamental to the perception or logic of invariance.

The starting point of the present study is peculiar in that the same investigator obtained diametrically opposed findings, i.e. both over- and under-constancy from groups of poor premorbid, process, male, non-paranoid schizophrenics (Hamilton, 1963; Hamilton & Phillips, 1963). In an attempt to account for the discrepancies, the relationship between intelligence and size constancy was initially re-examined

(Hamilton, 1966*b*). It was found that the relationship was curvilinear, and that under-constancy as well as over-constancy may be found with low intelligence. Similarities in the size constancy means of adult schizophrenics and subnormal children (Hamilton, 1965) then suggested an investigation of the primitive cognitive skills of schizophrenics. The results of this study indicated that a schizophrenic group, as defined above, showed considerable deficits in skills such as conservation which are usually well established in 12-year-old normal children (Hamilton, 1966*a*).

These subsidiary experiments seemed to provide new leads to account for the findings of both under- and over-constancy in poor premorbid, process schizophrenics. If schizophrenics exhibit deficits in primitive cognitive operations (e.g. conservation) which in children are associated with less-than-optimal size constancy, and if we accept the experiments of Piaget and his associates (e.g. Piaget & Lambercier, 1943; Lambercier, 1946) on the so-called centration effect which had shown that a standard comparison object may be overestimated by children whether it is nearer to or further from the observer than the variable object, then it may be plausible to suggest that size constancy abnormalities are substantially determined by abnormalities in basic cognitive skills and strategies such as conservation and centration.

The analogy between constancy and conservation has also been argued by Smedslund (1965), but he may have over-stressed the possible identity as distinct from the interrelatedness of the operations involved. Nevertheless, since in conservation as well as in size constancy the task requirements include the assessment of object invariance in the presence of misleading and inappropriate perceptual cues, it could be predicted that subjects with very low or absent conservation skills might also give primitive constancy responses, i.e. overestimate standard objects and base their settings of a variable on this overestimation. Since discrepant size constancy data for schizophrenics have been obtained in studies which have not always controlled for the respective positions in depth of standard and variable objects, a systematic study of the position effect seems to be required. It would be unwise to assume that a centration effect operates for all positions of a standard in adult schizophrenics simply because it does so for perceptually less sophisticated children.

The implications of deficient primitive cognitive skills in the development of schizophrenic disorders have been argued more extensively elsewhere (Hamilton, 1966*a*). Conservation, as a logical operation, appears in the life of a child at a time when he is most sensitive to the socializing demands of the family. If, for a variety of reasons, he should develop excessive anxiety in interactions with the family and particularly the mother, he might respond with behavioural withdrawal in a number of areas of functioning and thus be predisposed to test his logical inferences subjectively rather than objectively. In this event, the conserving skill may be only insecurely or partially established. In the absence of longitudinal studies it is not possible to say, however, whether inadequacies of conserving in schizophrenic adults are the result of the schizophrenic condition, i.e. cognitive regression, whether these deficits have always been present in poor premorbid cases, or whether both explanations apply.

The object of this investigation was therefore threefold: (1) to test for size constancy with apparatus permitting random variation in the respective depth positions of standard and variable objects; (2) to test a new sample of schizophrenic subjects for

adequacy of early cognitive skills; and (3) to test the prediction that over-constancy in poor premorbid non-paranoid schizophrenics is not an indication of paradoxical normality, but the result of low levels of primitive cognitive operations.

Method

Subjects

These were a group of poor premorbid, non-paranoid chronic schizophrenics ($n = 24$), all hospitalized for more than 2 years (mean 12·6, S.D. 6·7, range 2–27). The original subdiagnoses were: 5 catatonics, 3 hebephrenics, and 16 simple schizophrenics. Only cooperative patients with 6/6 uncorrected or corrected visual acuity and yielding a mean IQ of close to 100 were used. They were assessed for social activity level and withdrawal characteristics, and an objective rating of their clinical status was carried out by the ward doctor. All patients had been regularly medicated for a number of years with substantial doses of phenothiazines, and no attempt was made to withdraw the treatments for the experimental period which extended for each subject over more than 2 weeks. The non-schizophrenic control group ($n = 12$) was selected on a volunteer basis from the non-professional staff of the same hospital with the overriding aim of matching the two groups closely for age and intelligence. The schizophrenics were assessed under group test conditions with the Mill Hill Vocabulary and the Progressive Matrices, while estimates of IQs on the latter for the non-schizophrenics were obtained from available occupational norms (Vernon, 1949).

The age and intelligence parameters were as follows. Age: schizophrenics, mean 40·5, S.D. 5·1, range 30–51; non-schizophrenics, mean 35·8, S.D. 10·1, range 22–49; Progressive Matrices IQ: schizophrenics, mean 97·6, S.D. 13·6, range 70–122; non-schizophrenics, mean 98·2, S.D. 7·7, range 85–113.

Apparatus

(*a*) *Size constancy*. The apparatus consisted of a matt black table surface 4·00 m long, with slots at distances of 0·75 m and 3·70 m for the display, the elevation and the lowering of variable and standard objects. Size constancy estimates were obtained for a white Rod 12·8 mm diameter, and a white square in the diagonal orientation, subsequently referred to as the Diamond. The variable stimuli were electrically driven within the range of 0–300 mm and were operated by subjects by the movement of a toggle switch. Subjects' visual orientation was standardized by the use of a chin rest. For half the number of trials the standard stimulus was in the near position, for the other half it was positioned at the far end of the display, with the position of the variable stimuli appropriately distributed between the far and near positions. Size estimates for Rod and Diamond were obtained in separate experimental sessions, counterbalancing for order of objects. There was a 5° horizontal angular separation between standard and variable, with an additional vertical angular separation between the Diamond objects to allow the lower part of the distant stimulus to project above the upper edge of the near object. For each of the two stimuli a randomized sequence of positions of the standard (near or far) and starting points of the variable (high or low) was prepared. These sequences were introduced by the experimenter from a central control panel which permitted all operations to be carried out concerning switching between the positions of standard and variable, selection of high and low starting points of variables, accurate settings of standard heights and the reading off of subjects' estimates.

The control panel counter readings of standard and variable sizes yielded an accuracy of ± 0·50 mm compared with manual measurement. At the end of the series of constancy estimates for each standard object, subjects were asked to make additional and independent estimates of the size of each standard in the far and the near position. For this purpose, the original experimental variables were removed from the display and subjects were given new variables, held in their two hands, which permitted settings in the range of 0–300 mm of a white flexible steel ruler and an extendable cardboard diamond shape, respectively, for the two objects. They were then asked to indicate how big the standards were with which they had been working. The Diamond and Rod were each displayed twice in the near and the far standard position. These data were used (i) to determine the presence or absence of a centration effect, i.e. over- or underestimation of a near or a far standard, and (ii) to pinpoint subjects whose size estimates in the first part of

the study varied systematically with their second, independent size estimate of the appropriate standard. It was therefore possible to relate over- or underestimation of a standard to type of constancy (over- or under-), and to relate consistency of size constancy performance, as affected by centration, to performance level of other cognitive skills.

(*b*) *Cognitive skill.* This was assessed in the areas of seriation, conservation and awareness of the laws of perspective.

Seriation. The seriation skill was elicited in a situation requiring the ordering in size of 10 coloured sticks with base 1 cm square, ranging from 1 to 10 cm in length. The sequence from largest to smallest stick had to be indicated either verbally or by pointing, or by both responses, before subjects were invited to place the objects on a board with a straight horizontal bar which provided a base level for the series. To obtain the maximum score, anticipation and construction of the series had to be correct without overt trials and *post hoc* corrections.

Conservation. This skill was assessed with respect to number, substance and quantity. To test for conservation of number two heaps of dried beans, 12 in each heap, were counted out, and with subjects instructed to observe closely, the experimenter arranged the beans of each heap in a row, the distance between beans being 1 in. in one row and 3 in. in the other. To obtain the maximum score, subjects were required to give an unqualified equality response to the question 'Are there the same number of beans in each row, or does one row have more (fewer) than the other?' (The order of questions was counterbalanced.)

In the assessment of conservation of quantity or volume subjects were shown two glass beakers filled with dried beans and told that they contained the same amount. (They were invited to check and re-equalize.) The contents of the first and subsequently the second beaker were then distributed into other containers varying in height, diameter and number in a standard procedure. To obtain the maximum score, subjects had to state unequivocally that the amounts being compared were equal. Conservation of substance was assessed with balls of plasticine as described in many other reports, changing the shape first of one and then the other ball. As before, a maximum score and evidence of the conserving skill depended on subjects' unqualified equality response when comparing the amounts of substance.

Knowledge of perspective. Awareness of the parameters operating in two-dimensional perspective and the ability to use them, depends on the seriation skill, on the availability of conserving operations and partially on general knowledge and experience. For the present purpose subjects were presented with a perspective drawing of a mansion '. . . where a long driveway leads from the gate to the house . . .' as well as three sets of eight cardboard cut-outs, shaped and painted to represent trees which had to be placed '. . . at the eight points marked with a cross along the driveway to the house . . .'. The trees were the same size within each set, and the sets differed in size in proportion to the perspective angle representing the driveway in the drawing. Subjects received no further instruction, and were thus free to use the same size trees for all positions, or any combination of sizes in distance-reducing series, placed in the true vertical position or perpendicular to the line demarcating the driveway. To obtain maximum score for this task, subjects had to place the cut-outs in a true vertical, size-reducing series in correct sequence.

The cognitive skill tasks were scored on a three-point scale: 1 for an incorrect response showing no knowledge of the general principle, 2 for a partially correct response, and 3 for a response indicating knowledge and application of the general principles. Two further scores were developed from these five test situations: the sum of conservation scores and an average of all five scores.

Design

The two size constancy stimuli employed in the study – the Rod and the Diamond – were combined in a split-split-plot nested design on which an analysis of variance was performed, followed by individual t tests. Due to the unequal n's in the experimental groups, the replication factor contained an unequal number of levels for each combination of levels of the factors in which it was nested. Since the number of levels was proportional, however, the design was balanced. Separate analyses of variance were performed on means and intra-subject S.D.s. The design contained the following sources of variance: two groups, two comparison objects, two positions of the standard objects and two starting points of the variable stimulus objects.

Schizophrenic subjects gave 48 responses to all conditions of size judgements for the two comparison objects; the control group (hospital employees) was available only long enough to give a total of 24 responses.

Careful instructions were given, particularly to the schizophrenic subjects, to indicate that objective size and not apparent or perspective size matches were required. Practice trials and unlimited time were given to ensure that the task requirements were fully understood. The psychophysical method employed was the method of adjustment, counterbalancing for starting position of the variable stimulus, and position of standard.

Results

Size constancy

The analysis of variance on the means showed the following factors and interactions to be significant with d.f.s of 1 and 34; starting positions of variables ($F = 129{\cdot}449; P < 0{\cdot}001$); positions of standards ($F = 11{\cdot}573; P < 0{\cdot}01$); stimulus objects ($F = 8{\cdot}764; P < 0{\cdot}01$); groups × stimulus objects ($F = 10{\cdot}322; P < 0{\cdot}01$); starting positions of variables × stimulus objects ($F = 34{\cdot}929; P < 0{\cdot}001$); positions of standards × stimulus objects ($F = 95{\cdot}303; P < 0{\cdot}001$); starting positions of variables × positions of standards × stimulus objects ($F = 49{\cdot}765; P < 0{\cdot}001$). The interaction between groups × positions of standards fell short of significance ($F = 3{\cdot}420$; $0{\cdot}10 < P > 0{\cdot}05$). The examination of the mean values of individual factors indicates that the two experimental groups performed similarly when the stimulus object was the Rod, but differently with the Diamond object, particularly when the Diamond standard stimulus was in the near position. Individual t tests carried out on estimates of the Rod by the two experimental groups were non-significant for all conditions varying position of standard and starting position of variable. For the Diamond stimulus the following significant differences were obtained: position of standard near, variable starting high, $t = 3{\cdot}503$ ($P < 0{\cdot}01$); position of standard near, variable starting low, $t = 3{\cdot}197$ ($P < 0{\cdot}01$); position of standard near, with pooled starting positions of variable, $t = 3{\cdot}776$ ($P < 0{\cdot}001$). In each case the schizophrenic means were greater.

The second analysis of variance performed on intra-subject S.D.s yielded two significant results: groups ($F = 7{\cdot}154; P < 0{\cdot}05$) and positions of standard × stimulus objects ($F = 6{\cdot}569; P < 0{\cdot}05$). The interactions between groups and stimulus objects, positions of standards and starting positions of variables were not significant, although for all conditions the variability of individual schizophrenic subjects was greater than the variability of normals. The differences in intra-individual variability were significant in the following conditions: Rod – standard near, starting position

Table 1. *Distribution of over- and under-constant size estimates* (± 10 *per cent standard*)

(Percentage and frequencies for each position of standard are out of 48 and 24 mean estimates for schizophrenics and non-schizophrenics derived from two starting positions of the variables.)

		Far				Near			
		Under-constant		Over-constant		Under-constant		Over-constant	
Stimulus object	Standard	*n*	%	*n*	%	*n*	%	*n*	%
Rod	Schizophrenics	0	0	26	54·2	2	4·2	14	29·2
	Non-schizophrenics	0	0	18	75·0	0	0	7	29·2
Diamond	Schizophrenics	8	16·7	6	12·5	21	43·8	2	4·2
	Non-schizophrenics	2	8·3	2	8·3	2	8·3	4	16·7

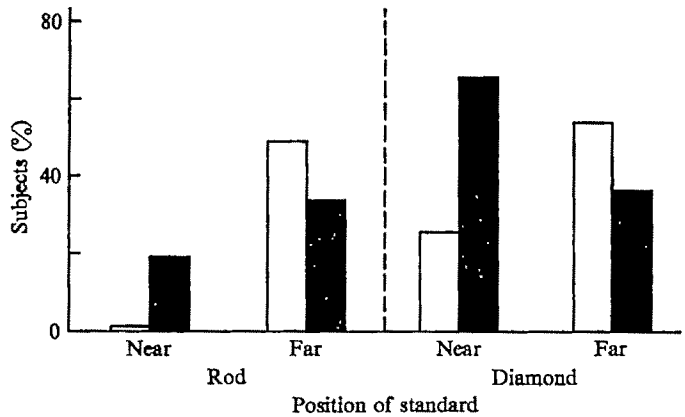

Fig. 1. Matched overestimation of standard and variable. □, Non-schizophrenics; ■, schizophrenics.

of variable high ($t = 2{\cdot}508$; $P < 0{\cdot}02$); standard near, starting position of variable low ($t = 2{\cdot}860$; $P < 0{\cdot}01$); standard near, starting positions of variable pooled ($t = 2{\cdot}774$; $P < 0{\cdot}01$); Diamond – standard near, starting positions of variable low ($t = 2{\cdot}541$; $P < 0{\cdot}02$).

Considering the data shown in Table 1, from the point of view of over- and under-constancy, the Rod stimulus generally yielded over-constant estimates for both experimental groups. This unexpected result will be discussed further below. It had the practical implication of preventing a comparison on other parameters of schizophrenics respectively over- and under-constant with this stimulus because of the very small number of under-constant schizophrenics. For the Diamond stimulus, the pooled schizophrenic means were under-constant, and significantly so compared with normals when the standard was near, as in a previous study (Hamilton, 1963). It appears that over-constant responses in schizophrenics occurred with greatest frequency when the standard was in the far position.

Estimation of standard

If the most usual response of normal subjects of average intelligence is over-constancy, i.e. over-correcting for distance (Hamilton, 1966*b*), this has two implications. One, a far standard will be more overestimated than a near one, and secondly and concomitantly a near variable will be set higher than a far one. For populations habitually tending towards under-constancy, a far standard will be more under-estimated than a near one, and the near variable will be set lower than a far one. This relationship was here confirmed with respect to settings of the variables in the constancy experiment proper when the stimulus objects were Diamonds. But in comparing the additional estimates of the standards for schizophrenics and normals it was found that schizophrenics made significantly lower estimates of the Rod standard either far or near ($U = 86$, $P < 0{\cdot}05$ and $U = 92$, P $0{\cdot}05$ respectively), and non-significantly higher estimates of the Diamond standard in its two positions. This absence of clear trends was, of course, reflected in the non-significant interactions for groups × positions of standards. The centration hypothesis, however, can be tested

most adequately only by examining the *matched* estimates of size, i.e. matches between overestimates of standards and the concomitant overestimates of the variables. If a centration effect operates for schizophrenics, who are normally under-constant, then the incidence of matched overestimation of standard and variable should be greater for them than for a non-schizophrenic group and particularly so when the standard is near. If, on the other hand, some schizophrenics show matched overestimation of standard and variable when the standard occupies the more distant position of two comparison objects, this would confirm and supply a reason for findings of over-constancy from this type of population. Such subjects may then be said to be more prone than other schizophrenics to the centration effect because they are apparently subject to it even when the stimulus display discourages it. They may further be adjudged as functioning perceptually and cognitively as primitively as the young children in the Geneva researches.

Fig. 1 indicates that a substantial centration effect is present in the responses of schizophrenics particularly when the standards are near, but also when they are far. The matched overestimation of standards and variables for the normal group with a far standard was, of course, expected on the basis of the established over-constancy bias of a normal population of average intelligence. The demonstrated effect is non-significant with respect to the Rod stimuli, and just falls short of significance when the stimuli are Diamonds ($\chi^2 = 3{\cdot}718$; $0{\cdot}10 < P > 0{\cdot}05$). Four schizophrenics were over-constant on the Diamond for both positions of the standard (including two who did not quite reach the objective criterion of ± 10 per cent standard size). Three of these subjects showed the anticipated matched overestimation of the distant standard and the near variable.

Cognitive skills

The previous data from a non-schizophrenic population on conservation, seriation and knowledge of perspective were obtained from children and not adults (Hamilton, 1966*a*), and it has been possible to suggest that the significant deficits of the schizophrenics could be partly determined by an adult subject's doubt that something else besides a logically simple answer to the questions might be required. In the present experiment, therefore, the first statistical comparison was between the logical and perceptual skill levels of the whole schizophrenic and the normal sample. The statistical comparisons of the performance scores (obtained as described above) showed that the normal group was significantly superior to the schizophrenics on five of the seven test and scoring categories: conservation of substance and conservation of quantity (in each case $U = 84$; $P < 0{\cdot}025$), total conservation score (number plus substance plus quantity) and knowledge of perspective ($U = 66, P < 0{\cdot}01$; $U = 60, P < 0{\cdot}01$ respectively) and mean skill score ($U = 16$; $P < 0{\cdot}001$). The differences between the groups for conservation of number and for seriation were not significant. The results are shown graphically in Fig. 2, and were obtained, as indicated above, from groups not significantly different on Progressive Matrices IQ.

Six of the cognitive skill parameters had significant negative correlations with mean size constancy estimate and intra-individual variability of the Diamond stimulus. These were conservation of quantity and of number, total conservation score, seriation, knowledge of perspective and mean skill score. Most of these correlations remained significant when partialling out intelligence.

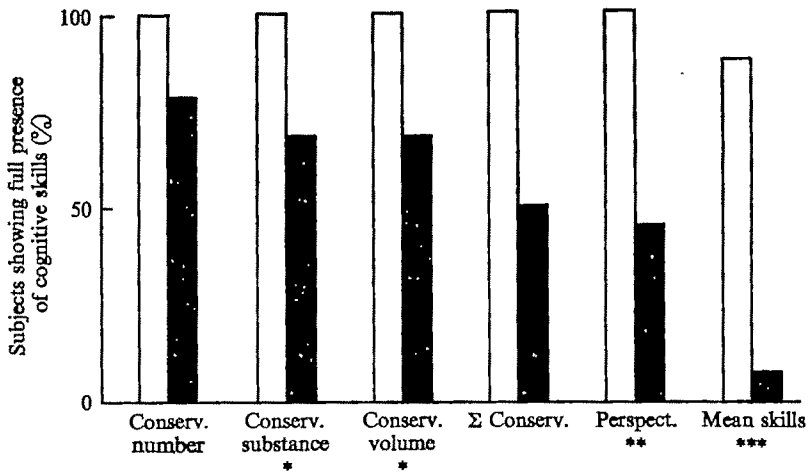

Fig. 2. Performance of schizophrenics (■, $n = 24$) and non-schizophrenics (□, $n = 12$) on primitive cognitive skills. * $P < 0{\cdot}025$; ** $P < 0{\cdot}01$; *** $P < 0{\cdot}001$ (Mann–Whitney; non-schizophrenic > on performance scores).

Over- and under-constancy were defined in this and previous studies as estimates which were plus or minus 10 per cent of the size of the standard stimulus. In considering the relationship between constancy for Rod and Diamond and cognitive skill level, no dichotomies of estimates could be obtained from performances with the Rod stimuli, since only two schizophrenic subjects gave under-constant means with the Rod. With respect to the Diamond stimulus only four schizophrenic subjects gave mean size estimates in the direction of over-constancy with the standard near, so that the objective criterion of over-constancy had to be adjusted in order to achieve an over-constant comparison group. (This was done by extending the criterion of over-constancy to all those estimates of the variable which were smaller than the near standard.) In the following findings, the significant differences between two subgroups of 15 under-constant and eight over-constant schizophrenic subjects were obtained from the pooled results from trials with the Diamond standard in the near as well as the far position. They show that under-constant schizophrenics were superior on the seven cognitive skill parameters compared with the over-constant subjects in 11 out of 14 comparisons, four of which were statistically reliable, using Fisher's exact probability test. These were: conservation of substance ($P = 0{\cdot}026$), total conservation score ($P = 0{\cdot}05$), knowledge of perspective ($P = 0{\cdot}05$) and mean skill score ($P = 0{\cdot}05$).

Additional significant differences between the two schizophrenic subgroups differing in size constancy performance were found for Progressive Matrices IQ, clinical rating, chronicity and activity/withdrawal, with over-constant schizophrenics giving the more adverse scores.

Discussion

The experiments confirmed previous findings of the same investigator that under-constant as well as over-constant size estimates may be obtained from poor premorbid, non-paranoid, chronic schizophrenic men. The data further confirm the assumption that schizophrenics and normals differ in their size estimates of a distant compared with a near standard, at least in respect of one stimulus object. It was also shown that over-constant schizophrenics almost consistently overestimate the size of a distant standard compared with under-constant schizophrenics, while both schizophrenic subgroups overestimate a near standard and make appropriate matching responses with settings of the variables. The statistical tests fall short of significance at times and the comparison groups are rather small, but the anticipated contribution of the centration effect could be tested additionally by the parallel investigation of cognitive skill levels, since these also reflect primitive cognitive functioning. As indicated in the result, over-constant schizophrenics are considerably inferior to under-constant schizophrenics on the cognitive skills tested here.

These results are inconsistent with Silverman's suggestion (1964*a*) that process, chronic schizophrenics should be characterized by extensive scanning. They also yield a more complex differential behaviour pattern than suggested by him in a later publication (Silverman, 1967). Apart from the absence of a demonstrated relationship for schizophrenics between size constancy (as distinct from size estimation) and direct measures of eye movement, care must be taken to distinguish between centration or minimal scanning in relation to a standard, and to a variable, and a standard in the near or far position. Basically, the centration hypothesis refers only to the perception of standards, and in view of the data presented here, it is not possible to relate in a simple manner over-constancy to paranoid, and under-constancy to non-paranoid schizophrenia, or to the reactive/process dimension. Process, non-paranoid schizophrenics may show either under- or over-constancy, depending apparently at least in part on the functional level of primitive cognitive operations, where an associated role of the scanning strategy remains to be demonstrated. A further complication arises from McKinnon & Singer's finding (1969) that medicated patients show lower eye movement rates. (In the experiment reported here, the over- and under-constant schizophrenic subgroups were both medicated with maintenance doses.)

If cognitive style parameters are to be considered as contributing to individual differences in size constancy, field articulation (Gardner, 1961; Gardner & Long, 1962) seems more promising than the less complex processes underlying scanning. (This lack of complexity has been partly rectified by Wachtel (1967), but his re-analysis of scanning has not yet been applied to schizophrenic perception.) While Weckowicz & Blewett (1959) found no significant correlations between field articulation as measured by the Gottschaldt Embedded Figures Test and size constancy estimates, Silverman (1964*b*) reports non-paranoid schizophrenics impaired on this variable. A group paper-and-pencil version of the Gottschaldt Test formed part of the present investigation. As in Weckowicz & Blewett's study, no significant correlations with size constancy were obtained, but the variable distinguished reliably between the over- and under-constant schizophrenic subgroups, with the former performing worse. The high correlation of the Gottschaldt Test with intelligence and

with the cognitive skill parameters – which are themselves significantly covariant with intelligence – and over-constant schizophrenics' ability to over-isolate a single stimulus in a perceptual field by centring, which they are unable to do in the embedded figures situation, make it difficult to pinpoint the special function of field articulation in size constancy operations. A possible explanation, to be tested in future experiments, might consider the possibility that some of Wachtel's scanning subcategories, rather than Gardner's undifferentiated scanning cognitive control, interact with field articulation parameters, so that the factorial independence of field articulation and scanning demonstrated by Gardner may be partially artifactual, at least for schizophrenics.

The surfeit of over-constant responses obtained when the stimulus was the Rod suggests that the display characteristics may have encouraged subjects to adopt a partially perspective method of judgement. This, according to Carlson (1960), usually leads to over-constancy. On the other hand, it is likely, though remaining to be established, that a Rod and a Diamond in a size constancy task make differential demands on information integration and processing. In the light of recent reports (Court & Garwoli, 1968; Yates, 1966*b*), schizophrenics find it more difficult to process the information in perceptual complexity than simpler information. It is possible that the comparison of Diamond stimuli here presented with horizontal as well as vertical angular separation may be a more complex task than the comparison of Rod stimuli, and thus elicits more primitive methods of processing.

It is still unclear which of the two demonstrated behaviours is cognitively more primitive: estimates in the direction of retinal size yielding under-constancy, or estimates affected by centration yielding over-constancy. On balance, it is suggested that with a distant standard centration is more primitive because it ignores a larger number of relevant signals. Having established, however, that *quasi*-normal constancy estimates are obtainable from an experimental population which usually yields under-constant responses, it will be necessary to use signal-detection methodology (instead of the method of adjustment) to establish whether sensory thresholds or criteria of judgements or both distinguish between the two constancy subgroups. This technique will then have to be pitted against the cognitive skill parameters advanced here to test their differential explanatory power, or to establish their possible interrelatedness. The conclusion that a centration effect may account for paradoxical over-constancy in schizophrenics additionally requires oculographic confirmation. It would become untenable if fixation and scanning favour a near, or for that matter a distant variable, rather than the standards in either position.

The impairment of developmentally early logical skills in schizophrenics has now been confirmed. This suggests that for the categories of schizophrenics represented here, if for none other, the acquisition of cognitive skills has been less secure and clear-cut than the acquisition of these skills by non-schizophrenics. If we accept that conservation is a conceptual process which imposes a considerable degree of permanence and constancy on a sensorially and perceptually variable environment, then deficiencies in conservation would diminish subjective certainty of environmental stability and invariance. If we accept reports that schizophrenics often perceive their environment as subjectively fluid and changeable, as for example in episodic delusions or hallucinations, the possibility arises that these abnormalities of cognitive behaviour

may be partly determined by temporary or permanent deficiencies in conserving operations. If, furthermore, we hold that the conserving skill depends not only on structural maturation but on learning in a favourable socialization setting (Hamilton, 1971), the establishment or re-establishment of the conserving skill in patients by appropriate training techniques might conceivably modify some of the more obvious cognitive disorders of the schizophrenias.

The investigation was facilitated through the generous assistance of the staff of Fairmile Hospital, Wallingford, Berks. Dr D. H. Taylor helped with the calibration of the apparatus and developed the randomized series of stimulus presentations, Messrs G. Jury and C. Murphin designed and built the apparatus. The statistical computations were carried out by the Institute of Behavioural Research, York University, Toronto, with the help of a grant from the Research Grant Committee under the direction of Paul Herzberg, using the computer program 'Balanova 5' devised by him for a general analysis of variance.

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(*Manuscript received* 4 *March* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 85–87
Printed in Great Britain

FIELD DEPENDENCE IN RELATION TO PAIN THRESHOLD

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It is known that pain-threshold values are considerably influenced by the stimulus scale-interval used in their assessment (Haslam, 1965, 1968; Haslam & Thomas, 1967). As predicted, a significant positive correlation was found between the amount by which a subject's pain threshold was lowered by a small interval-size and the extent of his field dependence as measured by the Embedded Figures Test (Witkin *et al.*, 1962).

When heat-pain threshold is assessed by means of the limiting method, threshold values are dependent upon the size of the increment in successive values of the variable stimulus (Haslam, 1965, 1968; Haslam & Thomas, 1967). For example, when the increment is as small as 8 mc./sec./cm^2 pain threshold is significantly lower than when the variable stimulus is increased in steps twice or four times as large as this.

It was suggested (Haslam, 1968) that an anchoring effect similar to that described by Helson (1947) might account for the low pain threshold values found when a small stimulus-interval is used. A further experiment was carried out (Haslam, 1970) which confirmed the hypothesis that an anchor in the form of repeated applications of the starting-point stimulus would have the effect of lowering the pain threshold. The present research was designed to determine the relationship, if any, between the influence of stimulus scale-interval and field dependence.

In the assessment of pain threshold, subjects are required to select from a series of stimuli one stimulus which gives rise to minimal pain. This sequence of stimuli may be regarded as the context in which the 'painful' stimulus is embedded. One aspect of this context, namely the stimulus scale-interval, has been found to influence pain threshold. Some people are more influenced by context than others, and this is known as 'field dependence' (Witkin *et al.*, 1962). It was thought, therefore, that field-dependent subjects would be more influenced than field-independent subjects by the stimulus scale-interval. The scale which progresses in steps of 8 mc./sec./cm^2 has been found to exert a greater influence upon pain threshold, in the sense of giving less reliable threshold values, than the scale which progresses in steps of 16 mc./sec./cm^2 (Haslam, 1965; Haslam & Thomas, 1967). On the basis of this argument, therefore, it was thought that the pain threshold of field-dependent subjects should be influenced to a greater extent than that of field-independent subjects by a scale which progressed in steps of 8 mc./sec./cm^2.

One way of assessing field dependence is by means of the Embedded Figures Test (EFT; Witkin *et al.*, 1962). The ease with which a person can see a given figure independently of the context in which it is presented is indicated by a time score. The prediction was made that the amount by which a subject's pain threshold was lowered by a small step-size would be positively correlated with his time score on the EFT.

METHOD

Subjects

Twenty psychology students served as subjects. Their mean age was 22·1 years, with a range of 18–38. They were naive as to the nature of the experiment, and they were not paid for their participation.

Apparatus and procedure

Pain threshold. Pain threshold was assessed by means of a modified Hardy–Wolff (Hardy *et al.*, 1952) radiant-heat apparatus in which the heat from a 500 W. bulb was concentrated by a lens through an aperture 2 cm in diameter. Mains power was supplied to the lamp through a Variac transformer and monitored by means of a voltmeter. The apparatus was calibrated by means of a Moll thermopile and millivoltmeter in terms of mc./sec./cm², and the calibration was checked before each experimental session. The forehead was exposed to the radiant-heat source for 3 sec. at 1 min. intervals, and the subject was asked to report when he felt a small stab of pain at the end of a 3 sec. exposure. Details of the procedure used are given elsewhere (Haslam, 1965).

The energy level of the starting-point stimulus was 98 mc./sec./cm², which the majority of subjects described as warm. Pain threshold was assessed twice for each subject, once using a step-size of 8 mc./sec./cm² and once using a step-size of 16 mc./sec./cm². The difference between these two threshold values was the score used to indicate the influence of stimulus scale-interval. This will be referred to as 'D'.

Embedded Figures Test. A short form of the EFT, consisting of the first 12 figures listed, was used, and the time limit for each figure was 3 min. (Witkin *et al.*, 1962, p. 40). A subject's score was the mean time taken for the 12 figures.

The two pain-threshold assessments and the EFT were carried out in a random order, and at least three days were allowed to elapse between the threshold assessments.

RESULTS AND DISCUSSION

With an interval-size of 16 mc./sec./cm² mean pain threshold for the whole group was 231 mc./sec./cm² and with an interval-size of 8 mc./sec./cm² it was 198 mc./sec./cm². A Wilcoxon matched-pairs signed-ranks test (Siegel, 1956) showed the difference between these two values to be statistically significant ($P < 0{\cdot}005$, one-tailed). The mean number of stimulus presentations under the large step-size condition was 13·5 and under the small step-size condition it was 9·1. This difference is statistically significant (Wilcoxon matched-pairs signed-ranks test, $P < 0{\cdot}005$, one-tailed).

Table 1 shows for male and female subjects the mean value and range of the

Table 1. *Mean value and range of D score, EFT score and pain threshold for male and female subjects*

	Males ($n = 10$)	Females ($n = 10$)	Total ($n = 20$)
Mean D score	36	30	33
Range	9–56	9–81	9–81
Mean time (sec.) taken on EFT	25·80	35·70	30·75
Range	13·16–46·75	8·33–67·33	8·33–67·33
Mean pain threshold			
8 mc./sec./cm² intervals	208	188	198
Range	132–264	116–247	116–264
16 mc./sec./cm² intervals	244	218	231
Range	176–320	156–264	156–320

D score, of time taken on the EFT, and of pain threshold under both conditions (8 and 16 mc./sec./cm^2 interval-size). A Mann–Whitney U test failed to show a significant difference as between male and female subjects in respect of any of these measures.

The prediction that the amount by which a subject's pain threshold was lowered by a small step-size would be positively correlated with his time score on the EFT was confirmed in that a Spearman rank correlation of $+0{\cdot}60$ ($P < 0{\cdot}01$) was found between time scores for the EFT and D.

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(*Manuscript received* 16 *March* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 89–99
Printed in Great Britain

EFFECTS OF EXTERNAL STIMULATION ON THE DOMESTIC CHICK'S CAPACITY TO STAND AND WALK

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External conditions affecting the rate at which the domestic chick gets up on to its toes and walks in the hours immediately following hatching were investigated. Five groups of chicks were tested; four groups were kept after hatching under different conditions (with or without other chicks, under different light conditions and under different conditions of restriction), and one group was hatched under different conditions: each egg was stimulated artificially so that the chick hatched early. The results showed that the last group developed most slowly. The group reared in the light and with other chicks attained the normal chick posture most rapidly, and was followed next in sequence by chicks reared in the light but in isolation. Chicks kept in the incubator until the time of testing developed more slowly. It is concluded that external stimulation can contribute towards, or retard, the development of this behaviour.

Although there is a large body of information on locomotion in animals and the mechanisms underlying it (see, e.g., Gray, 1968) less is known about the initiation of this behaviour in animals, and the transition from embryonic to infant activity has received little attention in birds and mammals. However, in the domestic fowl, Kovach (1971) has recently discussed postnatal righting reflexes in terms of pre-hatching motility and postural reflexes and the suggestion has been made that maternal stimulation plays some part in early righting and standing behaviour in the precocial mammal (Herscher *et al.*, 1963; Altmann, 1963). In the human foetus Prechtl (1965) has shown an effect of abnormal prenatal postures on postnatal postural reflexes.

In precocial birds it is well known that the newly hatched young creep on their tarsi, their legs largely flexed, at first without raising their bodies from the ground, and that they gradually get up on to their toes and walk, a few hours after hatching. In the domestic chick, observation suggests that not only postural and stepping reflexes present at the time of hatching, but also the conditions of natural life, can play a part in the development of early walking; for example, chicks kept on a slippery floor tend to develop splayed-out legs; thus a normally rough surface to walk on presumably contributes towards the standing posture. Similarly, pre-hatching conditions would appear to contribute towards a normal standing posture; for example, in the bobwhite quail (*Colinus virginianus*) abnormally early hatching (mostly, but not always, associated with artificial stimulation) can result in a small minority of chicks whose legs splay out when they begin to stand, while very late hatching (often, but not necessarily, also associated with artificial stimulation) can result in crooked toes (Vince, unpublished). The foetus almost entirely fills the shell during the immediately pre-hatching period, the areas at which the skeleton is closest

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to, and forced against, the shell being not only the spine and major leg joints but the toes and the tarsal joints. At this time the legs and feet are flexed in all joints, thus it seems possible that there is a passive maintenance of the pre-hatching posture by the shell which has a lasting effect on the chick and contributes towards the way in which the limbs unfold after hatching, and the postural and stepping reflexes become effective. That time in the shell has an effect on the rate of postural development could be suggested by the finding (Vince & Chinn, 1971) that Japanese quail chicks whose hatching has been accelerated by external stimulation develop the capacity to stand on their toes and walk slightly later than unstimulated chicks.

Thus it would appear that although the initiation of standing and walking must develop mainly from the automatic mechanisms of locomotion (reflex activity guided by proprioceptive and vestibular feedback), this need not be exclusively so; immediate environmental factors including rate of development (time in the shell) appear to contribute towards the behaviour. Given the suggestions outlined above these environmental factors could be operating both before and after hatching.

In the experiment described below the effect of pre- and post-hatching stimulation on the rate at which the normal walking posture is attained have been investigated in the domestic chick in a preliminary attempt to outline this problem. In the present experiment post-hatching postures have been observed in chicks aged up to 20 hr.

METHOD

Materials

Eggs from a pure strain of Light Sussex were used throughout, and were stored for not more than 5 days before incubation began. They were incubated in batches in a 'Westernette' forced draught incubator run at 37·8° C. The eggs were removed from the Westernette 48 hr. before the expected time of hatching (i.e. at 19 days of incubation), candled, and viable foetuses were transferred to 'Curfew' observation incubators kept at the same temperature. The incubators had a black cover, which kept the interior in semi-darkness. While in the Curfew incubator eggs were incubated singly and (except for group 3 below) in cylinders of perforated zinc about 2½ in. in diameter, so that the chicks could be identified and their movements restricted. Chicks in group 3 were kept in net bags about 4 × 7 in. when flat, but drawn up at the top. These bags allowed the chicks to hatch normally, but then not only prevented them from moving freely round the incubator, but prevented them from standing up (inside the cylinders chicks often stand leaning against the side). All visual stimulation was excluded from group 6 by hatching and keeping them in total darkness, inside the incubator, until they were removed for testing. Eggs in group 5 (see below) were stimulated from 48 hr. before the expected time of hatching by being placed in a cradle attached to the spindle of an 'Advance' vibration generator. The cradle was arranged so that the egg rested a few millimetres above the level of the normal egg tray. Square waves were fed from an oscillator into the vibrator at a rate of 1½ Hz, equivalent to three

Table 1. *Pre- and post-hatching conditions for experimental chick groups*

Group	Pre-hatching conditions	Post-hatching conditions
1	Isolation (in cylinder)	In brooder with other chicks
2	Isolation (in cylinder)	In brooder alone
3	Isolation (in bag)	Isolation in incubator (in bag)
4	Isolation (in cylinder)	Isolation in incubator (in cylinder)
5	Isolation (in cylinder) Stimulated and accelerated	Isolation in incubator (in cylinder)
6	Isolation in darkened incubator in cylinder)	Isolation in darkened incubator (in cylinder)

clicks/sec. Thus these eggs were stimulated at a rate and amplitude known to accelerate hatching (Vince *et al.*, 1970). The hatching of all eggs was timed automatically (Salter & Vince, 1966).

Chicks were divided into six groups (Table 1), in three of which (groups 1, 2 and 4) pre-hatching conditions were the same (eggs were hatched in isolation from each other, and without stimulation), while post-hatching conditions were varied by giving different groups different amounts of stimulation. Thus group 1, given the most stimulation, was kept for at least 2 hr. before testing in a brooder, in the light, and with other chicks. Group 2, given a medium amount of stimulation, was kept for at least 2 hr. before testing in the same brooder, and in the light, but alone, and group 4, again given less stimulation, was kept until testing in the semi-darkness of the incubator, inside the cylinder of perforated zinc. Groups 3 and 4 were hatched under similar conditions (in isolation) but kept after hatching under different degrees of restriction, although both groups were kept in isolation and in the incubator; group 4 within the perforated zinc cylinder, and group 3 in the net bag. In two groups (4 and 5) post-hatching conditions were the same (isolation, in the incubator) while pre-hatching conditions were varied by giving the accelerating type of stimulation to group 5, and no stimulation to group 4. Group 6 were incubated, and kept after hatching until being tested, in total darkness. The brooders were kept at approximately 35° C, by overhead infra-red lamps.

Table 1 shows the pre- and post-hatching conditions for each group.

Each chick was tested once only, singly, in the same way and at an age between 0 and 20 hr. from the time of hatching. For testing, the chick was placed for 5 min. in a box about 10 × 16 in. lined with rough kitchen paper and warmed by a 60 W lamp. As all chicks of this post-hatch age rest in the same position (flat on the ground) each one was stimulated in two ways in order to assess its capabilities when (1) standing and (2) moving. Also when manipulated on the back and sides all chicks responded by struggling movements which brought them up on to their toes in a rather exaggerated vertical position, unless they were exceedingly immature. Thus the chicks were first manipulated in this way and their standing postures assessed, they were then left unsupported, and stimulated by the experimenter calling and tapping, until each one moved along the ground, or for 5 min. if the chick failed to move, and the mode of locomotion was observed. The chick was then placed in one of seven categories according to its level of maturity. The categories were: 0, chick flat on floor, unable to move; 1, able to lurch its body momentarily off the floor, but not to support itself on its toes when moving; 2, just able to lurch on to the toes when moving; 3, able to take a few steps on the toes before collapsing; 4, able to walk on the toes, but with legs very much flexed; 5, able to walk in the normal position, but wobbly; and 6, able to stand and walk in the normal, steady and upright posture. Thus chicks in categories 1 and 2 were able to move about, but only resting on the tarsal joints. Latencies (time between stimulation and response) were not recorded in this experiment.

Results

All chicks tested attained normal postures within a few days from the time of hatching.

The results are summarized in Fig. 1, where the individual scores for each group are divided into 2-hr. blocks according to age and the mean level of maturity is plotted within each block. Fig. 1 suggests that the rate of post-hatching development varies both with the pre- and post-hatching conditions in which the individual is kept. A comparison between groups 4 and 5, where the post-hatching conditions were the same and the pre-hatching conditions varied, shows that the rate of postural development after hatching can be changed by stimulating the foetus in such a way that its rate of pre-hatching development and hatching time are advanced. Again, a comparison between group 4 and groups 1 and 2 shows that post-hatching conditions can affect the chick's rate of postural development, when conditions of incubation are kept constant. The rate of postural development of groups 1 and 2 also differed, showing an effect of contact with other chicks, when both groups were in the light. Again, the level of development in groups 2 and 4 differed, showing an

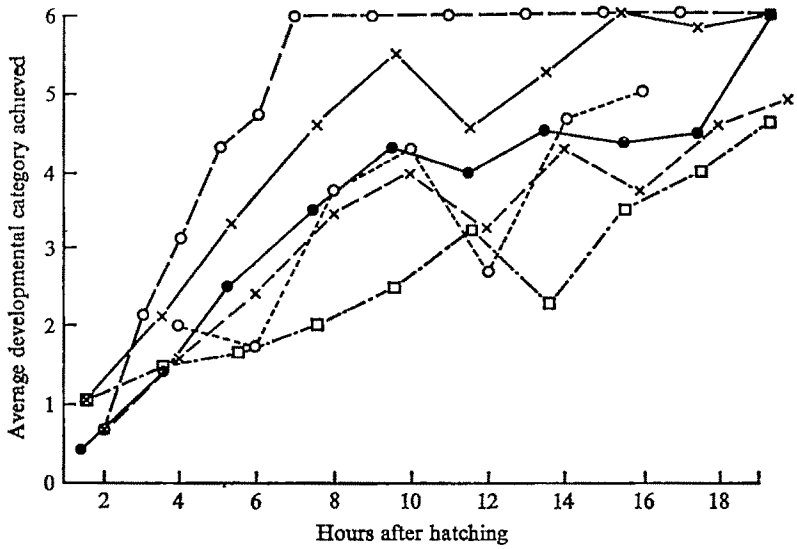

Fig. 1. Comparison between groups 1–6; mean levels of maturity within each group, averaged over 2 hr. blocks. ○——○, group 1: chicks with at least 2 hr. experience in a brooder with other chicks; ×—×, group 2: chicks with at least 2 hr. experience in a brooder, but alone; ○- - -○, group 3: chicks hatched in a net bag and kept in the incubator; ●—●, group 4: chicks hatched in 3 in. containers and kept in the incubator; □- -□, group 5: stimulated chicks which had hatched early; ×- - -×, group 6: chicks hatched and kept until testing in total darkness.

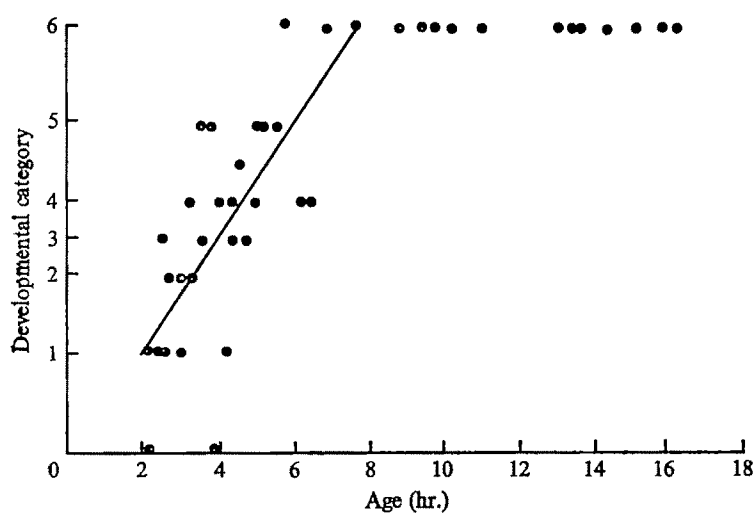

Fig. 2. Individual scores, showing level of development achieved at different ages from the time of hatching, and with regression line fitted. Non-uniform scale due to transformation of data (see text). Group 1: chicks with at least 2 hr. experience in the light, and in a brooder with other chicks.

effect of removal from the semi-darkness of the incubator, into the light (there was also a slight drop in temperature here for group 2). There was very little difference between groups 3 and 4, where the post-hatching conditions within the incubator were varied, although a comparison between the means suggested that the more

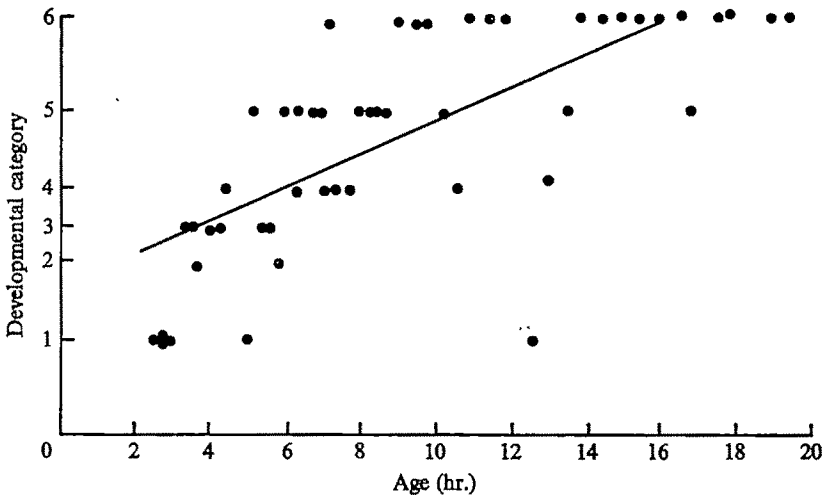

Fig. 3. Individual scores with regression line fitted. Non-uniform scale (see text). Group 2: chicks with at least 2 hr. experience in the light, in the brooder, but alone.

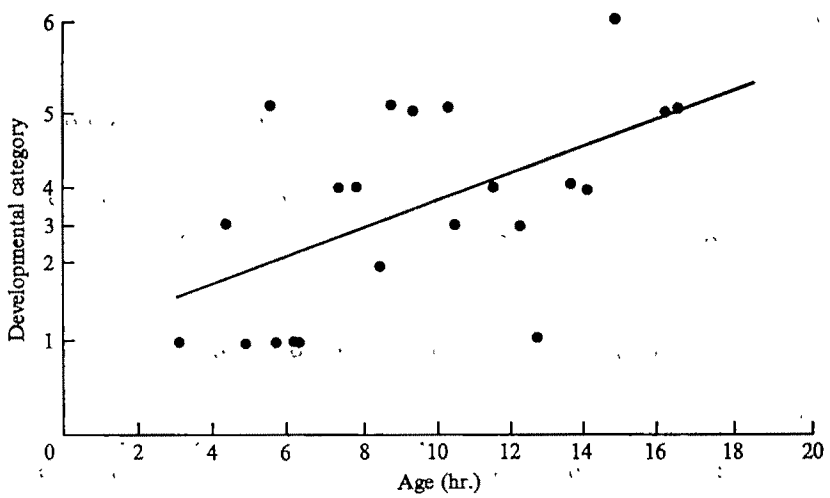

Fig. 4. Individual scores with regression line fitted. Non-uniform scale (see text). Group 3: chicks hatched and kept in net bag in incubator until the time of testing.

restricted chicks were developing more slowly than the less restricted; and there was very little difference between groups 3 and 4 and group 6, although the chicks hatched and kept in total darkness developed at a slightly slower rate than did those hatched and kept in the normal semi-darkness of the 'Curfew' observation incubator. Thus the group which developed the capacity to stand normally with the greatest speed

was the group kept under the most stimulating conditions – in a brooder with other chicks, and in the light. The next in rate of development was that where chicks were kept singly in the light, and the slowest, immediately comparable, group was that

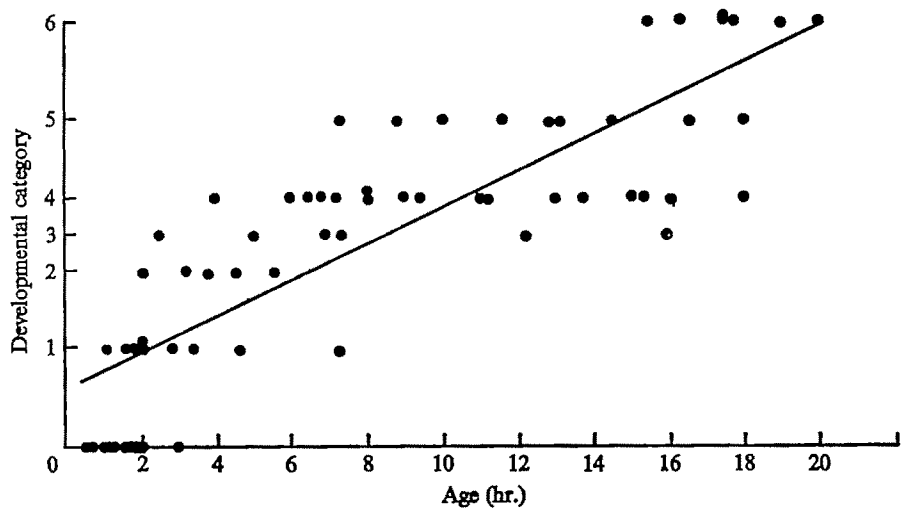

Fig. 5. Individual scores with regression line fitted. Non-uniform scale (see text). Group 4: chicks hatched in 3 in. cylinders and taken straight from the incubator for testing.

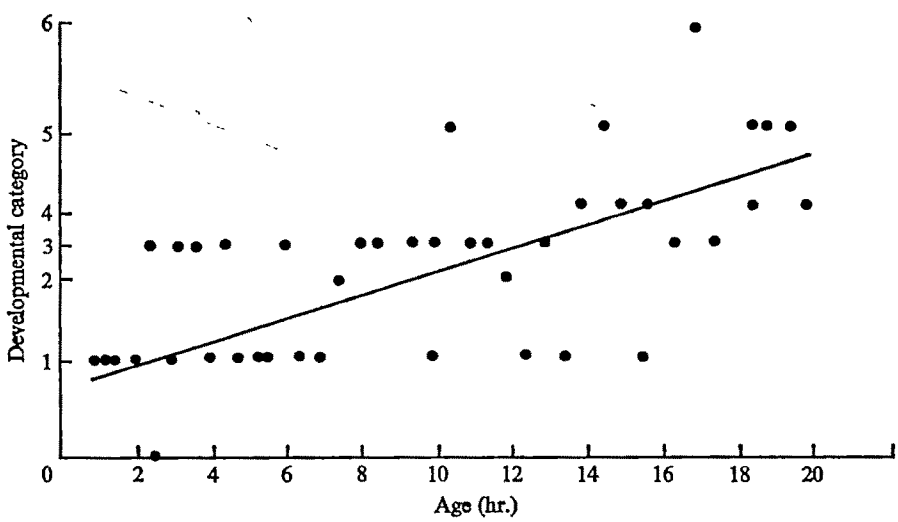

Fig. 6. Individual scores with regression line fitted. Non-uniform scale (see text). Group 5: chicks hatched early as a result of stimulation and kept in incubator until the time of testing.

kept until testing in the semi-darkness of the incubator. However, the group hatched and kept in total darkness developed even more slowly than these. The group which developed most slowly of all and, within the age limit, to a lower level was that (group 5) which hatched early, as a result of pre-hatching stimulation.

Individual scores for each group are plotted in Figs. 2–7. In these figures the scale giving levels of development is non-uniform due to transformations (Bartlett, 1947) which were effected to obtain data with a constant variance. (A constant variance is a necessary condition together with linearity for the standard linear regression analysis undertaken.)

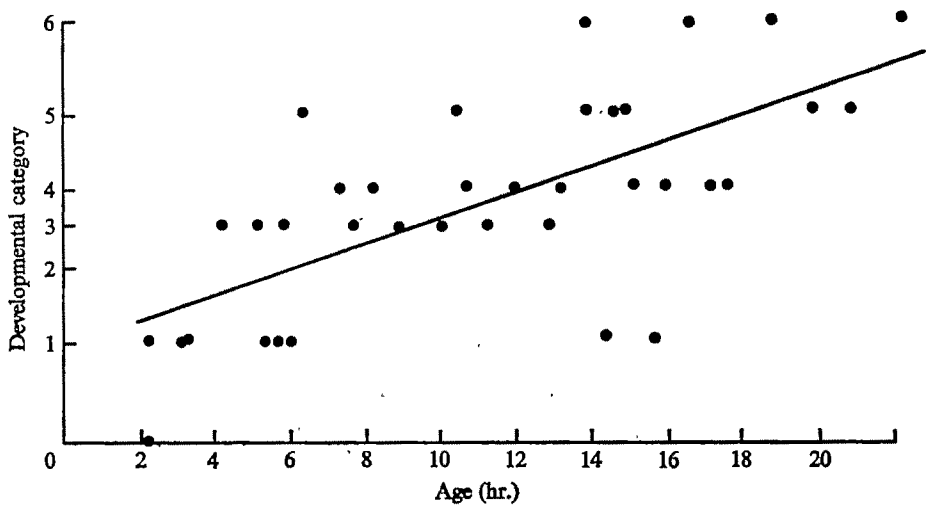

Fig. 7. Individual scores with regression line fitted. Non-uniform scale (see text). Group 6: chicks hatched and kept until testing in total darkness.

Statistical analysis

The results were analysed statistically in order to compare more precisely the rates of postural development of the chicks, and also their level of development at a particular age. This was achieved by fitting to the transformed observations within each of the six groups a regression line (Draper & Smith, 1966) of the level of development on the age of the chick.

The chicks in group 1 were all able to stand upright by 7½ hr. of age; as we wished to compare rates of development, in this group only observations on chicks aged less than 7½ hr. were considered in the analysis. The lines obtained by least squares

Table 2. *Levels and rates of chick development*

(Expressed on transformed scale; levels also given on original scale.)

Group	Level of development at age 10 hr.	Rate of development (slope of regression line)
1	73·2 (6·0)	7·5
2	58·5 (4·9)	2·2
3	47·9 (3·8)	1·8
4	48·8 (3·9)	2·4
5	41·3 (2·1)	1·5
6	46·1 (3·6)	1·7

regression are shown in Figs. 2–7. For groups 2–6 an estimate of the level of development of a chick aged 10 hr. was calculated, and is shown in Table 2, together with the level and rate of development on the transformed scale.

Comparisons of the groups in pairs were made using t tests; the differences between rates of development and between levels of development at 10 hr. are shown in Table 3, with the standard errors and levels of significance.

DISCUSSION

It is clear that the conditions of incubation and rearing can have an effect on the initiation and rate of development of standing and walking in the chick. In the present experiment the types of stimulation provided fell into two rather different categories, and these will be discussed separately.

Change in pre-hatching conditions

The accelerated chicks walked later than unstimulated chicks hatched at the same time, although, of course, they walked sooner than they would have done had they not been stimulated. There were statistically significant differences (Table 3) between the performance of chicks which hatched early, as a result of pre-hatching stimulation, and unstimulated chicks, when both groups were taken straight from the incubator for testing, and thus had the same post-hatching experience. This difference became apparent at about 4 hr. of age, when the unstimulated chicks first began to lurch on to their toes (up to this time the stimulated chicks had, in fact, been slightly the more advanced although the numbers were too small to test them for a significant difference). Within the limits of the experiment (20 hr.) the mean level of maturity achieved by the accelerated chicks remained lower than that of the unstimulated ones, although they were observed to become indistinguishable from them after a day or two in the brooder. We do not know what underlies this difference (which is consistent with that found in the Japanese quail; Vince & Chinn, 1971) as all the chicks appeared to be anatomically mature at the time of hatching; in fact, hatching seldom takes place before all the developmental changes leading up to it have been completed. Handling the chicks, however, gave the impression sometimes that the accelerated chicks were slightly weaker, or less well coordinated than normal chicks.

Table 3. *Comparison of groups*

Groups compared	Difference between levels of development at 10 hr., on transformed scale with S.E.	Difference between slopes of regression lines with S.E.
1–2	14·7 ± 1·4***	5·3 ± 1·6**
2–4	9·6 ± 1·9***	−0·2 ± 0·3
4–3	0·9 ± 2·4	0·6 ± 0·6
4–5	7·6 ± 2·0***	0·9 ± 0·3***
4–6	2·8 ± 2·0	0·7 ± 0·3*

* $P < 0·05$. ** $P < 0·01$. *** $P < 0·001$.

At the moment we do not know precisely what determines the time when a chick begins to walk on its toes, but we know how normal and accelerated foetuses differ: in eggs which hatch early the final stages of development are completed more rapidly. For example, it has been shown that accelerated quail have breathed for a shorter time before hatching than have unstimulated controls (Vince & Cheng, 1970) and that the beginning of lung ventilation is advanced in accelerated Japanese quail embryos although probably not in the bobwhite (Vince *et al.*, 1971). Unpublished experiments (Green & Vince) show that the yolk sac is withdrawn into the body cavity earlier in accelerated foetuses. Unpublished experiments (Vince & Green) now also show that lung ventilation begins earlier and lasts for a shorter time in accelerated as compared with unstimulated foetuses of the domestic fowl. In the domestic fowl Hamburger & Oppenheim (1967) have observed that coordinated behaviour occurs only from the 17th day of incubation; thus chicks hatched on day 19 or 20, instead of the normal day 20 or 21, had experienced about 25 per cent less practice in the coordinated reflex activity which could be relevant in standing and walking.

Changes in post-hatching conditions

Definite differences were found between chicks kept in the incubator and chicks taken from the incubator at least 2 hr. before testing; the differences were particularly marked when the chicks were kept in the light and with other chicks; in fact, in this group all the chicks were standing and walking normally at 7 hr. of age – several hours before this level was achieved by any other group. That this was the effect of other chicks rather than the effect of light alone, or the drop in temperature on removal to the brooder, is shown by the comparison between groups 1 and 2.

Again, it is not possible at this stage to suggest precisely what this effect could be due to; however, there is no doubt that the chicks were more active when in a group with other chicks, and there is evidence that stimulation such as that provided immediately before testing (stroking on the lower back and sides) caused the chicks to stand up, when they were able to do so. When huddling together under a brooder lamp similar stimulation could be provided by the other chicks. Chicks also appear to take up a standing posture more frequently when they have something to lean on, an effect which could be enhanced by the movements of siblings. More detailed work is needed here.

When group 2 (kept in brooder alone) were compared with group 4 (taken straight from the incubator for testing) the first group attained a significantly higher level of maturity; thus the presence of light could be having an effect, although in the present experiment the presence of light was accompanied by a drop in temperature and the presence of food and water, all of which need to be separated in future work.

The possible effect of actual restriction also needs further investigation; in the present experiment there was no significant difference between groups 3 and 4, both taken straight from the incubator for testing, but kept under different conditions while in the incubator, group 3 being slightly the more restricted. There was a significant difference between groups 4 and 6, where the latter were kept in total darkness. Thus this is a point which could repay further exploration, as in the present experi-

ment all the chicks were relatively restricted while in the incubator, and the age range of testing was limited.

The work described represents a preliminary survey of a restricted number of external factors affecting the early development of the capacity to stand on the toes and walk. It has been demonstrated that there is a problem here; the rate of development of the behaviour observed depends to some extent on pre- and post-hatching stimulation and is thus partly under external control.

The early development of standing and walking in the chick would appear to be similar to other essential patterns of behaviour such as pecking in the chick, analysed by Breed (1911), Bird (1933) and Cruze (1935), and also to the use of the feet in feeding by the great tit (*Parus major*) analysed by Vince (1964). In such work it has been shown that the rate of development of the behaviour varies with different conditions, begins as an approximation to the normal pattern and is perfected by experience, the similar end-result being, in part, due to the homogeneity of normal conditions. In precocial birds it does not appear to have been recognized that the attainment of standing and walking postures can be affected to some extent by individual experience; in precocial mammals, however, there are hints in the literature that this could be the case in certain species. For example, Herscher *et al.* (1963) have suggested that maternal licking may facilitate development in new-born lambs, and that lambs deprived of this type of stimulation may die. Similarly Altmann (1963) has made the suggestion that the elk calf is stimulated after birth to rise on to its feet by parental licking and that licking and nudging by the mother may stimulate the newly born moose calf to get up and nurse.

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(*Manuscript received* 15 *January* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 101–108
Printed in Great Britain

INFANTILE HANDLING AND ASSOCIATIVE PROCESSES OF RATS

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A review of the literature on the effects of infantile handling indicated that this variable has an effect upon the animal's later performance on various learning tasks. This effect may be the manifestation of at least two possible processes. As a result of its direct effects on the emotional as well as exploratory responsivity of the rat, infantile handling has an *indirect* effect upon the animal's associative learning processes. Because the learning criteria used in previous handling studies involve an increase in an animal's operant level of goal-directed activity, it is difficult to determine whether handling *directly* facilitates later learning. On the other hand, the results of studies of the effects of handling upon tasks where activity level increase is not confounded with learning would yield less ambiguous outcomes than those from earlier experiments. The aim of the present experiment was to train handled (H) and non-handled (NH) rats in a brightness discrimination learning situation and to examine the effects of punishment in that context. The fact that NH animals required fewer trials than H animals to relearn the formerly punished response suggests the possibility that H animals show greater retention of the learned fear response (hence greater associative ability) than NH animals.

The evidence indicating the facilitating effects of pre-weaning manipulation or handling of rats is considerable (Ader, 1970; Denenberg, 1964, 1967; Levine, 1962). Among these effects, infantile handling of rats results in more efficient performance on a conditioned active avoidance task (Denenberg, 1959; Denenberg & Bell, 1960; Denenberg & Karas, 1960; Levine, 1956). Although earlier studies did not show that infantile handling facilitates the rat's later learning on tasks involving food reward (Denenberg & Morton, 1962; Spence & Maher, 1962), more recent studies have clearly demonstrated the facilitating effects of handling on running speed performance (Goldman, 1965; Wong, 1970; Wong & Carey, 1968; Wong & Jamieson, 1968). It is now apparent that infantile handling does have an effect on the animal's later performance under food motivation. The critical issue concerns the interpretation of this effect.

The reactivity-activity hypothesis

An explanation of the above effect can be derived from Denenberg's (1967) recent theory. Although infantile handling has a direct effect on the emotional reactivity and exploratory tendency of rats (Denenberg, 1967), it does not directly facilitate the organism's associative learning processes. Because of their failure to observe performance differences between handled (H) and non-handled (NH) rats in the Hebb–Williams maze, Denenberg & Morton (1962) concluded that handling 'does not have any direct effect upon perceptual or problem-solving behaviour'. This result has been confirmed but interpreted differently by T. Schaefer (1968, expt. 3). More recently, Denenberg (1969) has hypothesized that 'stimulation before weaning appears to have its greatest effect upon affective-emotional behaviours while stimulation after weaning is more likely to modify cognitive function'.

A definite consequence of handling is the fact that H rats are more active and

defecate less than NH rats when tested in an open field. This finding was interpreted by Denenberg (1964) as indicating that H animals are less emotional than NH ones. More recently, Denenberg (1967) suggested that H rats exhibited a higher degree of exploratory behaviour than their NH counterparts when the animals were tested in situations involving the investigation of novel or social stimuli. From these data it is clear that handling increases an animal's activity level in the test apparatus.

An examination of the typical learning tasks used in previous handling studies indicates that the criterion measure of learning is generally one requiring activity increase. These tasks include: (*a*) acquisition of a conditioned active avoidance response (Denenberg, 1959; Denenberg & Bell, 1960; Denenberg & Karas, 1960; Levine, 1956); (*b*) discrimination learning motivated by negative reinforcement (electric shock) in a Yerkes–Thompson box (Smith, 1967); (*c*) acquisition of running in an alleyway for positive reinforcement (Goldman, 1965; Spence & Maher, 1962; Wong, 1970).

In these previous studies the apparent facilitating effect of handling on learning may be a *consequence* of its direct effect on increasing an animal's activity level. It may be for this reason that handling increases the number of avoidance responses or the running speed of the animal for food reward. This interpretation (which is derived from Denenberg's theory) suggests that handling has an *indirect* effect on associative learning. The effect is mediated by mechanisms which influence an animal's activity level.

THE ASSOCIATIVE FACILITATION HYPOTHESIS

In spite of the almost convincing logic of the above interpretation, a case can be made for the possibility that handling has a *direct* effect on the animal's later associative learning ability. Consider the finding that H rats alternate less under hunger than under food satiation (Wong, 1969, expt. 2). This was in contrast to NH rats who showed uniformly high rates of alternation under both drive levels. In this experiment food was present in both black goal-boxes of the T-maze. During trial 1 the animal was allowed to make a free choice and received food in either goal-box. On trial 2 the animal was given a 'forced' trial to the side which it chose on the previous trial. Between trials 2 and 3 the black goal-box of the previously non-entered side was removed and a white box was substituted in its place. During trial 3 the animal was allowed a free choice between the white and black goal-boxes. If the rat chose the white box (i.e. ran in the direction opposite to the previously entered black box) its response was scored as an alternation.

The results of the above experiment indicated an interaction between handling and drive level in their effects on alternation behaviour. When tested under low hunger drive both the H and NH groups alternated at a consistently high (80 per cent) level throughout the five test sessions. These findings are consistent with the results obtained by Wells *et al.* (1969), who compared H and NH rats in a Y-maze. Although their rats were neither food-deprived nor given food in the arms of the maze, Wells *et al.* found that both groups alternated on about 70 per cent of opportunities on each of the five experimental sessions. However, in the Wong (1969) experiment differences were found when the groups were tested under food deprivation. Whereas NH rats maintained a 72 per cent alternation rate throughout the

test sessions, H rats exhibited a progressively decreasing tendency to alternate. By the fifth test session H rats showed only 40 per cent alternation. These results were interpreted to indicate the superior ability of H over NH rats in acquiring a food-directed association. If a hungry animal were to receive food when responding in a specific direction to specific visual cues, an adaptive response would be to repeat that particular response on future occasions. It appears that H rats are more adaptive than NH rats in this respect. This interpretation is consistent with T. Schaefer's (1968) hypothesis that handling endows the rat with the capacity to make fine discriminations concerning the relevant aspects of the environment. The handled animal is thus more able to match its responses to the demand of the environment.

It should be noted that the response measure in the Wong (1969) study was that of choice rather than of speed or activity level. For this reason inferences about associative capacity were not confounded with the animal's operant level of activity. Another possible method of assessing the relative associative capacities of H and NH animals would be to compare their performance in a passive avoidance situation. The only published study which has dealt with this issue is a report by Ader & A. Schaefer (1968). Although they conducted their experiments for theoretical reasons different from the one being considered in this paper, Ader & Schaefer have shown that groups subjected to early stimulation picked up the contingencies of the experiment to a greater extent than the NH group. Expt. 2 of the Ader & Schaefer paper concerned the administration of variable shock to an animal for its departure from an elevation platform. During the single passive avoidance training trial, rats were shocked until they returned to the platform from which they originally jumped. On subsequent days the animals received one non-shock trial per day. The results showed that H rats exhibited longer latencies until leaving the platform than NH rats, i.e. NH rats jumped off the platform during the test trials much sooner than did the H rats.

Although Ader & Schaefer (1968) interpreted their results as demonstrating that 'manipulated' (including handled) animals show greater retention of a learned fear response than their 'unmanipulated' (NH) controls, another interpretation is possible. It may be the case that H rats show greater response suppression and slower recovery following the shock trial than NH rats because of their superior learning ability. In order to test this interpretation, it is necessary to conduct additional experiments in which animals are trained to the same criterion of learning for positive reinforcement before they are exposed to a punishment contingency.

On the basis of the results of the Wong (1969, expt. 2) alternation study and of the second Ader & Schaefer (1968) experiment, there is some basis for considering the possibility that infantile handling may facilitate the organism's associative processes. If it is granted that this effect is substantial, what is the mechanism underlying such a phenomenon? Such an effect may be explained in terms of Sluckin's (1964) notion of 'exposure learning'. Sluckin hypothesized that an animal's efficiency in coping with the environment is maintained or improved as a result of prior sensory input (Sluckin, 1964, p. 111). He suggests that exposure learning *per se* may contribute substantially to the matrix from which the more specific associative forms of learning emerge. He also indicates that exposure learning is at its height when the organism is very young and that such experiences would facilitate the animal's later

associative learning processes. The implication of this suggestion is that infantile handling has a *direct* effect on the animal's later associative learning ability.

It is obvious that the empirical status of the alternative interpretations of the 'handling-learning' effect must be determined by further studies. It is also clear that one must be very careful in choosing the appropriate tasks upon which to compare H and NH animals. On such tasks, learning ability should not be confounded with an animal's activity level. Alternatively, one should consider tasks which allow separate analysis of emotionality-activity and associative components of behaviour.

INFANTILE HANDLING AND SUPPRESSION-RECOVERY FOLLOWING PUNISHMENT: EFFECTS ON CHOICE

One solution to the problem concerning the confounding of activity level with learning would be to test the animals in a punishment situation. In such tests the animals would be trained to some criterion of stable performance in a positive reinforcement situation, then punished with aversive stimuli for making the formerly reinforced response. The animals thus could be compared in terms of degree of response suppression as well as in their rate of recovery following the removal of the aversive stimuli. If it is assumed that handling has a direct effect on the associative learning processes, the H rats should show faster response suppression and slower rate of recovery than the NH rats. On the other hand, if H rats show slower or less response suppression and faster response recovery than NH rats, then one can assume that the main effect of handling is to reduce the animal's emotional reactivity. For this reason H rats may exhibit less post-shock crouching and freezing behaviour than NH rats.

Upon what basis can we assume that the animal's learning ability is directly related to the degree of response suppression produced by punishment? One can examine an animal's associative capacity in terms of its adaptability in a particular situation. It follows that an animal that has been exposed to punishment for making a reinforced response and that continues to make this response is reflecting less associative learning than one which shows greater response suppression.

The preceding theoretical derivation is related to a hypothesis developed by Solomon (1964) on the relationship between degree of original learning and the suppressive effects of punishment. Solomon proposed that overtraining of a response often increases the effectiveness of punishment as a response suppressor. Support for Solomon's hypothesis has been provided by the results of experiments by Karsh (1962) and Miller (1960).

The aim of the present experiment was to compare H and NH rats in an instrumental learning situation involving positive reinforcement of a brightness discrimination and to examine the effects of punishment in this context.

METHOD

Subjects. The subjects were 26 female Wistar albino rats which were bred in this laboratory. The cross-litter method was used to assign the pups to either the H or NH conditions. Within a given litter half of the pups was exchanged with half from another litter within the same 2-day period. An equal number of litters was assigned by random procedure to either the H or NH conditions. The animals were housed by litters until they were weaned at 21 days.

Apparatus. The apparatus consisted of a T-maze which was 3 in. wide and 6 in. high. It had a 12 in. start-box, a 24 in. stem and 4 in. long goal arms which led to two 12 in. goal-boxes. Sliding doors separated the start-box and the goal-boxes from the stem. Except for the goal-boxes, all of the T-maze was painted grey. One of the goal-boxes was white and the other black.

Procedure. The handling treatment was begun 3 days after birth of animals in the H condition and the treatment continued until they were 21 days old. During the handling sessions the mother was removed from the nesting cage and the pups were placed into separate compartments of a wooden box. After 3 min. each animal was removed from the box and returned to the nesting cage along with the mother. The NH rats were not disturbed from their home cages during this period.

On day 22 all animals were weaned and housed individually in $7 \times 9\frac{1}{2} \times 7$ in. metal cages while on an *ad lib.* diet. When they were 75 days old, the animals were placed on a restricted diet of 10 g. food ration per day. They were maintained on this feeding schedule for 14 days before the training sessions and were kept on this schedule throughout the experiment.

On day 90 each animal was given daily blocks of four trials during the acquisition of brightness discrimination. A block consisted of one free trial followed by three forced trials. This sequence of free and forced trials was used in order to provide each subject with equal experience in the rewarded and non-rewarded goal-boxes. Half of the animals in each group were rewarded with four 0·045 g. Noyes pellets for choosing the white goal-box (during the free trial), while the other half received the pellets for choosing the black goal-box.

The blocks were arranged so that each rat ran into the rewarded goal-box for two trials and into the non-rewarded box for the other two trials each day. After the initial free trial, the sequence of trials was arranged so that the animal was forced into the box opposite to its initial choice for two consecutive trials. On trial 4 the rat was forced into the box which it chose on trial 1. The location of the rewarded and non-rewarded goal-boxes was randomized during each trial.

The animals were given daily acquisition trials until they made three correct choices (during the free trial) over three consecutive days. All subjects attained criterion within 15 days. The day following their last training session, the animals were given 1 sec. of 1·5 mA electric shock after they entered the S+ box and were kept in by the goal-box door. The shock was delivered through the grid-rod floor of the box via a Grason-Stadler shock generator and scrambler (Model E 1064 GS).

The testing sessions began the day after the punishment session. The animals were again placed in the T-maze and received food if they entered the S+ box and no food if they entered the S− box. They were given one free trial per day for 25 days.

Results

Acquisition. A comparison between the number of correct responses attained by the H and NH groups over the 15-day period was analysed in trial blocks of 3 days. An analysis of variance of the data revealed that the H group made more correct responses over the five trial blocks than did the NH group ($F = 4{\cdot}48$; d.f. $= 1, 24$; $P < 0{\cdot}05$). Although there was a significant trial blocks effect ($F = 14{\cdot}10$; d.f. $= 4, 96$; $P < 0{\cdot}001$), the interaction between handling and trial blocks was not statistically significant ($P > 0{\cdot}05$).

Post-shock performance. Over 25 consecutive days the number of choices made by the H and NH groups to the S+ box was recorded and analysed. For analysis the data were combined into trial blocks of five. A graphic presentation of these results is presented in Fig. 1. It can be seen that the NH group reached a higher level of post-shock performance than did the H group. Over the five trial blocks NH animals made a greater number of choices to the food-rewarded (S+) box than did the H animals ($F = 7{\cdot}365$; d.f. $= 1, 24$; $P < 0{\cdot}025$). By the fifth trial block (days 21–25) animals in the NH group had shown almost consistent preference for the S+ box in contrast to the H group's tendency to choose the S+ box about two times out of every five

test trials. These results indicate that NH rats had recovered from the punishment effects much sooner than did the H rats. Although the trial blocks effect was significant ($F = 2{\cdot}996$; d.f. = 4, 96; $P < 0{\cdot}05$), the handling by trial blocks interaction was not significant at the 0·05 level.

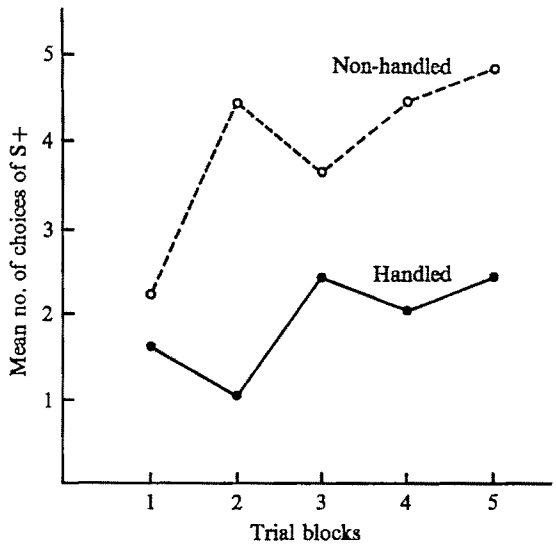

Fig. 1. Mean number of choices to S+ made by handled and non-handled animals following punishment.

Discussion

The results of this experiment are consistent with those obtained in expt. 2 of the Ader & Schaefer (1968) article. These results indicated that the H group showed greater suppression and slower recovery following punishment than the NH group. It appears that H rats behave in a consistent manner in both the step-down, passive avoidance and the punishment of a brightness discrimination situation. In both conditioned punishment situations, H animals show greater retention of a learned fear response than their NH counterparts. Such results are not consistent with a prediction from the reactivity–activity hypothesis that NH rats should show greater response decrement following punishment than H rats. Previous research on rats which were selectively bred for emotionality had shown that the reactive strain exhibit greater response suppression following shock than the non-reactive strain (Ferraro & York, 1968; Singh, 1959). Although the evidence indicating the profound effect of handling upon affective-emotional behaviour is unquestionable (Denenberg, 1967, 1969), it appears that one cannot assume that the effects of early stimulation and of selective breeding for emotionality result in behavioural equivalence.

If one can assume that the degree of learning exhibited in a punishment situation is related to the degree to which an animal ceases to make the formerly punished response, the results of the present experiment could be interpreted in terms of the superior associative learning exhibited by H rats. Although the emotional responsiveness of an animal does determine its degree of punishment suppression, it appears

that an associative element may also operate in this situation. The amount of suppression produced by a conditioned punishment procedure seems to be a function of the associative learning capacity of the animal.

It is possible that the results of the present experiment occur only when the animals are tested for the effects of conditioned punishment. In this procedure the presentation of shock is contingent upon the animal's response during the fear-acquisition stage of the experiment. In the conditioned suppression procedure the two stimuli, CS and shock, are presented independently of the animal's behaviour during fear acquisition (Lyon, 1968). In an experiment which compared H and NH groups for conditioned suppression, T. Schaefer (1968, expt. 8) initially trained the animals to lever-press for water in a Skinner box. After the animals had attained a stable level of responding, they received a series of classical fear-conditioning trials in which the onset of a 3-sec. light was terminated by shock to their feet. The day following the fear-conditioning trials the animals received further water-reinforced lever-pressing trials. This time, however, each lever-press produced the onset of the light as well as the water reinforcement. Under this condition NH rats took longer to resume lever-pressing to a 10-per-min. criterion than did H rats. In essence, NH rats required more time to extinguish their conditioned fear.

It thus appears that conditioned punishment and conditioned suppression procedures differ in their effects on the post-shock performance of H and NH animals. When tested for the effects of conditioned suppression, NH rats show greater suppression and slower recovery than H rats (T. Schaefer, 1968, expt. 8). However, the results of the present experiment as well as those of Ader & Schaefer (1968, expt. 2) indicate that H animals show slower recovery from the effects of conditioned punishment than their NH counterparts. It is believed that the processes underlying conditioned suppression and conditioned punishment are different. Hunt & Brady (1955) observed that the dominant response pattern exhibited by rats during conditioned suppression was crouching, freezing, and defecating. It is not surprising that, because of their greater emotionality, NH rats suppress more than H rats during that test. However, the dominant response pattern elicited by the conditioned punishment procedure is abortive instrumental responding (Hunt & Brady, 1955). It is believed that because of their better associative learning ability, H rats exhibit less responding following punishment than the NH animals.

This research was aided by a grant from the National Research Council of Canada, APA-247.

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(*Manuscript received* 8 *December* 1970; *revised manuscript received* 18 *March* 1971)

Br. J. Psychol. (1972), 63, 1, *pp.* 109–117
Printed in Great Britain

DEVELOPMENTAL CHANGES IN SHORT-TERM RECOGNITION MEMORY

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Changes in auditory and visual recognition were studied in subjects ranging in age from 10 to 60 years. Four-word lists were presented serially at a rate of 0·75 sec. per item and followed after 1·5 sec. by a test word for recognition. Under a separate condition subjects simply named the test word as rapidly as possible. It was argued that subtracting naming latency from decision latency yielded a relatively pure measure of comparison time in memory. Age effects were found in decision latency, but these were greatly reduced by the subtraction procedure. When presentation list and test word were both given auditorily no age differences were found in corrected decision latency, but developmental changes remained when the list and the test word were presented in different modalities. It was concluded, first, that in comparison with perceptual and response factors, memory scanning time is relatively insensitive to age differences and, second, that auditory recognition involves the use of a pre-linguistic or 'representational' memory system which is not sensitive to age over the sampled range.

A common finding in memory studies is that retention of auditorily presented linguistic information is superior – in the short term – to retention of visually presented material. This difference is exaggerated in young children and in the elderly for both recall (Arenberg, 1968; Murray & Roberts, 1968) and recognition (Carterette & Jones, 1967; McGhie *et al.*, 1965). However, it is not known whether these findings reflect changes in (*a*) the perception of aural as opposed to visual verbal stimuli or (*b*) the differential efficiency with which aural and visual items are acquired, stored in or retrieved from memory. It is possible that all of these functions change with age, and that they make various contributions to the aural/visual comparison at different ages. The investigation reported in this paper used a modification of the recognition reaction-time technique developed by Sternberg (1966, 1969) to study developmental changes in processes thought to underlie recognition performance in short-term memory.

In short-term recognition memory experiments the subject is presented with a list of items followed immediately by a test item. He must decide as rapidly as possible whether the test item was presented in the preceding list. Preliminary studies showed large age differences in the latency of this decision. The aim of the present study was to estimate the memorial component of these age-related changes in short-term recognition memory.

Following a previous paper (Kirsner & Craik, 1971) it is assumed that decision latency can be broken down into three main components: first, the time necessary to perceive the test item; second, memory comparison and decision time; and third, the time taken to organize and emit a response. Under one condition – decision latency – the subject decides whether or not the test item was present in the preceding list. Under a separate condition list and test stimuli are presented in identical fashion, but the subject is simply required to name the test word as rapidly as possible. It is

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assumed that this task – naming latency – involves only the first and third stages of overall decision latency and that the difference between decision and naming – corrected decision latency – provides a direct estimate of the memorial component of decision latency. In the present study corrected decision latency is used to examine age-related changes in memory where these are unconfounded by perceptual and response factors.

List and test modality manipulations can be used to determine the locus of age changes in memory comparison and decision time. For example, differences between the auditory presentation/auditory test (AA) and auditory presentation/visual test (AV) conditions may be attributed to translation or retrieval factors taking effect after presentation of the test word. On the other hand, if we assume, following Sperling (1967), that short-term memory uses an auditory code, it follows that differences between AA and VA may be attributed to factors operating in list acquisition and storage. Two modality comparisons were included in the present study. In Expt. I the comparison was made between AA and AV in order to examine age changes in processes operating when the test item is presented. In Expt. II the comparison was between AA and VA in order to assess differences arising during list acquisition. Preliminary studies had shown that for 11-year old children and young adults there were no differences between AV and VV for naming or decision. Therefore VV was not included in this study.

The main issues may be summarized at this point. First, does the naming control eliminate age changes? If corrected decision latency is invariant with age it may be inferred that age changes in 'memory' in this situation result from changes in perceptual (or response) factors rather than memorial factors. Second, does the naming control eliminate the increased disadvantage shown with visual material by young children and the elderly? From age changes in corrected decision latency for AV it might be inferred that a post-perceptual process – such as translation of the perceived test word into a form suitable for the recognition comparison – is particularly sensitive to the age variable. On the other hand, age changes in corrected decision latency for VA would lend support to the notion that young and old people are deficient in the acquisition of visual verbal material.

EXPERIMENT I

Method

Subjects

Sixteen subjects were drawn from each of four age ranges. These were 9·6–10·6 (mean 9·10), 13·6–14·6 (mean 13·10), 18–32 (mean 24·4) and 51–69 (mean 59·3) years. The age groups and subjects were chosen to cover a wide range of developmental differences, but excluded institutionalized elderly subjects and young children not yet able to read. The subjects, chosen from average or better ability ranges, were tested individually.

Design

The experiment was conducted using a mixed design. Age was necessarily a between-subject factor while, within subjects, the design comprised the factorial combination representing naming or decision and auditory or visual presentation of the test word.

This combination permitted analysis of decision and corrected decision data. The use of corrected as opposed to uncorrected decision data has several advantages in developmental and ageing experiments. First, it excludes the influence of age-related changes in the time taken to

perceive verbal stimuli. Similarly, any interaction between age and perception time for visual as opposed to aural test stimuli will be masked by the subtraction of naming from decision latency. Secondly, the naming latency control procedure should mask non-verbal age-related factors thought to influence perceptual and response times. For example, visual acuity (Farrimond, 1967), expectancy and warning interval (Botwinnick & Thompson, 1966), and the capacity to switch attention between modality are factors thought to be influenced by development or ageing. Provided that they act equally on the naming and decision processes, corrected decision latency will be free from these influences.

Subjects completed all conditions in one session. This was divided into four 35-trial blocks, one for each combination of test modality and task. Test modality order was counterbalanced with half the subjects completing each condition first. Task was similarly counterbalanced within each modality combination.

Each 35-trial block was continuous, and comprised 28 'old' words (words drawn from the preceding four-word list), and seven 'new' words (words not drawn from the preceding list) as test stimuli. Old words were drawn equally from serial positions 1–4 of the presentation list and these and new words occurred randomly within each 35-trial block.

The words were drawn from a pool of 580 common, two-syllable nouns of five, six or seven letters. Two blocks were constructed for each modality combination. For any subject one block was used for decision and the other for naming. Each block was presented to half the subjects for naming and to the other half for decision. No word was used more than once in the experiment.

Procedure

Five trials were presented per minute. For each trial the four list words were presented at a rate of 0·75 sec. per item and followed after 1·5 sec. by the test word. No warning signal was given before the test word, but subjects were instructed to treat the pause itself as the warning, thus avoiding the possibility of cross-modal interference. The subject wore a throat microphone and responded 'yes' or 'no' in the decision task and with the appropriate word in the naming task.

Auditory items, previously recorded by a male assistant, were presented through earphones from one track of a stereo tape-recorder at 60 db. Visual items, previously filmed on alternate frames of 16 mm film, were exposed on a screen measuring 30 × 50 cm and situated 90 cm in front of the subject. The letters, in upper-case characters, subtended an angle of approximately 50 min. and appeared as white stimuli on a black background. Visual test words were exposed for 1·5 sec.

In AA the test word was passed through a speech relay and this started the millisecond stop-clock. A diapilot facility on the tape-recorder was used to control the projector and start the timer. In all cases the timer was stopped by the subject's vocal response, amplified from the throat microphone signal.

Each subject was informed that he would be participating in an experiment concerning reading ability and memory. The auditory and visual presentation methods were demonstrated and the vocal response procedure was explained and then practised by the subject. Finally, the following specific instructions were given: '(1) Attend to each word in the memory set as it is presented but do not repeat it to yourself or rehearse it again before the test word is presented. (2*a*) In the naming task you should concentrate on naming the test word as fast as possible regardless of whether it was in the list or not. (2*b*) In the decision task you should say "yes" as quickly as possible if the test word occurred in the preceding list. If the test word was not in the list, you should say "no" as quickly as possible. (3) Accuracy is important but you should try to give your answers as quickly as possible.' These specific instructions were repeated before each block, with the exception that the naming or decision instructions were omitted as appropriate.

Results

The basic datum was median reaction time from correct responses for each combination of task and test modality. Mechanical and electrical failures and inappropriate responses accounted for 3 per cent of all responses. The overall omission error rate for the decision task was 2 per cent.

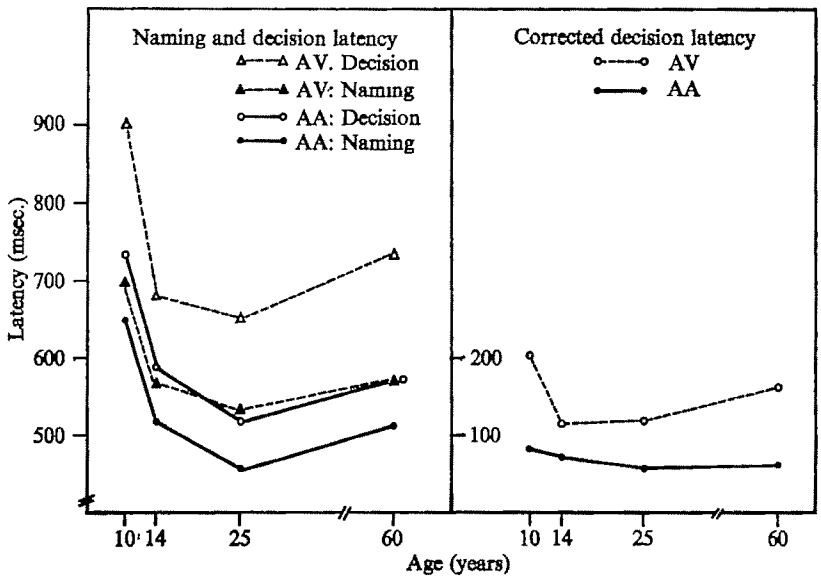

Fig. 1. Naming and recognition latency as a function of modality combination and age (Expt. I).

The main results, shown in Fig. 1, are the age functions for AA and AV for naming and decision. These data are based on at least 350 observations per point and represent mean medians for each age group. Fig. 1*a* shows that naming and decision latencies are much longer for the 10 year group than for the other groups, that there is a slight improvement from 14 to 25 years, and a slight decline from 25 to 60 years. In addition, decision is slower than naming and AV slower than AA. The AA and AV data are re-presented in Fig. 1*b* as corrected decision latencies. The figure shows that memory comparison times change only slightly as a function of age. Corrected decision latency for AA is virtually constant across age, and AV decreases from 10 to 14 years and increases from 25 to 60 years.

The decision and corrected decision data were subjected to separate analyses of variance. Table 1 shows that they yielded qualitatively similar results, but that the naming control reduced the magnitude of both main effects. Of particular interest was the finding that the proportion of total variance accounted for by the main effect of age was reduced from 30·2 per cent in the decision analysis to 6·6 per cent in the corrected decision analysis.

Table 1. *Summary of analyses of variance on median reaction time for decision latency and corrected decision latency for Expt. I*

	Decision latency		Corrected decision latency	
Source	D.F.	*F*	D.F.	*F*
Age group (A)	3, 60	15·73***	3, 60	2·93*
Test modality (B)	1, 60	190·64***	1, 60	76·55***
A×B	3, 60	2·77*	3, 60	3·34*

* $P < 0·05$. ** $P < 0·01$. *** $P < 0·001$.

In the corrected decision analysis the main effect of age was significant and AA latencies were reliably faster than AV latencies. The interaction between age and test modality was significant, but a test of the simple main effect of age showed no significant differences between the means in the AA condition ($F < 1$). Further tests showed that for AV the difference between the 10 and 14 year groups was significant ($t = 2{\cdot}90$; d.f. $= 15$; $P < 0{\cdot}01$), but that the difference between 25 and 60 years was not ($t = 1{\cdot}40$; d.f. $= 15$; $P < 0{\cdot}05$). The interaction should be attributed, in the main, to changes in AV between 10 and 14 years.

Mean correct rejection latencies (i.e. the time taken to say 'no' correctly to new items) were longer than correct detection latencies. Over all groups this increment was 17 and 11 per cent for AA and AV respectively. Naming latency, the effect of recent experience of an item on naming latency, and serial position effects are treated in a separate paper.

Experiment II

Method

Subjects

Sixteen subjects were drawn from each of the 10 (mean 10·1), 14 (mean 14·2), 25 (mean 26·3) and 60 (mean 58·7) year groups.

Design

As for Expt. I, except that the modality combinations were AA and VA instead of AA and AV.

Procedure

As for Expt. I, except that the visual items in the VA list were controlled by the diapilot facility on the tape-recorder and the timer was started by the auditory test word as in AA.

Visual list words were exposed for 0·38 sec. with a blank frame for 0·38 sec. between item frames.

Results

The basic datum was median RT for correct responses for each combination of task and presentation modality. Mechanical and electrical failures and inappropriate responses accounted for 3 per cent of all responses. In the decision task, omission error rates were 0·6 and 13·1 per cent for AA and VA respectively. The error rates in VA were 20, 9, 8 and 16 per cent for the 10, 14, 25 and 60 year groups respectively.

The main results, shown in Fig. 2, are age functions for AA and VA for naming, decision and corrected decision latency. These data are based on at least 290 observations per point. Fig. 2*a* shows that, as in Expt. I, the 10-year-olds are much slower, that there is a slight improvement from 14 to 25 and then a slight decline from 25 to 60 years. As in Expt. I, the correction procedure substantially reduced differences between age groups. Corrected decision latency is shown in Fig. 2*b*. The AA data show little change with age, but VA decreases from 10 to 25 and also – unlike AV – from 25 to 60 years.

The decision and corrected decision data were subjected to separate analyses of variance. Table 2 shows these analyses. As in Expt. I, the naming control reduced the magnitude of all main effects. The proportion of total variance accounted for by the main effect of age was reduced from 39·6 per cent in the decision analysis to 7·2 per cent in the corrected decision analysis.

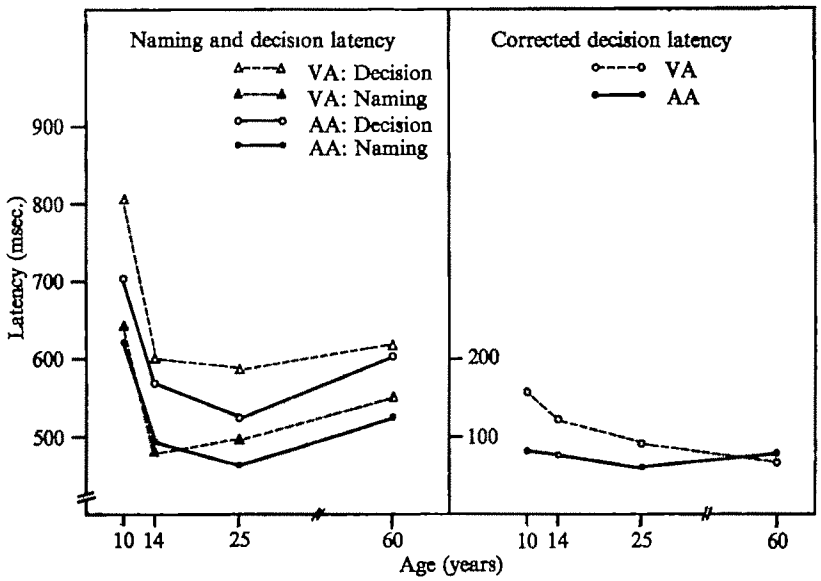

Fig. 2. Naming and recognition latency as a function of modality combination and age (Expt. II).

In the corrected decision analysis the main effect of age was not statistically reliable. AA latencies were significantly faster than VA latencies, but this result is qualified by the significant interaction between age and list modality. A test of the simple main effect of age in AA showed that these differences were not statistically reliable ($F < 1$) and the interaction should therefore be attributed to the decrease in VA as a function of increasing age.

The number of omission errors in VA was significantly greater (by sign tests) than in AA for all age groups. The disadvantage shown with visually presented material may be attributed to perceptual or memorial factors. A comparison between omission and correct rejection latencies may be used to explore the locus of this disadvantage. For example, if the omitted items were not 'perceived' during list presentation, rejection latency for these items should be similar to rejection latency for objectively new test items. Of the subjects with at least three omission errors in VA, median omission latency was longer than median correct rejection latency in 26 out of 28 cases. Therefore it seems likely that the omitted items were perceived

Table 2. *Summary of analyses of variance on median reaction time for decision latency and corrected decision latency for Expt. II*

	Decision latency		Corrected decision latency	
Source	D.F.	F	D.F.	F
Age group (A)	3, 60	17·39***	3, 60	2·44
List modality (B)	1, 60	39·24***	1, 60	14·49***
A × B	3, 60	5·05**	3, 60	4·25**

* $P < 0·05$. ** $P < 0·01$. *** $P < 0·001$.

during list presentation, but their memorial availability was inadequate for accurate recognition.

Discussion

The findings to be commented on include the reduction of age differences in decision latency when perceptual and response factors are controlled, the absence of age differences in corrected decision latency for AA and, finally, residual age-related changes in AV and VA.

The experiments showed substantial age differences in decision latency. These results followed an expected pattern, with the 25 year group making the fastest decisions. But when allowance was made for changes in perceptual and response components of decision latency the proportion of total variance attributable to age decreased by over 80 per cent. The finding that 'pure' decision latency is relatively insensitive to age is consistent with other recognition memory data. Calfee (1970), for example, found that accuracy in short-term recognition memory for pictorial stimuli changed little from 4 to 12 years, and Schonfield (1965), using visually presented verbal stimuli, reported no change in recognition accuracy over the 20–60 year range. Thus apparently both developmental and ageing changes affect perceptual and response factors much more than memory comparison time.

Corrected decision latency for AA was invariant with age over the range 10 to 60 years. Although young and old subjects took longer to perceive and respond to test stimuli when list and test stimuli were presented auditorily, they did not take more time on the search and decision components of the recognition memory task. An alternative interpretation is that the naming and decision tasks involve different sampling and decision procedures (hence the absolute difference) but extract information from a common system. This explanation is consistent with the suggestion made by Neisser *et al.* (1969) that the mechanisms of verbal short-term memory are continuous with those of speech perception.

The poor beginning and fast growth of skill in AV and VA suggests that encoding between vision and memory is an important developmental process. The similar functions for AV and VA might indicate a common factor – such as auditory/visual integration (Birch & Belmont, 1965) – but examination of the procedure and results suggests that this is not the case. In AV age changes in visual word perception time are estimated and excluded from the final analysis. If, as seems probable, the recognition comparisons in AV occur at a level of processing subsequent to sensory storage, the developmental change in corrected decision latency reflects changes in the efficiency with which comparisons and decisions are made in verbal or abstract memory. But in VA slow visual word perception will occupy time during list presentation and this cannot be estimated and excluded in the same way. Therefore it seems plausible that in so far as young subjects take longer to perceive list items, they will have less time to 'learn' or rehearse them. Developmental changes in rehearsal may be an additional factor, but the presence of significantly more errors in VA for all age groups suggests that simple changes in rehearsal efficacy are not critical.

The comparison between AA and VA is also of interest. Since young subjects take longer to perceive auditorily and visually presented list stimuli they will have, in AA as in VA, less 'post-naming' time for further processing. But comparison of the

developmental changes in AA and VA for corrected decision latency indicates that this is important only for VA. That is, when list items are presented visually young subjects need more verbal rehearsal time in which to store the appropriate information in memory. But when list items are presented auditorily the amount of time available for verbal rehearsal does not affect performance. This evidence may be adduced as further support for the notion that AA recognition is based on 'representational' memory, whereas VA recognition is based on comparisons made in an 'abstract-verbal' memory system (Wickelgren, 1969).

The notion that age differences are reduced in situations where the subject can use representational memory is consistent with the finding (Calfee, 1970) that recognition memory for pictorial stimuli does not change from 4 to 12 years. The generality of this – in terms of both age range and stimulus material – is not known.

The general proposition that representational and abstract-verbal memory follow different age functions does not find support in the ageing literature. It is well known that recall declines with advancing age, but in another task requiring abstract-verbal memory – auditory presentation followed by a visual recognition test after approximately 1 min. – recognition accuracy was invariant with age (Schonfield & Robertson, 1966). And finally, in the present study, performance on the two cross-modal tasks (AV and VA), which presumably use post-representational memory, did not decline with age. Thus the proposition that representational and abstract-verbal memory systems follow different age functions may be valid for developmental but not ageing changes.

The present results do throw some light on the question of whether memory loss with age is due to a storage or retrieval deficiency. McNulty & Caird (1966) suggested that the aged were unimpaired on recognition memory tasks because, although their storage was deficient, they could attain satisfactory performance by using partial images of the original stimuli. One general advantage of the recognition reaction-time technique is that it can be used as a sensitive index of storage strength in circumstances where overall accuracy is very high (Forrin & Morin, 1969). In the present study accuracy is very high for AA and AV, but the absence of an age decrement in corrected decision latency – for AA at least – suggests that the storage deficiency hypothesis is not tenable. This conclusion is of course limited to very short-term memory.

In summary, the present study explored a situation in which four words were followed by a test word. Under one condition the subject named the test word as quickly as possible, and under the second condition he decided rapidly whether the test word had occurred in the preceding list. In an attempt to obtain relatively pure measures of decision times in recognition memory, naming latency values were subtracted from the corresponding values of decision latency. The main findings were: (*a*) the subtraction procedure substantially reduced differences between age groups over the range 10 to 60 years, (*b*) corrected decision latency for AA was insensitive to development and ageing changes, and (*c*) corrected decision latency for AV and VA showed a developmental trend. It was concluded, first, that perceptual and response factors are more sensitive to age changes than memory and, second, that AV and VA recognition decisions are based on information in an abstract-verbal memory system, whereas AA decisions use representational information.

This research was supported by Medical Research Council grants to Kim Kirsner and to F. I. M. Craik. The study forms part of a Ph.D. thesis submitted to the University of London and supervised jointly by B. Hermelin and F. I. M. Craik. The author is grateful for the cooperation of staff and children at the following schools: St John Fisher's School, Perivale; Ealing Boys' Grammar School, Ealing; Friends' School, Saffron Walden; Dame Alice Owen's Girls' School, London; Montpelier Primary School, Ealing; and Drayton Manor Grammar School, Ealing.

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(*Manuscript received* 15 *December* 1970)

Br. J. Psychol. (1972), 63, 1, *p.* 119
Printed in Great Britain

PAPERS TO APPEAR IN FORTHCOMING ISSUES

(Not previously listed)

BOWER, A. C. & DAS, J. P., Centre for the Study of Mental Retardation, University of Alberta, Edmonton, Alb., Canada. Acquisition and reversal of orienting responses to word signals.

CONRAD, R., MRC Applied Psychology Unit, Cambridge. Short-term memory in the deaf: a test for speech codmg.

DERREGOWSKI, J. B., Department of Psychology, University of Aberdeen. Reproduction of orientation of Kohs-type figures: a cross-cultural study.

FRICK, J. W. & COFER, C. N., Department of Psychology, Pennsylvania State University, University Park, Penna., U.S.A. Berlyne's demonstration of epistemic curiosity: an experimental re-evaluation.

FRIED, P. A., Department of Psychology, Carleton University, Ottowa, Canada. Conflict resolution by septal, dorsal hippocampal or ventral hippocampal lesioned rats with pre- or post-operative approach training.

INNES, J. M., Department of Psychology, University of Birmingham. The relationship of word-association commonality response set to cognitive and personality variables.

KENNEDY, R. A., Department of Psychology, University of Dundee. Semantic constraints on location operations in simple sentences.

MILLAR, S. A., Department of Experimental Psychology, University of Oxford. The development of visual and kinaesthetic judgements of distance.

NICKI, R. M., Department of Psychology, University of New Brunswick, Fredericton, N.B., Canada. Arousal increment and degree of complexity as incentive.

SIDDLE, D. T., Department of Psychology, University of Southampton, Vigilance decrement and speed of habituation of the GSR component of the orienting response.

WILDING, J. M. & MEDDIS, R., Department of Psychology, Bedford College, University of London. A note on personality correlates of motion sickness.

WOOKEY, P. E. & STRONGMAN, K. T., Department of Psychology, University of Exeter. Quantitative reward shift in the double runway.

Br. J. Psychol. (1972), 63, 1, *pp.* 121–144
Printed in Great Britain

BOOK REVIEWS

Pattern Recognition. By D. W. J. CORCORAN. Harmondsworth: Penguin Education. 1971. Pp. xvi + 207. 50p.

This book is the first to appear in the Perception and Thinking area of the Penguin Science of Behaviour project. Pattern recognition would seem to be an excellent choice of topic to begin the series as it is both fundamental and contemporary in the general area.

Corcoran has enthusiastically tackled the rather complex problem of pattern recognition, but obviously has felt very constrained by the necessary restriction imposed by the short 'unit' publication scheme.

The basic problem in pattern recognition is introduced from the start as how we are able to name or recognize some complex stimulus (such as a face), and the author concludes that the way to approach this experimentally is to investigate absolute judgements of multidimensional stimuli.

According to Corcoran, the theoretical framework in this area is weak and consists only of rather narrow, specialized theories. However, although there may not yet be enough facts to distinguish between theories, it would seem that there is currently sufficient activity in this area to enable investigators of pattern recognition to adopt more than just an 'attitude' to the problem.

Chapter 2 describes patterns as multidimensional stimuli, thus enabling the author to disregard work on unidimensional stimuli. However, some experiments with unidimensional stimuli are considered initially in order to introduce (very clearly and efficiently) the main ideas of information theory and its approach to the problem.

The basic experimental situations and tasks are described in considerable and deliberate detail. Corcoran argues that the notion of a limited capacity channel has been too readily accepted as an explanation of the fact that only a very small number of multidimensional stimuli can be perceived in typical experimental situations, and suggests that absolute identification experiments really demonstrate the effects of random sequences of stimuli upon perception. This chapter certainly makes clearer Garner's argument about the form and amount of redundancy contained in information patterns.

The next chapter is concerned with input processes and describes different scanning strategies (serial and parallel) for extracting information from the multidimensional stimulus. More detailed description is given of Selfridge's computer model and Neisser's experiments, which tends to make this chapter a bit tedious, especially when the rather artificial and confusing classification terminology is introduced. Corcoran discusses the notion of immediate perception of 'Gestalts' and the general concept of 'attention'. Although the initial scanning process is probably serial and self-terminating, the author concludes that with training it is possible to extract the relevant information from patterns by parallel scan.

Corcoran next introduces work on pattern recognition by computer and provides quite a good description of the salient features of the various models and programs. Speech recognition is considered in some detail and the notion that the phoneme is basically a multidimensional stimulus generates further implications for discussion. There is a reference to an interesting experiment by Corcoran and Rouse suggesting different programs for recognition of handwritten and printed characters, and, by this stage, one gets the impression that we are really getting somewhere with the problem. This is probably the best chapter in the book.

Chapter 5 (on theories of form and speech perception) starts off with shape perception in animals and discusses theories of initial coding of the input. Corcoran adds a timely criticism of the *ad hoc* creation of 'analysers' by theorists who happen to 'need' them. He then gives a brief account of Hebbian theory, pinpoints the major issues in the Gestalt–Behaviourist impasse, and outlines the theories of Deutsch, Sutherland and Dodwell – although all this takes us back a few years. Fortunately, Corcoran returns to the more contemporary issue of linguistic analysis with a useful description of Liberman's active theory of speech perception.

Corcoran really gets down to the problems of analysis, resynthesis and multiple read-off in

Chapter 6. Experiments are described which support the idea that the final stage in the analysis is an analogue-to-digital conversion, with read-off from different levels.

Probably too much emphasis is given to experiments on sensory independence, but this is one area in which Corcoran has been making a very active and substantial contribution. It is therefore understandable, but not necessarily desirable in a book of limited space on a wide topic, that the author has tried to pack in so much of his own experimental work.

Occasionally, there are teasing hints at the rather important philosophical problems involved in this topic, but Corcoran carefully avoids getting drawn into them.

The author has carried out his intentions (as stated in the preface) quite successfully. The problem seems to lie in his concern for experimental detail. In a book of this kind there is always the difficulty of deciding just how much detail to give, but Corcoran has probably overstepped the mark. In several of the more tedious chapters there is a tendency for the reader to become bogged down and even confused by the experimental details.

However, it is quite obvious that the book will be widely recommended and read, as there is probably no other current book on pattern recognition as useful as this. Even by Penguin Education standards, this book is exceptionally good value for money.

G. LOWE

Attention: Contemporary Theory and Analysis. Edited by DAVID I. MOSTOFSKY. New York: Appleton-Century-Crofts. 1970. Pp. vii + 447. $11.55.

This book comprises a collection of commissioned papers centred on the use of the concept of attention in research and theory. The list of contributors is impressive, including Berlyne, Broadbent, Hochberg, Honig, Luria and Mackintosh. Of the 18 contributions, two (those by Boring) are short historical perspectives and the rest are divided almost equally between those which are predominantly human-orientated and those which are animal-orientated. Six papers emphasize human performance/cognitive approaches to attention (perceptual selection, vigilance, novelty, multichannel information processing, attentional styles), five deal with behaviour theory (discrimination, generalization, stimulus control, probability learning), while the remaining six examine neurophysiological aspects (evoked potentials, EEG, the orientation reaction, arousal regulation). There is therefore a broad coverage of current research in attention. There are some omissions: very little is to be found on observing responses, though there is much current active research using this technique; nothing on perceptual sampling theory (e.g. Kristofferson).

Omissions are, however, almost inevitable in a book of this kind. As Mostofsky admits in the author's preface, 'this work does not pose as a definitive handbook on attention'. Another self-criticism is that 'Noticeably lacking is a single point of view, an articulate coherent, unifying argument'. While this certainly is the case it is not a criticism which a reviewer could justifiably make. Attention research is not in such a state that a single explanation will suffice, or even the various points of view be easily unified. More serious, I feel, is the lack of any emerging *patterns* or *themes* in reading through the book. While attention may not, at the moment, mean one thing, it should not mean 16 different things. If so, its usefulness really is to be doubted. There surely was room for occasional intervening comments on the part of the editor, attempting to show what these main themes are. It is not, in my opinion, sufficient to provide the reader with an 'eclectic assemblage' simply because it most faithfully mirrors the confusion of research in the area.

The papers by Boring were originally destined to be full historical perspectives, but, with the advent of his untimely ill health and death, ended up unfortunately as mere transcriptions of correspondence between him and the editor. The few pages that remain are written in that familiar lucid style, though their content inevitably fails to live up to the stated aims of 'making it possible for others to pick up the thread... [of research directions]'. As to the rest, most of the papers present recent research data, the standard of presentation being consistently high.

The 18 papers are split up into three parts: Systematic Issues, Perceptual and Learning Issues and Neuropsychological Issues. The allocation of papers to the various parts seems slightly arbitrary, except for the last group, but the blank pages between parts do at least provide the reviewer with a natural break. In part I, after Boring's sadly brief but enjoyable tit-bits, Mostofsky discusses definitions of attention and their shortcomings, and illustrates, with experiments by Margolius, his contention that attention goes beyond stimulus control. Berlyne dis-

cusses attention in a pronounced philosophical manner, then, more characteristically, in terms of 'attention attractiveness' properties of stimuli (novelty, intensity, etc.). Some recent experiments by McDonnell add weight to his argument. Also in this group are papers by Broadbent, Silverman and Hochberg. Broadbent makes the important point that a set for picking out one stimulus rather than another only helps if the stimuli are physically differentiable (different colour, male/female voice). Letters and digits in the same type fount can only be differentiated by using a *response* set. Some of his data are not, however, entirely convincing. Silverman presents some results on sex differences in attentional style, though a more analytic approach might have produced more tangible data. The paper by Hochberg is possibly the best thing in the book, bringing together work in selective attention, speech perception, visual search, ambiguous figures and many other paradigms. His essentially 'cognitivist' approach to attention emphasizes the extent to which perceptual organization is a natural function of basic well-practised skills.

In part II, Jerison presents a readable account of vigilance, though he adds little new to his various earlier reviews on the topic. Lindsay's contribution concerns the processing of multiple input streams. This is certain to increase in importance as a research field, but it is clear that a great deal more needs to be done before a workable model may be constructed. The rest of the papers in this section involve attention in animal discrimination learning. Mackintosh shows how attention failures with inconsistent reinforcement schedules can be regarded as underlying probability learning behaviour. Honig describes some sophisticated experiments on stimulus control in generalization situations. Jenkins and Sainsbury describe some pigeon studies on discrimination of distinctive features. Most interesting is the finding that the actual location of the peck response closely reflects the location of the distinctive feature in the stimulus pair. The last paper in the group is by Warren and others, extending the Sutherland–Mackintosh model to cats. They, like monkeys, do not apparently behave like rats.

The final group of papers, part III, concerns neuropsychological issues. Luria and Homskaya discuss regulation of arousal processes, including the orienting reflex, by frontal lobe activity. Tecce gives an excellent and comprehensive account of evoked potential findings in relation to attention, and shows how little we really do know of the significance of this tantalizing neurophysiological concomitant of behaviour. A useful, more general review of underlying processes is provided by Thompson and Bettinger, while Mirsky and Pragay discuss EEG changes of pharmacologically attention-impaired monkeys. The final contribution in the book is by Haider, and is an all-embracing, though rather speculative, account of neural mechanisms underlying attention, expectation and vigilance. Some overlap is found here with some of the previous papers in the group.

All in all, though, a worthy collection of research, theory and thoughts on attention and related topics. Certainly the book belongs in every university library, and researchers in the area will perhaps welcome the opportunity of sampling other approaches than the one they follow.

G. R. J. HOCKEY

Image Formation and Cognition. By MARDI J. HOROWITZ. London: Butterworths. 1971. Pp. xiii + 351. £5.80.

The author is a research psychiatrist and a psychoanalytically orientated therapist who is concerned with visual imaging as it presents itself in medical–psychological practice. The book is mostly devoted to his own clinical and experimental studies, some already published in journals, but these are supplemented by useful general considerations of imaging and of cognitive–affective controls and by generous bibliographies which give the book a textbook flavour. The intended readership is not clearly evident but two classes of reader will find value in the book. First, clinicians will be stimulated by the emphasis on visual representation and by the concluding description of interaction painting, i.e. the use of graphics as a technique for communicating with disturbed people, especially those who are withdrawn or verbally inexpressive. The reviewer wonders, for example, if this technique could facilitate contact with some autistic youngsters. Secondly, students of visual imaging will find new descriptive material and a complement to conventional academic literature about visual thinking. Such readers would be helped if the book were called 'The formation and function of unbidden images', and were written more selectively to this theme. Unbidden imagings are those suddenly intrusive visions which disturb and sometimes haunt certain people. These visions are here subjected to several ingenious investigations

and interpreted within a causal biological and psychoanalytic framework. They may be rare in the populace at large and are perhaps best known at second hand from fiction such as Shakespeare's *Macbeth* where, in the reviewer's experience, most spectators seem ready to give the visions an agreed dramatic and psychological function. However, unbidden images are not confined to fiction. They are real and raise interesting questions which are not altogether intractable. This is indicated by the author's account of clinical cases where such visions are treated as symbolically expressive of a patient's preoccupation and where their gradual elucidation is held to be both diagnostic and therapeutic. Other unbidden imagings are reported in the author's experiments on intrusive visualizations which follow the viewing of traumatic films, his experiments on experiences elicited by electrodes implanted in human brains, and his accounts of 'flashbacks' experienced by users of hallucinogenic drugs long after immediate biochemical effects have worn off. The author would be the first to warn that these investigations are beset by methodological problems; and such warnings are necessary because the drama inherent in these experiences could sweep a reader toward injudicious generalizations. Nonetheless, these investigations are a promising advance on most experimental studies, which tend to make imaging dull, and on most speculative essays which tend to make imaging capricious. The author's studies of unbidden imaging, coupled with his considerable clinical experience and his eclectic commentary, contribute seriously to the study of an aspect of imaging which has hitherto received less concentrated empirical attention than it deserves.

I. M. L. HUNTER

Optics, Painting and Photography. By M. H. PIRENNE. London: Cambridge University Press. 1970. Pp. xxiv + 199. £4.00.

This interesting and well-produced book is written by a physiologist distinguished for his work in vision. The book is not, however, primarily on the physiology (or indeed the psychology) of vision but rather on perspective and how we should think of retinal images. There are excursions into historical theories of perception – Kepler and Scheiner – and the use of perspective in art. The author's father was an artist: this book evidently represents a long-term interest in this aspect of perception. Of particular interest are excellent pin-hole photographs. These are of cubes, cylinders and spheres (peripheral spheres project as ellipses) and dramatic 'pin-hole perspective' pictures of the Temple of Neptune at Paestum. There are also unusually interesting comments on optical properties of the eye.

To a psychologist, there is surprisingly little reference to the significance of perspective for perception, and almost total omission of reference to size or shape constancy. (The constancies are absent from the index, but do receive a very brief mention in the text.) This book will be found useful to psychologists concerned with visual perception, as many of the facts are not represented in psychology texts; but some work is required to make use of them, as theoretical guides are, perhaps deliberately, not given by the author.

R. L. GREGORY

The Growth of Word Meaning. By JEREMY M. ANGLIN. Cambridge, Mass. and London: M.I.T. Press. 1970. Pp. xiii + 108. £2.80.

The Growth of Word Meaning by Jeremy Anglin is an M.I.T. research monograph which is concerned with the development of the internal lexicon. The author states that his object in carrying out the research was to offer 'a fairly detailed picture of *how* the internal lexicon evolves rather than to provide a complete explanation of *why* it evolves'. The approach adopted is largely descriptive, therefore, and involves a series of cross-sectional developmental studies, in which the major technique employed was the word-sorting task developed by Miller, although the free recall procedures of Bousfield were also employed, and free association techniques, and other methods borrowed from Bower and Bruner. The tasks were administered to children and adults, using a set of twenty words which included six nouns, four prepositions, five verbs and five adjectives. Each of these subsets included words which were semantically related at increasingly abstract levels. The principal findings from the studies were that adults are more likely to preserve the form class distinctions in their sorting of the twenty words, and are more likely to take account of abstract properties than are children. The monograph is clearly written and presented, is relatively non-technical, and is useful as an account of the results of applying a number of techniques to the study of semantic memory.

P. H. K. SEYMOUR

The Computer Simulation of Behaviour. By MICHAEL J. APTER. London: Hutchinson. 1970. Pp. 180. £1.75; paper, 75p.

Psychology, as always, is in search of new and more powerful concepts and languages for tackling the immensely complex task of describing and understanding how the human mind works. Some of the most important additions to the repertoire come from the fields which deal with the problems of information processing. The dissemination of these ideas is a slow process and it sometimes takes place – as is often the case with new ideas – against active resistance. In this situation the careful sifting of fads and fallacies from the real contributions, and a historical evaluation of progress during the last 10 or 15 years become urgent and important tasks. Michael Apter is the first in this country to undertake the job, and probably one of the most recent within the English-speaking world. The book thus sets out to do a very worthwhile job indeed. How far does it succeed?

The book is described by the author as a non-technical introduction, aimed at students of psychology, philosophy, the general reader and 'computers'. Its subgoals are listed as an examination of the mechanist hypothesis, an evaluation of the usefulness of simulation, a description of a set of representative models which are either classics in the field or typical contributions, an indication of the range of existing models, and to give a guide to the literature.

The reviewer feels that the book tries to do too much in too little space. For this reason it suffers in intelligibility, or else lapses into mere catalogue-like listing of programs and projects without detail or evaluative comment. At least one important piece of work is described, however, in some detail in each chapter. A more serious deficiency is the very compressed and too elementary introduction to programming, since some appreciation of the actual computer implementation of these models is really essential for their evaluation by the reader. Non-numerical computing, the essence of simulation, is not dealt with in the book at all extensively, and a single page on list-processing techniques is tucked away in the chapter on natural language models. Nor is the reader helped by any references to books, manuals or papers on non-numerical programming languages like IPL-5, LISP, POP-2, and so on. If the book succeeds in making psychologists interested in simulation, it fails completely to set them on their way to becoming active in this field. The chapter on digital computers and programming should have been given a completely different emphasis.

The chapter on cybernetics gives some historical background and describes the relation of simulation to artificial intelligence and cybernetics. It is the reviewer's impression that the beauty of systematic connexions between these fields is more in the eye of the author than in the realities of the literature. Some 'artificial intelligencers' would not take too kindly to the suggestion that their work is a subset of cybernetics.

It would have been important to indicate prominently in the preface or introduction how recent in time the coverage of the field is intended to be. The reader may be misled into believing that the book is an up-to-date presentation of the state of the art. This is, alas, completely untrue. While this does not affect the inclusion of such real classics as the Newell, Simon and Shaw General Problem Solver, Samuel's work on game playing, and Selfridge's ideas on pattern recognition, the picture given of natural language processing and question-answering includes neither classics nor representative current work. One would have expected a mention of the work of Raphael, Quillian, Bobrow and Evans.

To the reviewer's taste, less philosophy and more practical information would have made this into a better book. For use as a text at university level the book has too much of a journalistic flavour and relies on popular secondary literature rather than original papers.

Keeping these limitations in mind, the book can be well recommended for use in undergraduate psychology courses, but should be supplemented with a more up-to-date reading list.

GEORGE KISS

Human Factors Applications in Teleoperator Design and Operation. By EDWIN G. JOHNSEN and WILLIAM R. CORLISS. New York and London: Wiley-Interscience. 1971. Pp. xii + 252. £6.25.

'A teleoperator is a general purpose dexterous cybernetic machine.' A teleoperator must have a man in control, though not necessarily in full control, and it must permit to some extent the projection of man's dexterity across physical or spatial barriers. Examples of teleoperators include wearer-actuated artificial arms, lunar vehicles steered by a man on the earth, remotely controlled devices for repairing underwater pipelines, and devices controlled by man for applying large forces or manipulating dangerous materials. A robot is not a teleoperator, nor is any form of pre-programmed machinery such as an automatic record changer.

From the above, it is obvious that teleoperators are relatively recent, rapidly developing, and likely to expand greatly in their uses and applications in the next decade or two. This volume provides definitions of teleoperators, and classifies the applications which they have at present. It distinguishes, on an admittedly arbitrary basis, ten subsystems characteristic of teleoperators, each of which can be defined functionally and in terms of hardware, and lists nine kinds of interfaces between subsystems. The design principles for each of these subsystems are discussed, and much of the book consists of a detailed treatment of control subsystems, sensor subsystems, and actuator subsystems. These topics apparently reflect the main enthusiasms of the authors, as well as predominating emphases in the existing literature, for the treatment of them is much more stimulating than the rather dull approach of the introductory chapters.

This volume adopts a fairly narrow interpretation of human factors. As a result, it is not of such immediate practical interest to psychologists as it could be. Engineering themes are dealt with more thoroughly than psychological ones. Many topics of apparent relevance, such as the use of transfer of training principles in teleoperator design or the possible role of operant conditioning in learning to use prosthetic devices, are mentioned only in passing or not at all. Some of the most interesting issues raised, such as whether the principle of trying to replicate man's dexterity in the teleoperator is the correct one to follow, are not developed or discussed with reference to basic concepts. The emphasis is on displays and controls and the description of dexterous behaviour. The psychologist reading this book has to deduce the relationships between psychological concepts and teleoperators since these are not generally made explicit. For such a reader, the book may be thought-provoking rather than directly instructive.

Sometimes the treatment of a topic lacks balance. For example, a problem with an artificial arm, particularly an elaborate one which requires extensive learning by the amputee before it can be fully utilized, is that the man must fully accept his artificial arm as an extension of himself and have wholeheartedly favourable attitudes towards it before he can be expected to devote the hundreds of hours necessary to learn to realize its full potential. The arm must be acceptable not only in its dexterity but also in its appearance, so that ideally a casual observer will fail to detect that it is an artificial limb. These problems of cosmesis are critical for the widespread acceptance of teleoperators in the form of prosthetic devices, but the treatment of such problems in this book is brief and superficial. It could justifiably be claimed that this neglect is merely a reflexion of the existing literature which also fails to deal with cosmesis adequately, but a book which ignores such issues can scarcely purport to provide comprehensive coverage of relevant human factors applications.

On the whole, the book is well-written. It records faithfully the American work on teleoperators, but the authors are apparently unaware of some of the European work, and therefore the bibliography, though extensive, is not complete. The book contains numerous tables, lists and definitions which are one of its most valuable features. However, it fails to provide either a list of these tables or any separate indication of their contents or even their existence, and since the index is not adequate for this purpose much of the value of these tables is lost. The illustrations are, for once, well chosen, and more could, with advantage, have been included. The price is yet another indication of how serious the problem of inflation has become in this country.

V. DAVID HOPKIN

Measurement of Man at Work: an Appraisal of Physiological and Psychological Criteria in Man–Machine Systems. Edited by W. T. SINGLETON, J. G. FOX and D. W. WHITFIELD. London: Taylor & Francis. 1971. Pp. xiii + 267. £6.00.

This book is the edited proceedings of a symposium held in Amsterdam in September 1969. Such works nearly always present peculiar difficulties to the reviewer, since they usually want homogeneity of style and merit, and the several contributions frequently overlap in content. Save that the papers are all of great merit in content, this is no exception; indeed the amount of repetition makes it somewhat tedious to read consecutively. However, one of the stated aims of the editor is to make it a work of reference; and as such it is successful.

The book consists of an introductory survey paper by Professor Chapanis, 24 papers by other eminent ergonomists, laid out in three sections and two 'conclusion' papers. The three sections are 'Man' (six papers), 'Techniques' (11 papers) and 'Applications' (seven papers). Each section is provided by the editors with a useful 'prologue' and 'epilogue'. Sad to relate, in a book on ergonomics, the first page of each 'prologue' is printed on paper so strongly coloured as to reduce the readability to an unacceptably low level.

In his introductory paper, Professor Chapanis remarks that 'ergonomics' corresponds roughly to what is called 'human engineering' in the U.S.A. He observes that there are differences of emphasis on the two sides of the Atlantic – the Americans being more inclined towards psychology, the Europeans towards physiology – but that the similarities are much greater than these differences. He forcefully makes the point which, disappointingly, is little touched upon in the rest of the book, that 'Safety and survivability (*sic*) are complex criteria...not neat and simple linear transformations of...basic research'.

The difference between sections 1 and 2 is less than their titles would suggest. They would, perhaps, better be called 'general techniques', and 'particular techniques'; although even so there seems to be a certain arbitrariness in the selection and order. In any event, Dr Edwards' lucid and concise paper on 'Techniques for the evaluation of human performance' should logically be the first, not the eighth, paper in its section.

These, however, are relatively minor criticisms. The 'techniques' papers will certainly prove very valuable to researchers in the field, for each provides a sound introduction to its particular topic, and is fortified by lavish references. I was particularly interested by Dr Chiles's paper on measurement in complex tasks, and by Dr Rolfe's discussion of the use of secondary tasks. Dr Chiles's emphasis on the importance of conducting studies in a situation at least closely comparable to the work situation is timely. Every applied psychologist must at times have pondered on the terrifying specificity of experimental results; but there is still a tendency to carry out precise but stylized laboratory tasks and to hope that the results will transfer adequately to the factory floor. Dr Chiles firmly reminds us that that is just not good enough.

Dr Rolfe rightly calls our attention to the careless way in which secondary tasks are often used, but does not quite convince me of the incomparable merits of adaptive loading methods. Indeed, a general difficulty which arises in attempting to test in a work situation is that the very testing alters that situation – the 'Hawthorne effect' is a well-known instance of this. Dr Rabideau makes this point central to his plea for 'non-interfering' techniques, though his specific suggestions might turn out to have difficulties of their own. Somewhat surprisingly, he does not discuss the use of semantic differentials in this context.

A more serious omission generally is that the whole book contains only the scantest references to motivation. This is the more surprising since Professor Singleton discusses arousal mechanisms at length, and there is surely a connexion between the two.

The two papers of concluding remarks survey a wider field. Dr Sinaiko wonders why so many American studies have been of 'the more glamorous tasks...of the aircraft pilot or man-in-space.' I don't wonder why; since I know that, until recently, the aerospace industries have disposed of lavish funds for such research, which, in other areas, were wanting. However, both he and Dr Williams make a number of very shrewd points about the aims of ergonomic studies. Scientific statements are ethically neutral, but the applications of science are not, and ergonomics is very much an applied field. They do well to remind us that the criteria of success in ergonomic studies include the satisfaction of work people.

In conclusion, it should be said that this book is well produced (except for the silly coloured

pages already mentioned), the diagrams are clear, and errors few. Its price is a bit steep, even bearing the current inflation in mind; but most professional ergonomists, at whom it is aimed, will want to have a copy within easy reach.

M. HAMMERTON

Environment and Human Efficiency. By E. C. POULTON. Springfield, Ill.: Thomas. 1970. Pp. xxi + 328. $15.50.

This book is concerned with providing a description of man's capabilities and limitations for performing a variety of tasks in a variety of environmental conditions, and lays particular emphasis on ways in which human efficiency can be improved.

The book contains 25 chapters and the average chapter length is 13 pages. The two longest are concerned with a simple model for human performance and with simultaneous tasks. There are five very short chapters, each of only six pages, which are concerned with measuring the effects of environment, arousal, cold, decompression and old age. There are 74 beautifully produced figures and 255 references, about two-thirds of which were published during the 1960s and of which about one-third were authored or co-authored by past or present members of APRU, Cambridge. In his introduction, Poulton explains that in the interests of readability the selection of references was ruthless and that his aim was to be representative rather than comprehensive. Both indices are reliable and the subject index is extremely comprehensive.

Throughout, the book eschews speculation and controversy, and with the exception of a chapter presenting a simple model of human performance, based on that put forward by Broadbent in *Perception and Communication*, and a straightforward chapter on arousal, is almost entirely devoid of theory. It relies on a lucid presentation, in a simple and direct style, of well-established data together with a clear analysis of the experimental design problems, such as dealing with asymmetrical transfer effects, involved in establishing the effects of a particular environment or of pairs of environments on efficiency. Some of the shorter chapters, notably the one on old age, are perhaps rather superficial in their treatment of the topic in question but overall the book is a useful compendium of information on what is known about stress and efficiency and on how the effects of stress can be obviated. It should be of particular interest to experimental psychologists, ergonomists and industrial engineers. It is to be hoped that it will soon be available at a cheaper price, perhaps in a paperback edition, although it must be admitted that the present edition is extremely well-produced.

D. R. DAVIES

A Primer of Physiological Psychology. By R. L. ISAACSON, R. J. DOUGLAS, J. F. LUBAR and L. W. SCHMALTZ. London: Harper & Row. 1971. Pp. vi + 295. £4.15.

In a note to students at the beginning of this book the authors indicate that its purpose is to provide an introduction to physiological psychology even for those without a knowledge of biology beyond 'O' level. The book treats the subject matter at a level of complexity greater than that of the introductory volumes of Teitelbaum and Butter, but with much less documentation and detail than conventional texts, e.g. Milner's *Physiological Psychology*.

There are nine main sections. The first is an introduction which adumbrates the area of study. After this there are three chapters which set the tone of the book. They deal respectively with cells, nervous system anatomy and its evolution. These topics are presented excellently and provide first- and perhaps second-year students with a background that is essential for the adequate comprehension of more advanced material. The five remaining sections cover sensory processes, motor systems, memory and learning, emotion and motivation and higher processes. I have two reservations with these. First, certain topics are emphasized to the virtual exclusion of others, e.g. in chapter 8 there is a long and interesting discussion of emotion but no mention is made of all the work that has been done on self-stimulation. Second, that although experiments are described, there is no bibliography to enable students to pursue their particular interests in greater depth. There are, also, anomalies in the index, e.g. the work of both Peiper and Olds is referred to but does not appear in the index.

Despite a number of faults, this is a book which fills an important gap well and is to be recommended for that reason. However, the cost of £4.15 for a 295-page volume shows a strong desire to keep ahead of inflation.

ANDREW MAYES

Information Theory. By J. F. Young. London: Butterworth. 1971. Pp. viii + 168. £3.00.

The preface to this new book on information theory states that it aims for a 'unified approach', evidently unblinkered by parochial single-subject allegiances. It is by implication written in awareness of the contributions of many disciplines, including psychology, by an author who describes himself as 'an engineer working in the field of psychology'.

It is the more disappointing to find that there is surprisingly little here that the psychologist will either recognize or identify with. He might even be moderately alarmed by the naive view of things psychological that occasionally intrudes. Thus within a few lines (p. 57) we meet an overstatement about the fallibility of the human signal receiving system and the unfortunate inference about redundancy that 'one half of written English is there... to ensure that sentences are correctly constructed'. Similarly, commenting (p. 75) on the dismal record of the human operator as a component in a communications channel, Young implies, but fails to capitalize on the suggestion, that some sort of maximum channel capacity for man could be 'deduced from functional data'. There surely is an opportunity for the unified approach; but instead of working it through in information theoretic terms, the author follows it up with the proposition that man's informational underachieving could have had some evolutionary value because it is sometimes useful to be a slow responder.

Surprise and information are consistently linked throughout the book. While this is of course perfectly proper it is frequently disconcerting, since they are surely not psychologically related in the same simple sense. Undoubtedly momentary puzzlement will be generated by some of the odd causality relationships in which surprise features (e.g. surprise caused at the transmitter). If this usage is intended to assist the psychological reader then it is merely misguided; if it is not then it reflects an ignorance of psychology's sensibilities and represents a further failure to achieve the 'unified approach'. Exactly how out of sympathy with psychology the author seems to be can be judged from his comment that the exercise of constructing approximations to English is 'not very fruitful'.

In fact, were it not for the prefatory implication about its multidisciplinary appeal the book could be very comfortably described as one for engineers, with some human interest. Certainly central and non-mundane concepts like bandwidth, phase shift and signal power are introduced so cursorily that unless he is a communications engineer the reader may be led unprofitably to check either the index for clarification or his own intellectual equipment for functional failure. An occasional competence in engineering mathematics is needed, although he can be ignorant about probability, since this is explained, rather inelegantly, in two appendixes.

The mathematics, which is not in general especially demanding, is almost continuous and varies between an over-elaborate and somewhat pedantic concern with algebraic detail and verbal formulae to some rather cavalier manipulations with dimensional arithmetic (p. 96) and Fourier series (p. 71).

The excellent and extensive bibliography effectively stops short at 1964 but ranges over Bar-Hillel, Chomsky, Garner, Hick and Miller. On the other hand, they are not mentioned in the text and in their absence the psychologist could perhaps be excused for being inhibited by the algebraic territory that he must otherwise explore alone. The spirit of controversy is in consequence missing. In any case there are few enough places to seek it in this book; the introduction is brief and historical and there is no concluding perspective.

The inquiring mind, ostensibly encouraged by the preface (again), could be forgiven for finally baulking after 30 pages at the quotation which follows; to the reviewer it is as priceless as the book is over-priced: 'In order to obtain the actual information per symbol received, the total amount of the prior information per symbol possessed by the recipient must be subtracted from the information per symbol which the recipient would have received if he did not already have the prior information.'

PAUL J. BARBER

Neuropsychopharmacology and the Affective Disorders. By JOSEPH J. SCHILDKRAUT. Boston: Little, Brown. 1969. Pp. xiv + 111. £4.50.

In this very short monograph (some 57 text pages, exclusive of appendixes) Professor Schildkraut presents the view that an examination of the pharmacology of drugs used in the clinical treatment of the affective disorders may lead to a better understanding of the neurochemical bases of these disorders.

The first part of the book is orientated towards the experimental psychopharmacologist. A brief review and reclassification of affective disorders leads on to a concise account of the various kinds of drugs used in the treatment of disorders of affect. This is followed by a discussion of the changes in biogenic amine metabolism which have been associated with manic and various types of depressed states.

The remainder of the book is directed mainly towards the clinician, there being a chapter on the differential diagnosis of the depressive disorders and their various methods of treatment, followed by appendixes dealing with such matters as drug dose ranges, possible side effects and contraindications.

Professor Schildkraut's manner of exposition is crisp and, in a breathless way, attractive. The book can be read from cover to cover in a matter of two to three hours yet seldom can the author be accused of resorting to superficiality in the pursuit of brevity. Statements made in the text are always fully supported by reference to a useful list of original sources.

The proposed reclassification of depressive states presented in chapter 1 differs little, in fact, from the American Psychiatric Association scheme; reasonable arguments are put forward in support of such changes as are suggested.

The account of drugs used clinically in the treatment of affective disorders leaves something to be desired. Attention is limited almost entirely to the monoamine oxidase inhibitors and tricyclic antidepressants and little mention is made of other drugs (for example, phenothiazine-related compounds such as doxepin) which do not belong to these categories but which are, nevertheless, proving useful in the control of depression, particularly when the depression is accompanied by other symptoms such as anxiety. Passing reference is made to lithium salts but the remark that 'the effectiveness of lithium salts seems well documented only in the treatment of hypomania and mania' is no longer supportable in the light of recent very clear demonstrations (not subject to the objections on grounds of statistical methodology which were levelled at previous studies) of a prophylactic action of lithium in the treatment of recurrent depression. It is, of course, inevitable that in a field of research which is developing with such rapidity a book such as this should be already out of date in many important respects. Unless it is Professor Schildkraut's intention to produce updated editions of his monograph at regular intervals (and the next edition will be required very shortly) a review of this nature might be regarded as more appropriately presented in the more ephemeral context of a journal article rather than in book form, and indeed the greater part of this monograph is a revision of material previously presented in the *New England Journal of Medicine*.

The outline of the neuropharmacology of drugs used to treat affective disorders is a thorough and well-organized review of the state of knowledge as it existed in 1969. Later work not covered by the author would modify some of the details but would leave the overall picture basically unchanged.

A criticism of the survey of metabolic idiosyncrasies of patients showing disorders of affect is that it is limited to a consideration of the biogenic amines. Some account of alterations in electrolyte and total body fluid balance might also have been valuable.

I feel that that section of the book (the last chapter and the appendixes) which has been specifically written for the clinician is somewhat less satisfactory than the remainder of the book: there is much here which might have been omitted without great loss. Thus a prescribing physician would be unlikely to need to refer to this particular source for guidance about drug dosages, and the information has no great relevance to any other section of the readership. Nor can I see much point in listing, without comment, the chemical formulae of the major antidepressant drugs (though one welcomes the fact that, in contrast to at least three recent psychopharmacology texts, the structural formulae are correctly given). The lists of drug side effects contain an enormous number of possible metabolic and physiological dysfunctions but no indication of their relative likelihood of occurrence, information which, though notoriously difficult to assemble,

would have added greatly to the value of the lists. I am surprised to find that the author makes no mention of the strict precautions which are vitally necessary in controlling the dosage and administration regime of lithium salts, a serious omission which any later edition of the book should rectify.

The greatest limitation, not only of this book in particular, but also of the whole field of biochemical neuropsychopharmacology in general, is revealed in the section directed towards the therapist. The presenting symptoms of affective disorders are, in the first instance at least, exclusively behavioural-cognitive, not biochemical, and the therapeutic drug of choice is that which effects appropriate behavioural-cognitive changes, whatever the associated biochemistry might be. Professor Schildkraut's guidelines for the differential diagnosis of depression, based on the overt behavioural symptomatology of the patient, suggest that the description of drug effects in psychological conceptual terms, a level of analysis neglected in this monograph, would be of more practical value to the clinician than descriptions couched exclusively in biochemical terms. In any case, the relative coarseness of the biochemical changes following drug administration (altered biogenic amine turnover rates, total brain depletion of specific neurotransmitters, increased excretion of metabolic breakdown products, and the like) as compared with the complexity and subtlety of the psychiatric syndrome, suggests that it may be very difficult to establish any kind of simple relationship between the two sets of concepts which would have practical usefulness. The author's expressed hope that 'pharmacology may ultimately provide a critical bridge between neurochemistry and clinical psychiatry' finds little support in his book.

Whatever one's personal views might be about the likely practical pay-off of neurochemical studies, however, Professor Schildkraut's little book is as stimulating a volume as I have read in recent years: the author's evident enthusiasm for his subject enlivens every page and for that reason alone I would recommend this book, were it not for its quite astonishingly unrealistic price which will ensure its place amongst the esoteric literature of our time. F. N. JOHNSON

Drugs and Human Behaviour. By GORDON CLARIDGE. London: Allen Lane The Penguin Press. 1970. Pp. 266. £2.40.

This is a good book, though not the 'pioneering study' that the publishers claim. It is at first difficult to assess, for they give no guidance at all about the audience for which the book is intended. However, Dr Claridge's own clear and robust if not always aesthetic style makes this plain within the first few paragraphs. He is writing for a popular audience of non-specialists – indeed, of those, one would guess, who have little knowledge of scientific techniques of any kind. He keeps firmly to this self-appointed road: except that one may question, for example, whether this audience would know the meaning of such terms as 'bandwidth' that he occasionally uses (and, in passing, may one wonder whether the use of this term in the context is justified at all). Again, although most of the illustrations are beautifully chosen for their simplicity the comparison that non-scientists or scientists are required to make between graphs 7.15 and 7.16 may tax some of each a little, to say the least. On the other hand, he is writing about human work, and that is certainly easier to understand and – to speak prejudicially – more relevant too.

Dr Claridge is also cool and compassionate. He is a clinical psychologist of distinction as well as a true psychopharmacologist, i.e. one who has been cradled in both arms of this nowadays seemingly all-embracing discipline. Perhaps this explains how he has managed to write a book in which the effects of drugs upon the emotions are discussed in unemotive terms and upon society in sociable ones.

Dr Claridge is certainly at home in both disciplines. It is difficult to fault him on his choice of topics: placebo and environmental factors are given due importance (although there is no mention of the Rosenthal effects); sedation threshold is given rather a large share, but the author has made important contributions to this field. On the other hand, if something appears at first sight to be missing, it is usually because the relevant entry is missing from the index. 'Motivation', for example, is not mentioned, but any point-scorer anxious to fault the author for such a glaring error will find himself out of court on pp. 92 and 126 at least.

Perhaps best of all (apart from the usual pleasure one has in handling books from Allen Lane), the work provokes questions, even about well-worn topics. For example, first, if, as Claridge points out on p. 141, an individual's tolerance of drugs (under the conditions of his own experiments –

note the excellent qualification) was 'much more a function of temperament than of tonnage': why should our attempts to reduce the number of road accidents not include, in addition to the breathalyser, a test of extraversion–introversion rather than reaction time or simulation tests, the relevance or difficulties of which respectively are such as to make them inappropriate. The relevance of this measure to certain appropriate kinds of behaviour as well as its connexion with alcohol response-tendencies are sufficiently well established to make it at least as valid an indicator of guilt as the blood alcohol level alone. Are psychologists not sufficiently confident of their instruments that they are prepared to recommend that the alcohol content weighted by E/I score would be even more just than the crude alcohol content?

Second, though it may be no more than a coincidence that there are two 'type A and type B' personality typologies (the second of which Claridge does not mention) – that of DiMascio and his colleagues describing drug responders and that of Betz and Whitehorn describing different kinds of therapist – this may be a basis for putting the often-made joke that it is better for the doctor to take the drug than for his patient to do so on to an experimental footing. After all, Starkweather and others have shown that the effect of a drug taken by one's partner before performing certain tasks can be greater than that taken by the subject himself. But in the clinical setting there are as yet one or two suggestive anecdotes but not the controlled study that is clearly and seriously overdue. Good reasons for the therapist to experiment with drugs may thus include not only the need to acquire possible insight into the mental processes of his patients, but also to influence them, to interact with them and, it may be, in certain cases to induce inertia where enthusiasm is too great.

Finally, on drug dependence and measures for its so-called control. Such drugs as heroin are controlled by medical prescribing only to known users and setting strict legal penalties on improper possession by all others; alcohol is controlled by setting legal limits to the age and time of consumption and for the concentration permitted in the blood in very particular circumstances; tobacco, like alcohol a drug of such high, known toxicity that its continued sale makes a mockery of the Committee on Safety of Drugs, is controlled merely by providing impersonal information about its long-term personal and social consequences. These are three very different approaches. Why not try to combine the best features from them all? For all three the ingestion of information, provided by relevant and effective methods, should be made compulsory; exceeding a defined concentration in the blood of any of these drugs or of its metabolites should lead automatically to prosecution in such specified circumstances as impending or frequently repeated parenthood, possession of a responsible job in society, etc. Should the user also be required to write a prescription for himself each time he wished to use one or other of these three drugs? In doing so, he might eventually come to realize that they are really inseparable on all counts from drugs prescribed by his doctor, and that most of our drug use, medical and social as well as illegal, is not merely irrational but finally idiotic.

C. R. B. JOYCE

Testing Freudian Concepts: an Experimental Social Approach. By IRVING SARNOFF. New York: Springer. 1971. Pp. xi + 276. £8.50.

In this book Sarnoff examines some experimental designs with which, he hopes, it will be possible to get to grips with psychoanalytic theory and put it to the rigorous scientific test. His ideas are, in the main, illustrated by examples drawn from his own work and that of his students. This means, in effect, that a small number of studies is scrutinized in great detail. This, in its turn, ensures that the reader fully understands the rationale of and the thinking behind the designs.

According to Sarnoff, this careful study of the methodology of a few experiments will be useful for the development of good research designs in quite different fields from the investigation of Freudian theory so that the book may be regarded as a primer of research methods and, its main use, as a stimulus for good research into psychoanalytic theory.

To what extent are these two aims fulfilled? As a guide to methodology in general experimental psychology, especially where the problems of measurement and definition of concepts are severe, this book seems to this reviewer to be highly useful. Those who know Sarnoff's work will recognize, in all the studies examined, the extremely ingenious derivation of hypotheses (too ingenious perhaps), the imaginative experimental manipulations (who else would suggest a dramatist's skills are useful in scientific psychology?) and the careful attention paid to the mental health of

subjects. Sarnoff is admirably aware of his ethical responsibilities as an experimenter. Indeed, students who study this book could well become imaginative researchers.

However, as regards the second aim of testing Freudian concepts, this book is disappointing. Sarnoff admits the modest scope of the work, that it only presents principles of experimentation on Freudian concepts and that it omits examination of component parts of the theory. Yet this omission is most serious since, as Farrell (to whom no reference is made) has long argued, Freudian theory is not a unitary theory but a collection of parts each of which needs testing. Thus the principles discussed by Sarnoff, and exemplified in his and his students' work, are not so easily applied to all parts of Freudian theory.

This limitation to Sarnoff's own work is emphasized by the fact that no studies are quoted referring to such cardinal points of theory as repression or the Oedipus complex and little is made of the opportunities afforded by cross-cultural investigations. Thus even as regards principles of experimentation relevant to Freudian theory, it seems to this reviewer that the book is too limited and, as a result, too general, perhaps a consequence of attempting to make the book a general primer in research design.

In conclusion, this book gives good advice on how to devise imaginative experimental researches for a small number of Freudian concepts – advice which would be useful with similar non-Freudian investigations. However, large areas of Freudian psychology which involve different problems are not discussed. This means that this book could not possibly serve as an updated Sears, as a guide for anyone who wished to know how much of Freudian theory had been proven or disproven. At £8.50 it would seem, in this country, to be a very expensive way of developing research skills and it is to be regretted that Sarnoff chose to limit this book to a study of the problems besetting his own work.

PAUL KLINE

Psychotherapy and the Modification of Abnormal Behaviour. By HANS H. STRUPP. New York: McGraw-Hill. 1971. Pp. xii+209. £2.85; paper, £1.90.

Unfortunately, the title does not convey the primary intent of the book: it is not until the title page is reached that the reader becomes aware that this volume is 'An Introduction to Theory and Research' in the field of psychotherapy. There is no good reason for omitting this information from the main title and adding it as a postscript.

Strupp commences with a chapter on the nature of psychotherapy, in which such topics as 'The role and function of the psychotherapist' and 'The essence of psychotherapy' are covered in a page and a half. Chapters 2, 3 and 4 deal with psychoanalytic therapy, Rogerian therapy and behaviour therapy in 14 pages each. Other forms of psychotherapy, including psychodrama, child psychotherapy and family therapy are described in a fifth chapter. Although these sections contain passages entitled 'Advantages' and 'Criticisms', the discussion is incomplete and the level of analysis is extremely superficial.

Despite the fact that 'The concept of transference is the pivot upon which psychoanalytic therapy turns' (p. 27), there is no discussion of the validity of this phenomenon when the author turns to his section on criticisms. Moreover, psychoanalytic therapy is defended on the grounds that it can be differentiated from the frequently criticized psychoanalytic theory of personality because 'the actual operations of psychotherapy are not nearly as closely articulated to the theoretical substrate as the proponents would have one believe' (p. 33). It is a strange defence that disclaims its vulnerable supports without substituting adequate alternatives. Also, it is difficult to envisage a therapy in which the course taken and the information transmitted are totally independent of an underlying rationale. The criticisms of behaviour therapy are largely reiterations of those stated by Breger and McGaugh and assume a largely unwarranted naïveté on the part of contemporary behaviour therapists.

Two chapters are allocated to the topic of research in psychotherapy, the first concentrating on critiques of studies by Eysenck and by Paul, and the second providing a more general appraisal of the problems which beset investigations of therapeutic effectiveness. This latter chapter, in which the author identifies and discusses the relevant variables of therapist, patient and technique, is the most useful section in the book. The main text concludes with a short discussion of the present state and possible future of psychotherapy, followed by two appendixes which deal with professional problems of ethics and training.

With the exception of the general section upon research problems and the appendixes, the relevance of the latter being debatable in the present context, there is no chapter whose function could not be better served by the suggested readings given throughout the book. In the author's preface it is explicitly stated that this volume is an introduction to the field of psychotherapy and is designed to acquaint the undergraduate student with the subject matter of the area. In attempting to satisfy these aims, the relative ignorance of the undergraduate appears to have been confused with intellectual incompetence. This confusion has resulted in a degree of superficiality which discourages recommendation of the book for undergraduate students of psychology. There may be other student groups who require only a very limited appreciation of psychotherapy, for whom this volume would be appropriate: a paperback edition is available at £1.90.

A. T. CARR

Community Psychology and Mental Health: Perspectives and Challenges. Edited by DANIEL ADELSON and BETTY L. KALIS. Scranton, Penn.: Chandler. 1970. Pp. vii + 341. $8.00; paper, $4.75.

Community Psychology: Perspectives in Training and Research. By IRA ISCOE and CHARLES D. SPIELBERGER. New York: Appleton-Century-Crofts. 1970. Pp. xv + 285. £3.90.

These two important books together provide a comprehensive survey of the growing subdivision of psychology which in the United States has been given the label 'community psychology'. Community psychology emerged as a distinctive movement in the mid-1960s primarily because more and more psychologists had come to recognize that the course content and practical training provided by the then existing psychology training programmes contained little that could possibly prepare the next generation of psychologists for the new roles and complex social demands that were likely to confront them. There was also a growing tendency amongst psychologists to insist that psychological theories developed largely on the basis of studies of laboratory animals and college sophomores required testing in much wider contexts.

In addition, the limited applicability of the majority of psychological research findings to community problems despite most government spending had also become apparent. So community psychology finally crystallized as a reaction to the growing frustration and dissatisfaction with academic psychology that many applied psychologists experienced in their day-to-day activities. The term 'community psychology' was coined in 1965 during a conference of psychologists who came together to discuss the problem of educating psychologists for work on community mental health programmes. Community psychology was seen by the conference participants as a distinctive and challenging area of psychology which needed to attract psychologists who were willing and able to both participate in and contribute to programmes concerned primarily with the 'betterment of the human condition'. In the following year a major stimulus was given to the movement towards community psychology by the publication of the American Psychological Association position paper on the formation of community mental health centres. This paper, drafted principally by Brewster Smith, clearly outlined the implicit philosophy of community psychology. New roles were advocated for psychologists and other specialists active in the mental health arena. Instead of concentrating on individual clients they needed above all to concentrate on helping the various social systems of which the community is composed to function in ways that could develop and sustain the effectiveness of the individuals who took part in them. In the case of breakdown by an individual within the community, intervention would involve helping the community systems to regroup their forces to support the person who ran into trouble rather than focusing on the person directly.

Many of the writers who contribute to these two books were equally influential in launching community psychology and some of the more seminal papers were in fact originally contributed to the conferences between 1965 and 1967 which culminated in the A.P.A. recognizing community psychology as a major division of its activities. Since over 20 authors contribute to each book, it is an impossible task to do justice to the strengths and weaknesses of their views. However, it is necessary to point out that, although there is a degree of overlap between the two books in terms of both authors and subject matter, there is a difference in emphasis between them. Thus Iscoe

and Spielberger concentrate primarily on issues which relate to training in community psychology. The opening sections of the book discuss the historical developments and social needs leading to the emergence of community psychology, and secondly some of the differences between traditional and community-orientated approaches to mental health problems and how they affect training programmes. The remaining sections of the book specifically evaluate existing training programmes for community psychology.

Adelson and Kalis's book pays more attention to the theoretical issues involved in community psychology and less to the question of training. Thus the two books are largely complementary. Iscoe and Spielberger provide what is essentially a handbook for psychologists interested in either pursuing training in community psychology or gaining ideas and insights concerning such programmes. Adelson and Kalis provide a more polemical and evangelical book which takes academic psychology firmly to task.

Community psychology is undoubtedly here to stay, but whether it will be able to provide a coherent developing body of theory and action remains an open question. These two books do much both to establish its credentials and to challenge psychologists to come down from their self-imposed exiles in their ivory towers.

ANDREW TREACHER

Kinesics and Context: Essays on Body–Motion Communication. By RAY L. BIRDWHISTELL. Edited by BARTON JONES. Philadelphia: University of Pennsylvania Press. 1970. Pp. xiv + 338. £3.75.

Ray Birdwhistell invented the term 'kinesics' and started recent research on communicational aspects of bodily movements with his *Introduction to Kinesics* (1952). The present volume is an edited and selected version of Birdwhistell's main writings since then, put together by one of his students, Barton Jones. In addition to previously published papers there are 94 pages of Birdwhistell's contribution to the long-awaited (but not yet published) *Natural History of an Interview*, and a reprinting of the elaborate notational system from *Introduction to Kinesics*.

Birdwhistell was originally an anthropologist, who later trained as a linguist. His method of research has been to study short pieces of film of everyday encounters, and look at them in great detail – at one time he spent 100 hours on each second of film. He developed an extraordinary sensitivity to the detailed structure of events, and a number of examples of his analysis are given in this book. He treats body movement as a kind of language and has been trying to decode it. He has classified small bodily movements into a large number of 'kines'; equivalent kines are called 'kinemes', and he now thinks there are 50–60 of these for Americans, 32 of them in the face and head area. Kinemes occur in larger patterns, simultaneously or in sequences, called 'kinemorphs', which in turn are part of larger units. He has been studying the way these units fit together, what they communicate, and how they relate to what is being said at the time. Birdwhistell does not do experiments, present quantitative data, use statistics or test hypotheses.

He has, however, come to a number of conclusions, which are necessarily rather loosely supported by the kind of data he collects. The main ones are: (1) bodily movements have no standard meanings, but depend for their meaning on the context; (2) while kines are built up into longer sequences of movements there does not seem to be the equivalent of a grammar for kinesics; (3) bodily movements, including facial expression, do not have a biological basis, but are the result of socialization; (4) bodily movement occurs in particular patterns selected by the culture and bodily movements are a function of the social system; (5) bodily movement is not necessarily less important than verbal communication and does not act only as a modifier, but may communicate the more important message. In addition, Birdwhistell makes a number of more detailed observations about different types of message that may be sent – providing illustration, punctuation, making moves in an interpersonal relationship, etc.

The main criticism that can be made of this approach is that propositions such as those listed above are not strongly supported by the data provided. There is evidence against proposition (1) from recent cross-cultural studies by Eibl-Eibesfeldt and Ekman; there are also gesture languages in particular communities. Recent collaboration between ethologists, ethnologists and social psychologists has led many people to an interest in the evolutionary origins of body movement. Birdwhistell appears to have been very little influenced by those outside his immediate circle; although it is too early to speak of a grammar for kinesics, the patterning of speech, gaze

and head-nods found in experimental work by Kendon and others at least indicates some of the rules. Other experimenters have found that some bodily movements do more than just communicate; shifts of proximity, orientation or gaze, for example, have a considerable effect on what can be perceived. Another approach which is not mentioned is the analysis of social behaviour as 'social action' which is meaningful and rule-governed, as proposed by the symbolic interactionists and others.

Nevertheless, it was Birdwhistell who first drew the attention of many researchers to the importance of bodily movements, and this book is the most useful single source of his writings. The study of this book, with the examples analysed and the coding systems presented, would probably make a reader more sensitive to the subtleties of bodily movement. It would act as a useful corrective to the approach of most linguists, though some of them have been influenced already. As Abercrombie said in a recent paper, 'we speak with our vocal organs, but we converse with our whole body'.

MICHAEL ARGYLE

Personality and Leadership Behavior. By HENRY P. KNOWLES and BORJE O. SAXBERG. London: Addison-Wesley. 1971. Pp. xii + 164. £2.40; paper, £1.75.

According to the authors of this book there is a world crisis in human relationships deriving from the individual's sense of loss of identity before the massive power of institutions. This crisis is reflected in industry, and their book, directed to students and practitioners in management, describes an approach to leadership to meet this crisis.

The book falls into three main parts. In the first three chapters, the social and political theories of writers from Cicero to Maslow are reviewed to demonstrate how the underlying assumptions about human nature which they adopted shaped their conception of the role of institutions in the regulation of behaviour. Knowles and Saxberg contend that in management the leader's basic orientation towards other people similarly determines the mode of control he adopts, and hence shapes the quality of human relations in the organization.

In the second part of the book, the authors develop their own view of personality, a self-theory, which synthesizes the culture–personality theories with phenomenological theories. They distinguish a 'social self' which is the product of shared cultural experiences, thus providing a consensual view with others of this aspect of self, and a 'personal self', which is a private and unique view of the self deriving from our personal experience, and which may deviate from the social self. By increasing the congruence between personal and social self we provide greater opportunity for personal development, since we can be open and authentic in our relations with others, and thereby gain greater self-awareness.

In the final four chapters the authors indicate how this approach to personality can be applied to human relations problems in the organizational setting. They stress the importance of self-awareness and sensitivity to others as a basis for creative work relationships, and the need for openness to change in effective management. A chapter on 'Altering the Self' gives a review of the various methods which may be adopted to improve sensitivity to self and others, including illustrations of case-study methods, role-playing procedures and laboratory training groups.

Knowles and Saxberg have deliberately opted against an examination of the empirical evidence for the relationship between personality, styles of leadership and group effectiveness, and adopt instead an explicitly value-laden advocacy of their viewpoint. As a result, the reader is left in the position of asking what evidence there is that their approach to leadership is effective. Reference is made to the works of Argyris, Likert, Vroom and McGregor, but there is little indication of what may be found there. There is an absence, also, of any reference to the psychological literature on leadership, and no account of how the success of styles of leadership may be evaluated.

An unsatisfactory feature of the book is its lack of systematic references. There is a bibliography of 54 book titles at the end of the book, but a number of these authors are not mentioned in the text. On the other hand, references are made in the text to authors for whom no source is given. Thus G. H. Mead and Lecky, from whom many ideas on the self are drawn, are listed in the references but not mentioned in the text, while Weber, whose importance for traditional organizational ideology is stressed, has no source for reference given in the bibliography.

In spite of its shortcomings, the book should provide provocative reading for management trainees. The challenge to traditional business values, and the casting of managers in a priestly

role concerned with the betterment of man and improving the world may not convince them, but may increase their awareness of their own value systems and attitudes to their roles. If the book does that, the authors should be well satisfied, for that, they say, is the aim of communication.

JOHN SHEPHERD

Personality in Social Theory. By PATRICKE JOHNS HEINE. Chicago, Ill.: Aldine. 1971. Pp. x + 197. $7.50.

This is an intelligent and enjoyable little book (181 pages of text), although for *psychologists* the title is somewhat misleading and the publisher's 'blurb', however unwittingly, misrepresents the contents. In essence the author is concerned with the conceptual problems which arise in any attempt at a systematic analysis of the relation between the individual and society. The one chapter in which she tries to tackle experimental studies (in relation to conformity) is weak, and one feels that in part it is offered as a token sacrifice to the twin gods of empiricism and methodology. Nevertheless, even that chapter illustrates the basic problem which interests Dr Heine: on the one hand any examination of the individual in relation to the group or society tends to reduce to a discussion of individual *versus* society; on the other hand, to isolate the individual and society as wholly different units is fundamentally artificial and hampers our understanding of either. To overcome this artificial analytic separation, Dr Heine looks to role theory. The greater part of the book is therefore devoted to a re-examination of 'individualistic' concepts, such as 'personality', 'trait', 'character' and 'self' in terms of the socially and sociologically derived notions of role theory. However, Dr Heine is by no means uncritical of that theory; she also considers its inadequacies in accounting for the 'uniqueness' which is implicit in such individualistic concepts as, for instance, 'self'.

The orientation throughout is sociological and theoretical (and often interestingly speculative) rather than psychological and empirical. This is where the title is, perhaps, a little misleading: psychologists in particular might expect some examination of personality theories, but they will find no more than occasional allusions to traits such as dominance or to achievement motivation; psychological theories and research on personality are for all practical purposes ignored. On the sociological side, Dr Heine assumes knowledge of social theories. She does not, with the exception of role theory, provide a review or detailed analysis of these. None of which would matter, were it not for the 'blurb'. This claims that the book shows 'the links of the individual with society along the perimeter of personality theory' and 'integrates psychological and sociological theory'. It doesn't, and Dr Heine herself, in her foreword, is more modest and accurate in describing her work as an 'essay'. That is what it is, an intelligent and stimulating commentary and discussion on the problems of studying and formulating ideas about the relation between the individual and society. It is a book which helps one to think: it is not a source of 'reference' as the blurb proclaims. Ultimately, though Dr Heine does not put it this way, it is concerned with the direction of causality from individual to society, and *vice versa*. I found its lucid but questioning approach valuable and refreshing in its own right: it is perhaps time that publishers rid themselves of the obsession that everything must be a 'text'.

PETER KELVIN

The Unresponsive Bystander: Why Doesn't He Help? By B. LATANÉ and J. M. DARLEY. New York: Appleton-Century-Crofts. 1970. Pp. xii + 131. $6.60.

In recent years mass-media reports of lawlessness and social disintegration in American cities have caused us growing concern, not least because in looking westward we fear we are peering through a crystal ball in which we behold our own future. We are told that urbanization ultimately spells dehumanization, that megalopolitan man is socially alienated and indifferent to the fate of his fellow citizen. From New York, Chicago and Los Angeles, in shocking testimony to this view, come reports of robberies, rapes and murders committed in the full view of onlookers who stand idly by and do not intervene. It is appealing to regard such unresponsive and socially irresponsible behaviour as symptomatic of a malignant social disease, to throw up our hands in horror and insist that we would behave differently, but the authors, pursuing an empirical approach to the question, take a more charitable view.

Their monograph is the 1968 winner of an annual award offered by Appleton-Century-Crofts for distinguished manuscripts in psychology, and follows in an eminent line of predecessors, including Rimland's *Infantile Autism* and Neisser's *Cognitive Psychology*. In it they bring together a dozen field and laboratory experiments, five of them previously unpublished, which explore step by step the situational factors that facilitate or inhibit intervention to render help in a crisis. As they point out in the preface, they have taken a considerable risk in aiming the book at both a lay and a professional audience. The result could have been disastrous (and indeed some readers may find the presentation patronizing) but the reviewer found it lucid and elegant, with a simplicity that made for rapid and compelling reading. If there is a price to pay, it is that the authors must often be taken on trust, and the reader is not infrequently referred for additional detail to previously published reports of experiments.

The experiments themselves are simply and sometimes ingeniously conceived, and frequently invoke the shades of Candid Camera; the dramas enacted extend to robberies but not to rapes or murders! In the case of the laboratory experiments the reviewer was left with lingering doubts about the subjects' gullibility despite the authors' claims for the success of their manipulations, but the field experiments are more immediately convincing. The major result to emerge consistently over a wide range of situations is striking. Although subjects steadfastly denied that they were influenced by the presence of other bystanders, such presence drastically reduced the likelihood that they would step in to help in a crisis, often to the point where a victim in need of aid was most likely to receive it if only one onlooker was present. The authors attribute this inhibitory effect to two factors: first, initial mutual inaction will predispose bystanders not to interpret the situation as an emergency, since no one else present seems to regard it as one; secondly, responsibility will be diffused amongst those present. The explanation is plausible but fails to account for the marked reduction in inhibition when the bystanders know either each other or the victim.

Although the authors' five-stage model of the intervention process is not entirely persuasive, their data appear to go a long way towards refuting the social alienation account of the unresponsive bystander. The fact remains, however, that whatever situational factors are operative some individuals give help in a crisis and others do not, and it is tempting to postulate consistent individual differences in this respect. It comes as a disappointment to find only one experiment in which such differences are investigated, but here there is a suggestion that an individual's readiness to intervene and give help may be related to the size of the community in which he was reared. In the end, then, our suspicions about the evils of urbanization are reawakened, and the original question is far from closed. This compelling little monograph is highly recommended.

JOHN D. DAVIS

The Psychology of Moral Behaviour. By DEREK WRIGHT. Harmondsworth: Penguin Books. 1971. Pp. 288. 35p.

The appearance of this survey reflects an increasing interest during the last decade. The painstaking work of Hartshorne and May in the 1920s has only recently been extended; experimental studies of 'guilt', 'altruism' and the like are now commonplace. It reflects also a growing acceptance of the idea that psychological theories should not needlessly denigrate man. Wright's common-sense, and humanistic bias pervade this account of the empirical work on moral behaviour – 'all the various things people do in connection with moral rules' (p. 15).

Wright follows convention and discusses six main topics – resistance to temptation, reaction to wrongdoing, altruism, moral thought, ideology, and character. One of the best chapters, on delinquency, is appropriately sandwiched between resistance to temptation and reaction to wrongdoing. The topics are conventional; their treatment is not, for an admirable degree of integration is achieved. Wright's eclectic theoretical disposition and his refusal to take authors' conclusions too seriously are responsible for this. Social group theory, learning theory, psychoanalysis and the 'cognitive-development approach' (i.e. Piaget) are used to link areas and investigations. Descriptions are restrained, factual, pertinent and mostly comprehensive. Occasionally, however, the evidently sparse knowledge does not really justify the rounded and predominantly soothing conclusions.

'Character' coordinates the other topics. The child is seen to be influenced by two (opposed) forces: of adults, and his peer group. 'Authoritarian' and 'conscientious' types are the product of

predominantly adult influences. When the peer group is uppermost, 'conformist' and (more speculatively) 'collectivist' orientations are the result. Both the 'amoral' and the 'altruistic-autonomous' types escape domination by either parents or peers. The former never succumbs, and the latter outgrows both. Inevitably we know more about the amoral than the autonomous type.

This oversimplified but interesting scheme illustrates Wright's attempt to bring order into the fragmented area of moral behaviour. Research on delinquency, for example, is highlighted by other studies; the role of intellectual development given proper place. Occasional omissions and exaggerations do not seriously detract from a text at once carefully documented, theoretically valuable and obviously applicable. This is a good book, and I shall be surprised if it does not achieve Penguin's normally thwarted aim of satisfying layman, student and teacher alike.

GEOFFREY M. STEPHENSON

Imitation in Children. By PAUL GUILLAUME. London: University of Chicago Press. 1971. Pp. xvii + 214. £3.60.

Forty-five years have passed since the first appearance in France of *L'Imitation chez l'enfant*. The circumstances surrounding the publication of this translation are outlined by Bernard Kaplan in a ponderous foreword. He believes that the psychological climate at that time was very much like the present – a world in which 'anti-instinctivists were challenging those who seemed ritually to invoke instinct and preformation; exponents of structure and system were at odds with the atomists and elementarists; mechanists and behaviourists had locked horns with proponents of teleology and mentalism; and those opting for discontinuities of form and function in phylogenesis and ontogenesis were controverted by those who insisted on continuity'. If you see this similarity, then this is a book for you; if you don't, nonetheless it may still be worth reading. Its importance lies in the fact that, as Aronfreed has recently pointed out, there has been no major breakthrough in the psychology of imitation. As one of the few attempts to deal with the processes involved in true imitation this work cannot be overlooked. It made sufficient impression on Piaget, despite its totally different orientation, to receive several mentions in *Play, Dreams and Imitation*.

Instead of thinking of imitation as a primitive fact Guillaume's concern is with mechanism. A theory of the role of imitation in the learning of children from birth to three years evolved from detailed observation of his own two children, and a comparison of his findings with those of other studies is presented. As case study material goes it does not escape the usual criticisms. Observations are poorly organized, with objective and subjective material fused in a narrative presentation.

Guillaume develops his views on imitation by, first, considering the 'Psychological Aspects of Movement'. He discusses the conflict between the unfolding of the action and the appearance of the image, the role of instinct and habit and the principle of trial and error. In the next section on 'Vocal Imitation' a theory of instincts is again examined and he concludes that we must look for a cause other than instinct. 'Imitation is no simple phenomenon. It is a transposition, a translation of what the child hears, in phonetics that are familiar to him.' The role of vision in verbal imitation and the results of studies of deaf-mutes and blind people are assessed. The third major focus is the 'Imitation of Movements'. At last a definition of what is meant by imitation appears. This section is followed by 'Awareness of Imitation' and the 'Affective Aspects of Imitation'. A final section on imitation in animals sets the work in context and provides a useful comparison of Guillaume's speculations with other more broadly based data.

The most provocative section of the book is that on self imitation in which an important though frequently overlooked point is raised, namely how can the infant realize that his movements and those of the model are identical when he has never direct visual perception of his own movements as a complete image?

The work concludes, naturally enough, with an essay on the problem of instinct. A predeterminist notion of instincts is rejected, Guillaume's observations pointing to the existence of instincts which experience and tradition make more precise.

This is a classic of its kind, but not one that has withstood the test of time unscathed. We

have become more sceptical about this type of data and many of those old burning issues have since lost their heat or changed in nature. Imitation, however, still has not moved much beyond the hunch-chasing stage. Here are some of the hunches.

MARY E. CROXEN

Childhood Behaviour and Mental Health. By MICHAEL SHEPHERD, BRAM OPPENHEIM and SHEILA MITCHELL. London: University of London Press. 1971. Pp. 240. £3.40.

At a time of general concern with problems of mental health and behaviour disturbance, of professional rethinking about remedial measures and of administrative reorganization of the helping services, this report of the Buckinghamshire survey of children's problems is welcome. It is also rather dangerous in that it might be cited by the uncritical as providing reasons for curtailing rather than improving existing services.

The authors – a professor of epidemiological psychiatry, a reader in social psychology and a senior lecturer in sociology – define the four main purposes of their investigation as: (1) to describe the nature and prevalence of several types of conduct commonly believed to indicate emotional disorder; (2) to study the duration of such behaviour; (3) to identify the factors which distinguish 'problem' children who are referred to child guidance clinics from 'problem' children who are not so referred; (4) to evaluate the extent to which 'improvement' in behaviour can be attributed to treatment rather than to natural development or environmental changes.

For the first of these purposes a sample of 6000 children aged between 5 and 15 was drawn, constituting a fairly representative 10 per cent of the population of Buckinghamshire state schools in 1961. Even though the two ends of the range of social background and parental intelligence are under-represented (by the omission of children in private schools and by dependence on parental response to a written inquiry), this still gives a far more diverse sample than those of earlier studies of problem incidence such as Macfarlane's at Berkeley and Gesell's at Yale. Data were collected by questionnaires to parents and teachers, both distributed through the schools. Whereas the teachers were merely asked to underline items from a list that described the child 'as he often is' and to supply information concerning attainments, the parents had a 68-item form to complete, including many assessments of behaviour on three-point or eight-point scales. The eight-point frequency assessments are fairly objective, but most of the three-point scales have as their middle value a statement saying or implying 'about the same as most children of this age'. The authors admit that many parents do not know what is typical behaviour at a given age; but they ignore the magnetic effect of such a wording, and of the socially desirable response generally. This effect is strongly suggested in the graphs showing incidence of some of the problems, but no check on it is possible in the absence of a probing interview with a sub-sample, which was not undertaken. There is a strong probability that the incidence of the less socially acceptable problems has been underestimated for this reason as well as from the non-return of forms mentioned on p. 33. It is quite likely that falsification, or misperception, will occur with different frequencies according to social class and according to the child's sex and ordinal position in the family, since these are known to affect parental attitudes: measures of association of these factors with 'total deviance scores' are therefore suspect. Association with age, and with some other factors not closely related to parental attitudes, may be less seriously affected, and some of these findings are interesting. Indeed this whole section could have been legitimately presented as a study of what parents (chiefly mothers) perceive, and are willing to report in writing, about their children, rather than as an account of what children actually do. Significantly, teachers see and report very different things: only one child in five classified as deviant on the parents' questionnaire was so classified on the teachers', and *vice versa*. True, children do behave differently in different settings; but the sifting of the objective from the perceived differences is a difficult task not yet undertaken.

The second objective of the inquiry was approached by means of a follow-up study in which the same questionnaires were used again after two and a half years with the parents and teachers of that 10 per cent of children who had shown statistically deviant behaviour on four or more items, and of a contrast group showing deviance on not more than one item. On the assumption that any misleading response tendencies would tend to operate constantly, this longitudinal information can be accepted as probably valid, and as complementary to the cross-sectional age

trends already reported. More children had reduced than had increased their deviance scores on the second occasion. The observations on the types of problem most likely to clear up, to persist, and to develop in boys and girls respectively, are valuable. There is an interesting sidelight too on the stressful effects of the 11+ selection year.

The third objective, to identify factors associated with child guidance referral or non-referral, is clearly important. Since only nine of the 6300 children in the original sample had received psychiatric treatment, it was necessary to recruit an additional clinic sample of 50 children. Their parents were interviewed and completed the survey questionnaire at the beginning of their clinic attendance. On this basis each child was matched for age, sex and main problems with a survey child, but this involved leaving out as unmatchable 27 per cent of clinic children, including those with the most serious problems. Even among the matched groups, the clinic children were rated as somewhat more disturbed and had suffered more separation experiences, bereavement and family disruption than the controls. The most striking differences, however, lay in the parental attitudes: control group mothers tended to regard their children's problems as unavoidable or physically caused and to feel able to 'soldier on', whereas those attending clinics admitted to feeling worried, helpless or unable to cope. While such an admission would certainly be expected from parents attending a clinic for the first time, it does throw into relief the urgent need of some parents for help with their anxieties as well as with their children.

This brings us to the last objective, the evaluation of child guidance treatment, which was pursued by follow-up interviews of the clinic and control groups after two years, with blind ratings for improvement or deterioration made by three child guidance workers. The finding of no significant difference between the groups is by now well known. Further analysis tends to confirm another well-attested finding that brief therapy (five sessions or fewer) may be more often effective for the milder type of problems studied here than either longer term treatment or none at all. The omission of severe disturbances is again emphasized by the authors, who also admit that they have not attempted to assess the effect of work with parents on the family atmosphere as a whole. The vital importance of this aspect of the work is borne out in the progress of treated and untreated groups alike. In the combined sample persistence of the child's problems was significantly related to persisting physical and mental ill-health in the mother, irritability in the parents and stressful relationships in the family. In such cases continued support and help would seem to be needed, often over a long period.

Where a child's disturbance is not too severe there may be a case for concentrating resources almost exclusively on work with his parents and teachers (educational psychologists should note that the last finding reported is of considerable improvement in a number of children following changes in the school situation). But this cannot be decided from the present study since the child's own point of view has been deliberately excluded. In defending this omission the authors (p. 22) quote a transatlantic suggestion 'that children might tell an interviewer that they had frightening dreams when there had been no evidence of this in their behaviour'. If the child is not to be the judge of whether his dreams have frightened him or not, neither apparently is he to be asked whether he feels better for child guidance treatment. Behaviouristic criteria demand that he be treated not as 'thou' but as 'it'. But children can get just as desperate about a family tangle as can their elders, and their claims to professional help in managing their feelings cannot justly be denied on the grounds that their views do not readily meet the requirements of objective research.

TERENCE MOORE

On Helping the Dyslexic Child. By T. R. MILES. London: Methuen Educational. 1970. Pp. ix + 70. £1.05.

The author starts from the position that 'the term "dyslexia" (or some word of equivalent meaning) is a necessity if certain sorts of backward readers and spellers are to be adequately helped'. He finds that the use of the term enables both parents and teachers to realize that dyslexia means a disability in reading which requires special understanding, thereby making assistance more constructive. The aim of this book is to help teachers to understand and deal with the condition.

The first step is to identify the dyslexic child as distinct from what one might call the 'normal' backward reader. This is no easy task for a teacher. As Professor Miles himself points out, survey

of errors made in reading and spelling may throw little light on this, since many poor readers show the same types of errors as the dyslexic. He himself proceeds in the matter of diagnosis by the method of exclusion. Thus children with reading difficulties who are dull, or suffering from emotional shock or worry, those with a history of school absence or poor school teaching (surely difficult to establish when past as well as present teaching has to be taken into account) or those with defects of the special senses are excluded from the category of dyslexic. But when failure is limited to special areas (particularly reading and spelling), when there is something genuinely *puzzling* about it, then one possibility among others (not specified) is that the child is dyslexic. The teacher is in this case advised to look for positive signs, of which the author lists as most important directional confusion, both in reversals of letters and sequence of letters in words, a tendency which has to be persistent, continuing up to the age of ten years or beyond.

He also cites very strange spelling, a feature, however, which is so commonly found in most children backward in reading that it is unlikely to be of much assistance to the teacher investigating dyslexia. Sometimes the handwriting is awkward. The other signs, e.g. at times not recognizing a word even though it has been shown a few moments before, difficulty in repeating longer words orally, are not likely to be of much assistance, since these may merely indicate poor attentional habits such as one is likely to get in a fair proportion of backward readers. The teacher is also not likely to feel a great deal of confidence in the use of standardized tests: perhaps wisely Professor Miles has pointed out their shortcomings rather than their assets, since a page and a half is all too scanty to deal with this method of diagnosis. The aetiology of the condition is likewise disposed of in about two pages, the author stating that for practical purposes it does not seem necessary to have a knowledge of the details of what causes dyslexia, and concluding that 'the causes are not known in detail, but the condition is almost certainly connected with some small failure of development in the brain'.

The remainder of the book is then devoted to the question of teaching the dyslexic child. As to general method, the guide-lines for teachers contain some sensible tips, e.g. to find out what the child feels about it all, to change standards of what is easy and difficult, to start with regular words and get the child to learn what to do by following rules. Professor Miles himself follows the specific method of phonetic cues, one of the main features of which is that the child should say the word very carefully and should pay special attention to the movements of his own tongue, lips and throat in doing so. He advises against the Look and Say (save for the occasional child) on the grounds that these children have difficulty with visual marks (i.e. letters and words). Hence he uses the same method with all but a small minority of dyslexics. The rest of the book consists of a presentation of his phonic method (unlikely to be very new to primary school teachers who have any acquaintance with the teaching of phonics), a sample phonic dictionary and a set of exercises, e.g. 'I had a big pig'. One is left with the uneasy notion that this umbrella sort of method may be used without any shaping to the needs of individual children, and that the material may possibly need a big sweetener in the form of a lively teacher, which some children are lucky enough to get in the form of Professor Miles himself.

GERTRUDE KEIR

Intelligence and Personality: Their Assessment and Relationship. By ALICE HEIM. Harmondsworth: Penguin Books. 1970. Pp. 206. 30p.

This is not a book that many psychologists will read with complacency for, in pursuance of her general theme of 'the essential oneness of the human being' and the need for experience as well as overt behaviour to be accepted by psychologists as valid, indeed essential, evidence, Dr Heim delivers a series of vigorous attacks on some broad conceptual standpoints and specific tests and methodologies. Nor is the attack confined to academic issues. The morality as well as the applicability of much animal experimentation is criticized and there are scathing comments on the attitudes that some psychologists apparently take towards their human subjects.

Dr Heim disapproves of schisms and dichotomies. Psychology, she maintains, is not the proper realm for their promotion. 'Discrete variables are the exception rather than the rule.' Some might question, however, whether discrete variables are commonly found in any field of science outside the artificial environment of the laboratory and her general stand on dichotomies is both open to and worthy of further debate. In pursuing her argument, however, Dr Heim has many thought-provoking comments to make on such divisions as intelligence and personality, mental health

and mental illness, intelligence and creativity, heredity and environment. The plea for a broader approach to the study of man also leads to interesting discussions of other important and diverse issues, including multiple-choice questions, validation, sex differences and the psychological aspects of ageing. The Brook Reaction Test and the Word-in-Context Test are described in some detail and presented as evidence for the practical as well as scientific usefulness of a broader approach to testing.

A novel feature is the inclusion, at the end of the book, of notes on the technical terms used to which the reader may refer for further explanation. Whilst most of these are descriptive, others, such as those on 'attenuation', involve further critical analysis and there is a danger that this could be missed by psychologists familiar with the terms.

This is no cool, dispassionate account and, written as it is in a forthright and, at times, angry style, it is likely to elicit some equally forthright replies. If these lead to a re-examination of some basic concepts and precepts, I suspect that the book will have achieved one of its main functions.

B. A. AKHURST

Multivariate Data Analysis. By WILLIAM W. COOLEY and PAUL R. LOHNES. London and New York: Wiley. 1971. Pp. x + 364. £4.50.

In this book the authors have attempted to enlarge upon and enrich the material presented in their earlier volume, *Multivariate Procedures for the Behavioral Sciences*. Much of the success of that work lay in the way in which the mathematical exposition and computer implementation of multivariate techniques were closely integrated. In the context of the mid-60s and the development of computing facilities such a text was most helpful.

The present book is organized in three parts. The first contains a short introduction to multivariate procedures and outlines the basic mathematical and statistical foundations from which one can proceed to the more complex analyses. From this early stage the reader is encouraged towards an understanding of the techniques for the translation of his ideas into a viable computer program. Basic FORTRAN programs are presented and exercises involving the amendment of these programs are provided.

Following this the authors launch into a discussion of those multivariate techniques appropriate to the study of the single population. From multiple correlation they progress to principal component analysis and other factor analytic procedures, finally considering canonical correlation and multiple partial correlation.

The final section of the book considers the extension of the multivariate approach into the area of multiple population studies. Following an outline of multivariate analysis of variance and discriminant analysis, there is a chapter on multivariate classification procedures. This section concludes with presentation of multiple covariance analysis and factorial discriminant analysis.

This outline of content will indicate that the book covers a selection of multivariate and associated techniques of importance to those involved in the field of survey research. The authors have as their objective a presentation of a selection of what they consider to be the most relevant techniques, rather than an inclusive catalogue. However, their bias is towards 'criterion oriented' rather than 'inductive' use of the procedures they outline, and this could well be rather unsatisfactory for many readers. In addition, despite an Introduction and Conclusion which dwell on some of the more general issues of the integration of multivariate techniques and research problems, the text leaves many of the basic questions raised by the application of multivariate analyses unanswered.

The presentation of each chapter follows a roughly similar pattern, each consisting of the mathematical background, followed by a numerical example and concluding with a research example relevant to each technique. These research examples are based on a selection of data from the TALENT project which are included as an appendix. This has the advantage of providing a link between the various techniques. For those who are prepared to follow through the programs provided these data are a useful introduction to the application of multivariate techniques, and the integration of theory and practice.

It is difficult in such a text to maintain a level of explanation which can satisfy readers with a variety of backgrounds, in both research experience and mathematics. The authors have managed to approach the essential requirements in most areas, but there is an imbalance in this respect

between some of the chapters. Perhaps one has to take refuge in the adequate reference material which they recommend.

In the period since the publication of their earlier work computer techniques have vastly advanced, and economical programming of these complex techniques has become the field of the expert rather than of the researcher himself. Indeed, most of those involved with such techniques have access to 'statistical packages' and to experts who will quickly and economically amend them to their own particular requirements. The time for the integrated text in this area may well have passed, but there is probably still a space for this book on the shelf of those approaching involvement in multivariate experimentation.

P. BRITTON

Readings in Extraversion–Introversion. Volume 3: *Bearings on Basic Psychological Processes.* Edited by HANS J. EYSENCK. London: Staples Press. 1971. Pp. 640. £6.75.

What can one say about a book of readings? This is not designed as an instructional volume, nor is it a bringing together of hitherto unrelated work. It is what it claims to be: a set of readings in extraversion–introversion. In this third of three volumes we have a broad range of inquiry. There are five sections, headed: physiological, sensory stimulation and deprivation, perceptual, motor movements, and conditioning and learning. There is little theoretical discussion. Some of the key issues appear in Volume 1, but to find the full story, one has to go to *The Biological Basis of Personality* and back even to *The Dynamics of Anxiety and Hysteria.* The key strength in the book is its demonstration of the ubiquitous influence of individual differences. There are few areas in hardcore experimental psychology where the influence of individual differences has not been demonstrated. There is a warning here therefore for those, not primarily interested in personality, who may be unaware of the hidden source of error variance which may be accounted for in a systematic manner. How many an unwanted baby has been thrown out with the experimental bathwater? Those of us who do work in the personality field will have copies of the papers already. For the beginner, there are many exciting possibilities examined in depth or referred to in passing. There is a source of worry though. The work reported here constitutes the empirical backbone of *The Biological Basis of Personality.* It is a strange backbone: some parts are made of sprung steel; others, alas, of spaghetti. Many of the papers look substantial and robust, but others (two or three pages in length) look suspiciously like one-off, single-shot pilots. What happened afterwards? Were these tantalizing titbits of complex behaviour sampled only once? If so, then it is a shame, for the questions asked are rarely trivial. It is inevitable that the range of topic examined is broad, for Eysenck's theoretical viewpoint is comprehensive. At times one wants to know more and one has to look elsewhere for the answers. Thus the book does not really stand on its own. However, some controversies are dealt with quite fairly here and both sides are represented. Generally though, the aim has been to demonstrate the sheer range of work done. Only those who have not worked in this field, coped with the problems of experimentation and handled the data which emerge, can fail to be impressed. Virtually all the work reported was done before 1967. Will there be a fourth volume in a year or so, to bring us up to date? Hopefully, it will contain a higher proportion of review-type articles, so that we can assess the current position. Hopefully also, the trend in prices will be curtailed: Volume 1, £4.00; Volume 2, £4.95; Volume 3, £6.75 – considerable outlay for a personal library and little possibility, I suspect, of a paperback issue.

ANTHONY GALE

Br. J. Psychol. (1972), 63, 1, *pp.* 145–147
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CHANGE-OVER TO SI UNITS

From January 1972 the Journals of the British Psychological Society will adopt the International System of Units (SI) based on the units: metre, kilogramme, second, ampere, Kelvin and candela. Further information about SI units is contained in the revised *Suggestions to Authors* pamphlet issued by the Society and obtainable, price 25p (U.S.A. $1.00) post free, from Cambridge University Press.

Authors who refer to physical measures in their paper should now normally use SI units; common units of time (e.g. hour, year) will, of course, persist. Conversion tables will be published during the change-over period, but in the *British Journal of Psychology* these will be restricted to units of length, as others rarely occur. When they do, conversion factors will be stated in footnotes.

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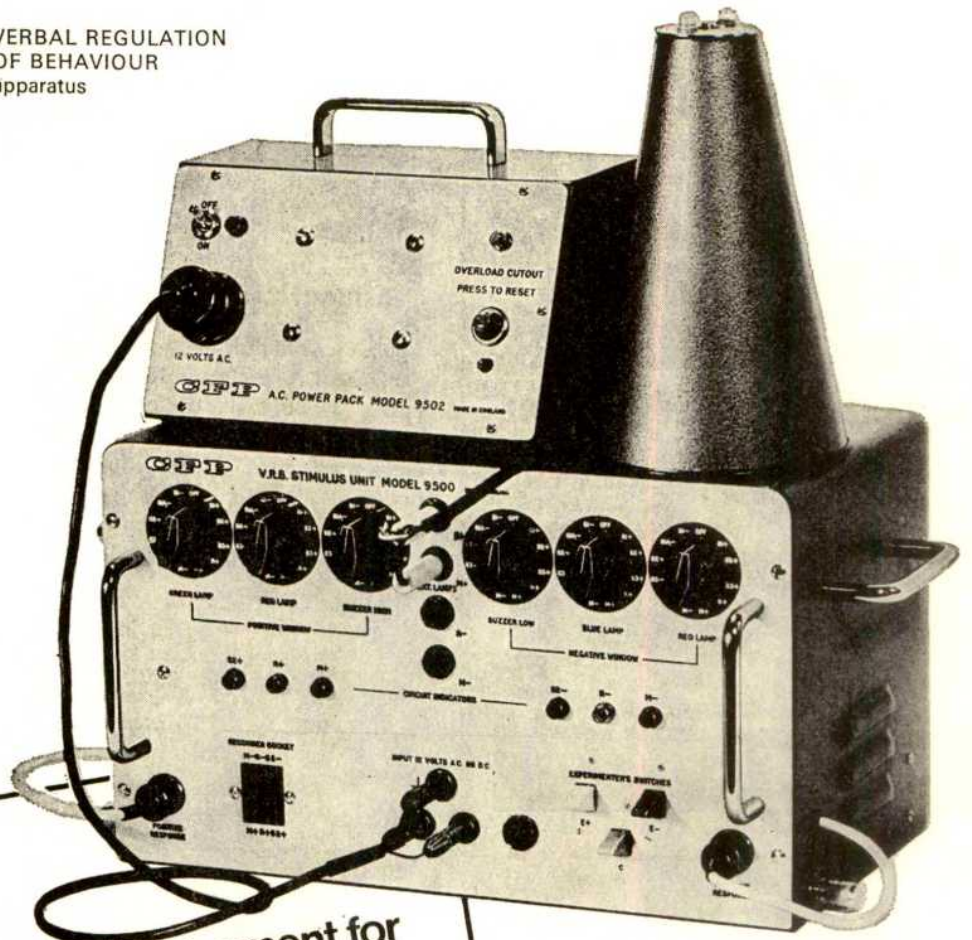

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Br. J. Psychol. (1972), 63, 2, *pp.* 149–162
Printed in Great Britain

IMAGERY: A DIMENSION OF MIND REDISCOVERED

By FRANK S. KESSEL*
Center for Advanced Study in Theoretical Psychology,
University of Alberta

This paper examines the re-emergence of imagery as a topic commanding attention in psychology. Recent research (by persons such as Haber, Paivio, Sheehan, Singer and Inhelder) is surveyed within the framework of the cognitive-experiential *Zeitgeist*. Attention is then given to methodological issues involved in devising a 'new' introspection and to a number of theoretical statements on the nature and development of imagery, notably those of Bartlett, Bruner and Piaget. Questions such as the kind and dimension of imagery, individual differences in imagery, and the relationship of imagery to creativity are raised. The general conclusion is that the study of imagery affords a good opportunity for gaining knowledge of the 'everyday stream'.

Imagery has hardly been an overworked topic in psychology over the past decades. In fact, in the 20-year period from 1940 *Psychological Abstracts* contains only five references to imagery. Of course, imagery did figure prominently in the early history of psychology as an experimental science when consciousness was its subject-matter and introspection its method of investigation. But behaviourism and 'the American revolution' (Hebb, 1960) shifted the focus of the field so that psychology purged itself of imagery, attention, states of consciousness and any other mentalistic concepts: 'As the stream of science broadens and deepens such older concepts are sucked under, never to reappear' (Watson, 1920, p. 94).

Watson's 'never' has, however, turned out to be about 40 years, with perceptual, cognitive and central processes commanding increased attention in psychology since the mid-1950s. Hebb (1960) therefore referred to the second phase of the American revolution, a phase which recognizes that 'these notions (thought, imagery, etc.) relate to a vital problem in the understanding of man' (p. 736). By all indications, and as noted by many (e.g. Antrobus, 1970; Kagan, 1967; Koch, 1960; Richardson, 1965; Zener, 1958), the *Zeitgeist* is 'cognitive-experiential' in nature. And both as a consequence and reflexion of this trend, imagery is re-emerging as a 'legitimate' area of study. Thus Holt (1964) could speak of the 'return of the ostracized': 'Cognitive psychology has enjoyed a remarkable boom. Inevitably, there has resulted some attention to imagery...The subjective world of images and the like has progressed from being at first the total subject matter of psychology, then a marshy realm of uninteresting epiphenomena, and now a legitimate constructed output of a theoretically constructed psychic black box, and perhaps a specially interesting one, since it may give something of a look inside the famous black box' (Holt, 1964, pp. 260, 263). This paper will examine the research which comprises psychology's rediscovery of imagery, as well as some of the methodological and theoretical issues raised by the rediscovery.†

* Present address: Department of Psychology, University of Cape Town, Cape Town, South Africa.

† Richardson's *Mental Imagery* (1969), published after this paper was first submitted, serves as the clearest indication of this development. Haber's (1970) review of the book is entitled, 'Imagine! They are finally talking about images again.'

RECENT RESEARCH

Eidetic imagery

In Europe (particularly Germany), away from the budding bastion of behaviourism, one form of imagery remained of interest during the 1920s and 1930s. A number of studies investigated eidetic imagery (EI) in persons who 'see' a 'visual image of a figure, usually long in duration, localized in front of the subject's eyes, positive in colour, and usually on the plane where the original figure was shown' (Leask *et al.*, 1969, p. 26). The most general finding was that EI was common among children, the percentages varying from 30 to 90, depending on age and population sampled, but almost always absent among adults (Kluever, 1932). While interest in EI began to wane in the mid-1930s it was widely accepted that EI was a 'primitive' form of imagery lost in the course of ontogenesis (Werner, 1948, pp. 143–65).

Haber and his colleagues (Haber & Haber, 1964; Leask *et al.*, 1969) have reopened the topic by undertaking a systematic study of EI. In their longitudinal investigation of the frequency of EI in two samples of children and experimental work with one of these samples, they have demonstrated both methodological and definitional weaknesses in the earlier research. Where previous research had classified subjects as 'eidetikers' if they produced any image of pictures, Haber has used strict and carefully operationalized criteria – an image has to be reported, it has to be located 'out there', the present tense has to be used when the subject refers to the image (past tense when he refers to memory of the original stimulus), appropriate eye movements must be made, the duration of the image should be over 40 sec., and the image should be reported with over 50 per cent accuracy. Given these criteria, the prevalence of EI is low – 12 out of 151 subjects in one sample and 23 out of 380 in the other.

The claim that EI is a primitive form of imagery present in most young children and that it decreases with age must now be open to serious question. On the other hand, in a study of five African tribes, Doob (1966) found some support for the view that 'primitive man' may possess greater and more persistent EI. Employing the Haber criteria, Doob concluded that the incidence of EI is 'not sensationally high but undoubtedly much higher than that normally reported for Westerners' (p. 20), the frequency being approximately 20 per cent as opposed to 8 per cent in the Haber studies. In addition, variables such as schooling, sex and rural or urban residence were not significantly related to presence of EI.

Despite the fact that EI is not as common as once thought, it remains a phenomenon of interest, especially since almost nothing is known about possible underlying processes. To the end of understanding the phenomenon and its correlates Leask *et al.* (1969) first questioned eidetikers and their parents on matters such as awareness of the ability. They conclude that 'eidetic children seem to be randomly distributed on all variables examined...except for their imagery and its properties, these children all seemed to be among those typically found in any elementary school' (p. 36). Nor did a series of 16 experiments provide any clear unified picture of the phenomenon, even though they revealed, for instance, that verbalization during screening of the original stimulus tends to interfere with EI. Leask *et al.* (pp. 44–7) must devote their final discussion to basic, still unresolved issues; for example, how

to devise a test of EI which cannot be contaminated by factors such as memory and faking.

Thus only a modest conclusion is possible after five years of research: 'We are left with the conclusion that eidetic imagery is an exciting phenomenon in its own right and one that clearly needs further explanation, but that with the data at hand we cannot yet anchor it to other cognitive or perceptual functions' (p. 47). At first blush this suggests very little progress in research on EI. However, awareness and examination of definitional problems and possible confounding variables involved in assessment procedures reflect a necessary, if less reassuring level of sophistication, a level which studies of other forms of imagery are also reaching.

Other forms of imagery

The fact that EI has 'a strong esoteric aroma to it' tends to lead to its dismissal as 'either a figment of phenomenological psychologists' imaginations or an atypical hallucination' (Leask *et al.*, 1969, p. 26). However, this is less likely to be the case with other types of imagery listed by Holt (1964), from the everyday thought or pictorial image, through the body image and hypnagogic image, to the dream image. Since pictorial imagery appears most amenable to experimental study, some form of this imagery has been the focus of recent research.

Paivio has devoted an extensive research programme to the role of imagery in standard verbal learning situations. His studies have tested the 'conceptual-peg' hypothesis that imagery aroused by the stimulus is crucial in paired-associate learning: 'Briefly stated, the hypothesis is that high-imagery, or concrete, stimulus terms such as "house" function as efficient stimulus "pegs" from which associates can be hung and retrieved by means of mediating images' (Paivio, 1970, pp. 387–8). Paivio therefore predicts that greater imagery of stimulus items will mediate better learning. A large number of studies have supported this prediction (Paivio, 1969). For instance, Paivio *et al.* (1966) systematically varied the imagery (I), concreteness and meaningfulness (*m*) of stimulus and response items and found, as hypothesized, that imagery was more facilitative on the stimulus side than on the response side, with *m* controlled. The following are examples of the word pairs used: concrete, S & R both High I – student–mosquito; concrete, S High I, R Low I – lobster–opium; abstract, S Low I, R High I – comparison–bravery; abstract, S & R both Low I – virtue–idea. Subjects' mediation reports provided further support for the imagery position – imaginal mediators were most frequently reported for concrete pairs, while learning scores were highest for pairs reportedly learned with the aid of mediating imagery. This, too, is a finding replicated in later studies (Paivio, 1969, 1970).

In discussing the implications of his research Paivio's major concern has tended to be with the verbal learning situation rather than with imagery *per se* – a factor analysis (Paivio, 1968) and imagery values for 925 nouns (Paivio *et al.*, 1968) are doubtless important for verbal learning research. More recently, however, he has opened a general discussion of the functional distinction between imagery and verbal processes as alternative memory coding systems, the relationship of this distinction to modality differences, and a further distinction between discovery and utilization of mediators (1969). By moving in this direction Paivio might well provide greater insight into the nature of imagery and its role in memory than he has thus far (cf.

Luria, 1968). He, along with a few other investigators (e.g. Bower, 1970), has at least established experimentally that 'imagery does indeed have functional significance for learning and memory – a conclusion that few people would question on the basis of common-sense observations of how we remember the world of concrete objects and events, but one which psychologists perversely rejected in their pursuit of pseudo-objectivity' (Paivio, 1970, p. 391). By establishing this fact Paivio has contributed significantly to the elimination of 'pseudo-objective' arguments against all imagery research.

The feasibility of studies concerned with the nature of imagery *per se* is demonstrated by Sheehan's work. In his first study Sheehan (1966*a*) turned to a problem originally investigated by Galton, who showed that subjects can look at an object, conjure up an image of it, and then compare the two. In Sheehan's situation subjects adjusted a variable display to match their image with respect to luminance, size and clarity of outline, thereby enabling Sheehan to determine the accuracy with which the image matches the original objects. After constructing the image, subjects rated the image on a seven-point scale of vividness. The data revealed a large positive correlation between reported vividness and accuracy of image matching to the original – vivid images reconstituted the stimulus features of the object more than poor images did. Sheehan therefore concluded that 'vivid imagery tends to reconstitute the accuracy of the perceiving process and that individual differences in reported vividness of imagery can be related to publicly observable differences in stimulus matching' (p. 397).

Sheehan (1967) has also revised and factor analysed a Questionnaire upon Mental Imagery (QMI) originally constructed by Betts in 1909. The questionnaire now contains five items for each of the seven modalities, the subject being required to rate vividness of his imagery for each item on a seven-point scale. (For example, Visual – 'think of seeing the sun sinking below the horizon'; Auditory – 'think of hearing the sound of escaping steam.') Sheehan (1966*b*) demonstrated the usefulness of this measure in a study of the behavioural correlates of individual differences in imaging.* Having obtained measures on the Betts QMI, Sheehan had subjects construct a pattern of blocks to match an original pattern which was either perceived, imaged or recalled. In terms of variables such as errors of location and inversions of order, he found that subjects in the recall condition behaved differently from those in the imaging condition. On the other hand, since vivid imagers reproduced the stimulus as they perceived it, Sheehan argues against the view that vivid imagers are usually 'confabulators and inventors'.

Whatever the interpretation of Sheehan's findings, and he argues for a 'functional similarity between imaging and perceiving', his studies demonstrate that systematic experimentation with imagery is possible. This conclusion is strengthened by the imaginative research of Singer, Antrobus and their co-workers, carried out with William James's (1890) views on the 'stream of thought' as a starting-point and within a long-standing study of daydreaming (Singer, 1966). In an illustrative experiment Antrobus *et al.* (1966) tested predictions derived from an information-processing model of mental processes involved in the generation of fantasy, imagery and other

* From the point of view of individual differences, Joseph Royce (personal communication) suggests that psychological testing literature may be the repository of relevant findings and assessment techniques.

'stimulus-independent mentation'. One such prediction was that an increase in the information load from external sources (effected by a faster presentation rate in a signal detection task) would lead to a decrement in the production of spontaneous cognitive events. The dependent measure was subjects' ratings on a number of five-point scales, such as amount of task-irrelevant thinking, verbal thinking, visual imagery and auditory imagery. As predicted, all the measures except auditory imagery decreased in a fast stimulus rate condition; in addition, task-irrelevant thinking and visual imagery decreased when the task increased the load on short-term memory.

The heuristic value of this sort of experimental situation and of the overall model is attested to by a number of further studies. After summarizing these studies Singer (1969) reaches the following conclusion: 'I have tried to describe very briefly specific kinds of experiments that examine parameters of the stream of thought, the conditions which enable it to emerge in consciousness, and its functional role for the organism. We have found that by careful use of a variety of self-report techniques in formal experimental situations, or by careful use of judging and matching techniques, it is possible to get reliable and apparently lawful data on the ongoing spontaneous thought processes. James's business is far from finished, but at least we can approach these issues in the laboratory today. Man does produce within himself a complex, ongoing flow of images and inner conversations, and these are indeed capable of study experimentally as well as in the consulting room' (pp. 71–2).

Imagery in children

While the work of Sheehan and Singer points to the clear possibility of systematic study of imagery it is worth noting that their subjects were all adults. Research with children does, of course, involve procedural difficulties since children cannot be expected to give reliable reports about vividness of imagery or to match a variable display with their images. However, indirect assessment has been imaginatively employed by a number of investigators.

A recent series of characteristically ingenious studies at Geneva were of this indirect sort. Inhelder (1965) reports experiments assessing 'anticipatory imagery', i.e. where the child anticipates the results of the displacement of one figure in relation to another or the successive transformations of one figure. For example, children from 4 to 9 were shown one square piece of cardboard directly on top of another and then had to anticipate the result of displacing the upper figure to one side, first, by drawing the result of the displacement, second, by choosing from a set of drawings the one best representing the displacement, and third, by drawing the result after seeing the actual displacement. The general result was that the younger children had difficulties in anticipating but not in copying, while choosing an alternative and making an anticipatory drawing were equally difficult. Other experiments produced the same result, for example, when the children had to anticipate what would happen if a piece of wire in the shape of an arc was straightened by drawing the end result and successive intermediate stages.

In the Inhelder (1965) report data are scarce and the link between the results and the interpretation in terms of 'static' and 'dynamic' imagery somewhat tenuous. However, Robertson & Youniss (1969) and Youniss & Robertson (1970) have undertaken replication studies involving deaf and hearing children and report evidence in

favour of Piaget's view regarding the dependence of imagery on the growth of operative intelligence: 'Deaf, linguistically deficient children and hearing children of CA 11–12 more accurately projected images of a given event over a wide range of physical alternations than deaf and hearing Ss of CA 8–9. The improvement in performance between the two age groups correspond to the span of the concrete operational stage' (Youniss & Robertson, 1970, p. 216).* Carey (Bruner *et al.*, 1966, pp. 202–205) has also replicated the finding reported by Inhelder (1965) that accurate anticipation of the level of liquid poured from one beaker to another precedes conservation of liquid quantity. Thus the Geneva research appears to hold promise for the investigation of imagery in children.

Research in more conventional settings has also been carried out. Kuhlman (Bruner *et al.*, 1966, pp. 26–8), for instance, hypothesized that good imagery facilitates performance on some tasks – for example, learning concrete nouns – but hinders performance on others – for example, learning an abstract concept. Using a number of psychometric subtests (Thurstone Space Test from the PMA, the Minnesota Paper Form Board) to assess imagery in a group of first- to fourth-grade children, she found that high-imagery children were indeed better on a task where arbitrary verbal labels were associated with pictures. On the other hand, the low-imagery children excelled in a task which involved recognizing the abstract attribute shared by a set of pictures. A similar finding with adults (Stewart, 1966) adds interest to this kind of experiment.

Reese (1965, 1970*a*) has employed a paradigm certainly more conventional than Piaget & Inhelder's tasks, namely paired-associate learning. In the imagery condition of his studies Reese's subjects were shown pictures containing the 'compound' of associates to be paired (for example, a cat carrying an umbrella), while other subjects were given verbal descriptions of that compound. The evidence shows that imagery conditions were as facilitating as the sentence descriptions in older children but were less effective for younger preschoolers. However, the fact that Reese (1970*a*) offers six alternative explanations of the age trend and that his own recent study (1970*b*) fails to reveal such a trend indicates how much more research is required in this particular task situation. Whether research effort should be invested in such a paradigm is itself an open question: 'The effects of imagery on the learning of pairs of unrelated items seems less significant for human learning capabilities than the effects which imagery may have on paraphrasing a paragraph which a child has heard or seen, or on using the principles of the geometry of a square to facilitate the solution of the problem associated with the geometry of right angle triangles' (Palermo, 1970, p. 419).

The research worker interested in imagery *per se*, whether in children or adults, must obviously face this general question of which experimental situations are likely to be most fruitful. The studies considered above suggest that he need not, perhaps should not (Deese, 1969), confine himself to paradigms commonly associated with experimental psychology.

* As part of his growing research programme on imagery, Bugelski (1970) has also found that deaf children do not appear to be deficient in their use of imagery in certain learning and memory tasks.

Issues – old and new

With some of the recent studies of imagery discussed we can turn to issues of a more general and far-reaching nature, some concerning methodology and others the place of imagery in theoretical accounts of mental processes.

Methodology and related matters

One possible reaction to the rediscovery of imagery would be the claim that, having seen the apparently justified demise and disappearance of introspectionism, we are now attempting to flog a dead horse. However, the fact is that introspection has not disappeared – as Boring (1961) has indicated, what behaviourism eliminated was merely introspection as a special analytic technique in the Titchenerian sensationistic sense. Introspection as a general method for studying mental content is widely used in several areas, one of which is considered 'objective' and quantitative psychology at its best: 'The lesson to be learned from psychophysics is therefore that, in respect of the observation of sensory experience, introspection has thrived for a hundred years and is still in style' (Boring 1961, p. 232). As Richardson (1965) has indicated, much research in perception, personality and social psychology involves an individual reporting his subjective experience. Thus introspection is far from being an outdated method and Taylor (1962), in formulating his S-R theory of perception, provides a cogent and concise statement of this point: 'Can we accept without question a subject's report on what he has perceived? The answer is that we have no alternative. The facts as he reports *are* the facts that stand in need of explanation, and if we refuse to accept them we might as well retire at once from the field of science' (p. 2).

It is also worth noting that the criterion of inter-observer agreement and the distinction between 'public' and 'private' events, often grounds for the claim that imagery is 'subjective' and not amenable to 'scientific' study, have been seriously questioned by those seeking to restore experience as part of psychology's proper subject-matter (Meissner, 1966; Zener, 1958; Zener & Gaffron, 1962).* For them 'objectivity' in science means that under given specifiable and manipulable conditions, repeatable events can be shown to recur – different individuals independently and repeatedly report similar, specifiable experiences under relevant, specifiable conditions. As Zener & Gaffron (1962) argue, 'if experience is accepted as a reality, and if the universe is accepted as orderly, then to the extent that relevant conditions ... are similar, the experiences of two comparable individuals under similar conditions should be similar' (p. 557).

The best support for this general position comes, ironically enough, from a founding logical positivist and from the father of operationism: 'A person's awareness of his own state of imagining, feeling, etc., must be recognized as a kind of observation in principle not different from external observation, and, therefore, as a legitimate source of knowledge' (Carnap, 1956). 'Introspectional report is, by the very fact of being a report, public, and, as such, proper subject for psychological inquiry...It is not meet that an outsider like me should question the judgement of the psychologist that he can at

* Burt (1962) provides probably the clearest statement of the argument that the concept of consciousness must be reintroduced in psychology and that introspection is a valid and viable means of gathering evidence on the contents of consciousness.

present spend his time more profitably on more public matters than introspectional report, but *as a physicist I may be permitted to express a mild surprise that this should apparently be made so much a matter of principle by the behaviourist, or that any method should be discarded which might conceivably help in unravelling the incredibly complicated maze of present psychological phenomena'* (Bridgman, 1959, p. 241; italics added).

To contend that experience is acceptable subject-matter for psychology and introspection an acceptable general method is, of course, not to suggest that the study of imagery and other contents of consciousness is free of problems. The past holds useful lessons – choice of the wrong content categories by Titchener* and categorization of introspective data by subjects themselves (McClelland, 1955); the rejection of meaning and the attempt to isolate sensory elements (MacLeod, 1964); the ignoring of experience's 'inherent intentionality' (Meissner, 1966). However, consideration of the simple case of several persons giving diverse experiential reports is sufficient to reveal problems the researcher must still face – he has to distinguish between genuine differences in experience and those arising from other factors, for example, 'ambiguity in phenomenal terminology', 'subsitutions of cognitive judgements for phenomenal report', 'associative or inferential comments', and 'sheer insensitivity or even carelessness in report' (Zener & Gaffron, 1962, p. 558).

Thus, in devising a 'new' methodology of introspection or self-observation, we will have to confront questions such as the reliability of phenomenal language, the nature of the relation between experience and report (Natsoulas, 1967), the formulation of dependent and independent variable categories for experiential data, and the necessary qualifications and abilities of subjects. But there is no intrinsic reason why such problems cannot be dealt with. In fact, the greatest contribution of recent research on imagery lies in its demonstration that systematic study is possible and that these difficulties can be overcome. Therefore, the appropriate concluding comment on the matter of methodology is that the study of imagery and of other experiential events does not differ, in basic principle, from the study of other psychological phenomena: 'There is no reason to think that experiential evidence cannot be treated methodologically in the same manner as any other evidence. Consequently, experimental designs, statistical inference, control group studies, rigid specification of stimulus conditions, etc., ought to be utilized in the interest of scientific objectivity' (Meissner, 1966, p. 526).

Theoretical positions

The search for statements concerning the nature and function of imagery reveals, not surprisingly, that the Englishman to whom the new cognitive psychology is clearly indebted (Neisser, 1967) had a carefully formulated view some 40 years ago. Bartlett (1927) considered imagery within the context of thinking processes: 'Imaging and thinking possess alike the general function of enabling us to deal with situations at a distance' (p. 25). However, the thinking processes were primary, since visual images constituted for Bartlett one sort of 'sign' employed in thought. For instance, although the image may vary in its 'stage of completeness', from a fleeting fragment

* 'It is astonishing what havoc is wrought by admitting at the outset apparently innocent suppositions that nevertheless contain a flaw. The bad consequences develop later on, and are irremediable, being woven through the whole texture of the work. The notion that sensations, being the simplest things to take up in psychology, should be taken up first is one of these suppositions' (James, 1890, p. 224).

to the most literal reinstatement, it is put to good use in the process of recall: 'In general, the image is a device for picking bits of schemes, for increasing the chance of variability in the reconstruction of past stimuli and situations...By the aid of the image, and particularly the visual image – this is the best of its own type – a man can take out of its setting something that happened a year ago, reinstate it with much if not all of its individuality unimpaired, condense and combine it with something that happened later' (p. 27).

Bartlett also stressed the flexibility of imagery and suggested that 'invention, the bringing in of totally new details, is particularly prone to occur in the course of the use of sensory imagery' (1921, p. 327). While this raises the possibility that 'bright discoveries' will result from imagery drifting into 'by-path meadows' (1927, p. 29), such flexibility also constitutes a defect. Since imagery and affect are thought to be closely connected (1925), the former is likely to be more 'individualized' than is useful. The constructions of imagery are therefore 'relatively wild, jerky, and irregular compared with the somewhat orderly march of thought' (1927, p. 27).

Finally, Bartlett considered thinking 'biologically subsequent' to the operation of imagery; thinking can only occur when the 'sequential tyranny of past events' has been broken by images. The ontogenesis of imagery precedes and then continues along with that of thinking: 'An interesting chapter in genetic psychology yet remains to be written on how words actually come to supplement, and in some cases, to replace direct sensory imagery, and how, at the same time, the sensory image is not only normally retained as an element in mental life, but may develop along lines of its own' (1921, pp. 336–7).

Bartlett's views are of interest because they are markedly similar to those of contemporary writers. The position most widely encountered is that imagery is one form of representation, its flexibility and potential inventiveness being 'superior' characteristics, its idiosyncratic nature and close tie to affect 'inferior'. So Neisser (1967, 1970) stresses the active and constructive nature of the imagery process and hence its basic continuity with other cognitive processes (1967, p. 146) but also suggests that, for recall at least, imagery is a kind of cognitive 'luxury'. Images are 'appropriate concrete representations of thoughts which would otherwise be too abstract to pictorialize' (pp. 157–8). A slightly different view, but one still in the Bartlett tradition, is that of Horowitz (1967), who regards thought as a process activating or suppressing 'multiple cognitive systems' each with its own characteristics and utility; imagery is one such system. In addition, imagery is seen as a developmentally more 'primitive' system that is later by and large discarded in favour of verbal representation or verbal thinking.

Bruner *et al.* (1966) provide another expression of this developmental view. For the Harvard school 'ikonic representation' is the second of three stages in the child's representation of the external world, the stage 'when a child is finally able to represent the world to himself by an image or spatial schema that is relatively independent of action' (p. 21). This is an advance from the first stage of 'enactive representation', with early cognitive development being marked by the gradual freeing of perception and imagery from the control of action. On the other hand, Bruner *et al.* see the child's imagery as inflexible, dependent upon small detail, 'autistic' and susceptible to the influence of affect. And, as demonstrated in Kuhlman's

study discussed above, these properties can handicap the child in certain task situations. They are therefore the limits of ikonic representation beyond which 'symbolic representation' is said to take the child (p. 53). However, Bruner *et al.* barely discuss the subsequent fate of imagery, merely mentioning two possibilities – either it is suppressed or it is retained and adapted to the symbolic mode of representation (p. 26, pp. 48–55).

The Geneva position on the development of imagery is somewhat more explicit on the relationship between imagery and other cognitive processes (Furth, 1969, chapters 5 and 7; Inhelder, 1965). In Piaget's theory images form one part of the 'symbolic function' which dominates the first part of the pre-operational period (2–4 years). However, as is the case for perception, memory, and language. Piaget conceives of a more or less one-way relationship between imagery and 'operative intelligence' – mental imagery is generally subordinate to or controlled by evolving operations of intelligence. So Inhelder (1965) contends that the 'static quality of pre-operative thought' dominates the 'elementary forms of symbolic imagery', while the attainment of 'operative thought' leads to the mobility and anticipatory quality of the 'higher forms of symbolic imagery'.

Whether the Geneva studies mentioned previously can be construed as much more than weak and indirect proof of this theoretical position is, of course, debatable. This is also true of the Bruner view. Nevertheless, the Geneva studies do demonstrate that imagery undergoes change in the course of development. Much work is required before we can decide whether the change takes the form postulated by Piaget, Bruner or, indeed, Bartlett.

Imagery and the 'everyday stream'

Attempting to obtain basic insights into the occurrence of imagery in the everyday stream of consciousness might prove heuristically valuable at this stage in the study of imagery. Holt (1964) has taken a taxonomic tack and his classification of different types of imagery leads directly to the question of dimensions along which these types vary (Horowitz, 1967):

(1) *Degree of dependence on external stimuli* – the eidetic image and some pictorial images are brought into effect by an external stimulus, while hypnagogic images are initiated by processes within the individual.

(2) *Flexibility and degree of conscious control* – the majority of everyday pictorial images seem to be willingly evoked by the individual; on the other hand, anyone who has trouble falling asleep knows that hypnagogic images are remarkably resistant to conscious control.

(3) *Amount, vividness, and duration.*

(4) *Degree to which image is tied to affect* – this serves mainly to differentiate fantasy imagery from the other forms.

(5) *Sensory modality* – though visual imagery is most often discussed, imagery in other modalities obviously exists – auditory imagery must have played a crucial role in Beethoven's* later compositions when his deafness was complete. Weber & Bach

* 'I carry my thoughts about me for a long time, often a very long time, before I write them down; meanwhile my memory is so faithful that I am sure never to forget, not even in years, a theme that has once occurred to me. I change many things, discard, and try again until I am satisfied. Then, however, there begins in my head the development in every direction. . .I see and hear the picture in all its extent

(1969) report some preliminary findings that 'imagery may follow different laws in different modalities' (p. 199).

A number of issues arise directly from these suggestions regarding imagery dimensions. What of the *relative efficacy* of the different modalities? Is visual imagery more vivid and long-lasting than auditory? A closely related question is whether *inter-modal transfer* can occur. It is now common practice to teach blind persons the layout of their local environment by using raised tactual maps (Gilson *et al.*, 1965). Introspective data might answer the question of whether the blind trained in this way can be said to go beyond tactual representation of their environment. Age of onset of blindness would obviously be an important factor.

What about *individual differences* in imagery? Bartlett (1921) himself bemoaned the fact that his images of a university cricket match were 'fragmentary, condensed ...and excessively disorderly' (p. 323). The question can be approached from the perspective of dimensions sketched above – individuals with a large amount of imagery in different modalities may or may not have much control over it, nor need their imagery be vivid (see footnote on p. 152). The problem assumes importance in light of the often made claim that scientific creativity involves much use of imagery. Roe's (1951) interview with eminent scientists provided the best-known support for this contention, while Walkup (1965) also provided evidence that 'creative persons appear to have stumbled onto and then developed to a high degree of perfection the ability to visualize in the area in which they are creative' (p. 36). Nevertheless, more systematic research, particularly with children, is required to answer the imagery-creativity question more fully?*

A related issue is whether imagery abilities can be 'trained', and, as a corollary, whether such abilities, where present, are somehow stifled by an education and a culture which stresses analytic, 'rational' thinking and verbal fluency: 'In a factually oriented, skeptical, anti-intraceptive, brasstacks culture like ours, where the paranormal is scoffed at and myth and religion are in decline, the capacity for vivid imagery has little survival value and less social acceptability. We live in an age of literalism, an era that disrupts the imagination' (Holt, 1964, p. 262).† Whether there is any

and dimensions stand before my mind like a cast, and there remains for me nothing but the labor of writing it down which is quickly accomplished'. (In H. W. Krehbiel (ed.), *Beethoven: The Man and the Artist, As Revealed in His Own Words*. This passage written in 1822.) 'I learned, for instance, *Norma* and *La Gioconda* before I could ever have sung them properly. So I could rehearse them in my mind, on the top of a bus, or walking in the street. There is a great deal to be done in the mind: you do not always require a piano, nor to open your mouth. Yes, that is very important; the preparation in the mind. The poets talk of the mind's eye: there is the mind's ear' (Maria Callas, *Observer Review*, 15 Feb. 1970).

* Shah (1969) has taken an interesting first step in investigating the relationship between visual imagery, verbal thinking and creativity in visual expression amongst children from 4 to 12. Amongst other things he found that the 'ability to operate with several channels of thought such as imagery as well as symbolic or verbal thinking is a significant factor in creativity' (p. 15).

† Holt's comments are little less accurate now than they were seven years ago. Society has since seen something of a reaction against a 'factually oriented...anti-intraceptive, brasstacks culture', the growth of the 'counter-culture' (Roszak, 1969). One indication of such a development is the interest in 'altered states of consciousness' within professional circles in psychology (Tart, 1969) as well as elsewhere. This is not to suggest, however, that imagery is totally freed of its 'paranormal' or 'inferior' connotations. Huttenlocher (1968) has demonstrated that individuals will invoke spatial images to represent the order of items in three-term series problems ('Tom is taller than Sam, John shorter than Sam, who is the tallest of the three men?'). This imagery strategy appears most effective. But note Huttenlocher's concluding remark: 'ironically, however, Ss are ashamed of this strategy; with what seems more than ordinary human perversity, they regard their imagery as an intellectual crutch, unseemly for the solution of formal reasoning problems' (p. 560).

reason why abstract thought and 'richness of fancy' should not both be encouraged is, at least, questionable. After noting that 'our bookish and wordy education tends to repress this valuable gift nature', Galton suggested a century ago that 'the highest minds are probably those in which it [visual imagery] is not lost, but subordinated, and is ready for use on suitable occasions' (in Roe, 1951, p. 461). (See footnote * on p. 159; cf. Maslow, 1966, ch. 6.)

Finally, within the context of interacting cognitive systems it is worth noting the image-evoking quality of verbal material. While Paivio's research demonstrates this quality experimentally, the reading of any worth-while literary or poetic work reveals, not only that words evoke images, but also how the imagery seems something over and above the mere sum of word meanings. It is not unreasonable to propose that the evocation of total visual scenes is one psychological event at the root of literary appreciation.

Concluding comments

That a discussion of imagery should end on the topics of creativity and literary appreciation is perfectly appropriate. Imagery stands very much at the psychological crossroads for, as demonstrated by Arnheim's (1969) outstanding text on *Visual Thinking*, the topic is equally appropriate in discussions of perception, of cognition in general, and of the psychological aspects of aesthetics.* Most significantly, it affords psychology an opportunity of opening, again, knowledge of the experiential realm and, as such, is part of a healthy *Zeitgeist* (Beloff, 1962) to which others, notably physiologists, are contributing (Eccles, 1966; Smythies, 1965; Sperry, 1969): 'The reintroduction of introspection, the new concern with the phenomenal field, the clinical attention to subjective experience, the studies in social perception, etc., seem to me to indicate not indeed a regression to an obsolete psychology, but rather an advance along the spiral...of the evolution of the scientific outlook' (Feigl, 1959, p. 123). Not only is there a 'Ghost in the Machine' (Koestler, 1967) but the study of imagery, as well as other experiential events, may well reveal that the Ghost is both friendly and fascinating.†

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* The Humanistic psychologists, as part of their emphasis on the world of experience, seek to consider psychological aspects of art in general (for example, Giorgi, 1970; Wyatt, 1968).

† 'One way of pointing up the issue is to ask whether the physicalist account can really be "complete". I had the privilege of discussing the problem...with Albert Einstein one afternoon in April 1954 at his home in Princeton. I asked Einstein whether in an ideally perfect (of course utopian) four-dimensional, physical representation...of the universe the qualities of immediate experience...were not left out. He replied in his characteristic, humorous manner (I translate from the German in which he used a rather uncouth word): "Why, if it weren't for this internal illumination [i.e. sentience] the world would be nothing but a pile of dirt"' (Feigl, 1967, p. 138).

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(*Manuscript received* 1 *April* 1970; *revised manuscript received* 19 *April* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 163–164
Printed in Great Britain

CANADIAN AND BRITISH RATINGS OF THE IMAGERY VALUES OF WORDS

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In recent years considerable research, much of it under the direction of Allan Paivio (see Paivio, 1969, 1970 for reviews) has shown that the image-evoking value (I) of a word is highly correlated with its ease of learning. Kintsch (1970) states: 'it seems safe to predict that imagery (I) will stay with us as one of the most important task variables in verbal learning' (p. 18). The I values of words are the mean ratings of a group of subjects on a seven-point Low Imagery–High Imagery dimension. Paivio *et al.* (1968) published I values for 925 nouns based on ratings by 30 Canadian students and these have become the standard source of material for experiments in which I is manipulated or controlled.

Because imagery has been shown to be an important variable, estimates of I values for British subjects are desirable both for research into the effects of I and for the control of I in verbal learning experiments. The following experiment attempts to assess the reliability of the Paivio ratings for British subjects.

A sample of 100 words was taken from the Paivio list. Of these, 93 were obtained by selecting every tenth word (the words in the Paivio list are ordered alphabetically). Four of these words were judged to be in much more common usage in North America than Britain. Therefore in case the correlation between the British and the Canadian rating proved low and it was shown to be necessary to restrict the use of the Paivio list to common British words only, possible replacements for each of these four words were included by also selecting the succeeding words. Finally, to make up the 100, one word at random was selected from each third of the list.

The words were typed in a random order on five pages, 20 to a page, with a seven-point Low Imagery–High Imagery rating scale alongside each. The pages, after being shuffled to randomize their order, were stapled into booklets with an introductory page of instructions exactly as used by Paivio, and a second page with four examples which, following Paivio's procedure, were to be rated before the main set of words.

The ratings were completed by 26 first-year psychology students during a practical class. I values were determined for each word and product moment correlations calculated with the I values given by Paivio for (i) the 96 words including the four with a more common American usage, (ii) the 96 words with the American words replaced by their succeeding words, and (iii) all 100 words. In each case the correlation was $+0{\cdot}93$. However, a related t test showed the Canadian ratings to be significantly higher than the British even when the words with a more common North American usage were excluded from the analysis. (Mean ratings: British, 4·62; Canadian, 4·98; $t = 5{\cdot}495$; d.f. $= 95$; $P < 0{\cdot}001$.) The standard deviations of the two samples were very similar (British, 1·52; Canadian, 1·47). Taken with the very high intercorrelation it would appear that British subjects rate words for I in a similar way

to Canadians but consistently give a lower value. The most parsimonious explanation is that the words evoke imagery to a similar degree in both British and Canadian subjects but that the British subjects are more conservative in their use of the rating scale.

The high correlation between the I values given by Paivio and those obtained in this experiment indicates that Paivio's list is satisfactory for use in experiments with British subjects. The lower ratings by British subjects suggest that direct comparison of I values obtained from British subjects with the values in Paivio's list is not advisable.

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(*Manuscript received* 21 *May* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 165–171
Printed in Great Britain

AROUSAL INCREMENT AND DEGREE OF COMPLEXITY AS INCENTIVE

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In Expt. I subjects were required by means of a key-press to view checkerboard patterns of either medium (36 bits) or low (four bits), medium or high (900 bits), or low or high complexity. Normally aroused subjects key-pressed more often to view slides of intermediate rather than low or high complexity. In Expt. II EEG desynchronization was found to be an inverted U-shaped function of complexity. In Expt. III subjective complexity (SC), based on the number of names of objects that the patterns reminded the subjects of and an estimate of the strength of each association, was found to be a U-shaped function of pattern complexity. These results were interpreted using Berlyne's notions regarding arousal, conflict and preference for complexity.

Sales (1968) with rats and checkerboard patterns, Munsinger & Kessen (1964) with humans and asymmetrical polygons, and Dorfman (1965) with humans and checkerboard-like patterns varying in absolute and relative uncertainty, have all found preference for complexity to be an inverted U-shaped function. Since Berlyne & McDonnell (1965) found that more complex patterns evoked on the average longer EEG desynchronization than less complex patterns, it seems likely that a subject's preference is related to some amount of arousal increment gained from viewing stimuli of an intermediate degree of complexity. Furthermore, since Berlyne (1969) has found bar-presses for light increment to be a function of the arousal level of rats, this suggests that preference for complexity may also be influenced by a subject's arousal level.

The aim of the first experiment was to investigate the effect of arousal level on preference for complexity using stimuli similar to Dorfman's (1965) in a situation involving a choice between two keys. One key when pressed resulted in a subject being presented a stimulus of one degree of complexity; the other key, a different degree. Arousal level was varied by means of white noise (WN); Berlyne *et al.* (1967) found that WN produced an 'especially intense EEG reaction'.

Experiment I

Method

Subjects

The subjects were 120 female students taking the introductory course in psychology at the University of New Brunswick.

Apparatus

The materials consisted of three slide projectors (Kodak Carousel, Model 550, 5 in., f/3·5 lens); a lenticular projection screen; one pair of earphones (Sharpe, model HA-10-A); a WN generator, Grason–Stadler, model 901B; programming equipment; a polygraph (Grass Instruments Co.); two biopotential skin electrodes (Beckman Instruments Co.), one attached to the palm of the subject's left hand and the other to the inside of her left forearm; 48 different slides, 16 of low

complexity (2 × 2 matrix grain), 16 of medium complexity (6 × 6 matrix grain), and 16 of high complexity (30 × 30 matrix grain). Since the probability of whether a particular cell was black or white was set at 0·50, in accord with the Shannon–Weaver average information measure, the class of low-complexity slides had four bits of information; medium, 36 bits; high, 900 bits.

Procedure

Each subject wore earphones, was seated in front of a table on which were mounted two telegraph keys, and faced a screen. Behind her were the three slide projectors, the programming equipment and the experimenter. To the subject's left was the polygraph with which her GSR was recorded throughout the experimental session.

There were six groups of 20 subjects: WN, medium (m)–low (l); medium (m)–high (h) and low (l)–high (h); no WN, m–l, m–h, l–h. In the no WN condition, essentially the subject was instructed to choose one of the keys and press it once when a light flashed beneath the screen. She did not know beforehand which key was associated with a particular degree of complexity; all trials were scored. In addition, in the WN condition, she was told that she might receive a slightly painful shock via the GSR electrodes (no shock was ever administered). For example, on a given trial, a WN, m–l subject, after receiving the instructions, heard 4 sec. of WN (85 db), the light flashed, and a choice of either the left or right key was made. Depending on her choice of keys a slide of either m or l complexity appeared on the screen for 7 sec.; 7 sec. after this slide went off the screen, the WN was presented again. Throughout the 32 trials of the session, one key (e.g. left) was associated with one degree of complexity. For trials 16–32 the same slides were used as in the first 16 trials but they were arranged in a different order. The assignment of subjects to groups, association of a particular degree of complexity with either the left or the right key, and order of slides was randomly determined. A no WN subject heard nothing during the corresponding 4 sec. period of WN for a WN subject.

Result and discussion

In the no WN condition, subjects preferred medium to low complexity ($t = 2{\cdot}54$; d.f. = 19; $P < 0{\cdot}02$) and medium to high complexity ($t = 2{\cdot}69$; d.f. = 19; $P < 0{\cdot}02$). No preference was evidenced between low and high complexity. The means are plotted in Fig. 1. These findings strongly support the claim that an intermediate degree of complexity has maximal reinforcement value for normally aroused subjects. They are especially important since although apparently the greatest verbal preference in Dorfman's (1965) study was shown for a pattern with 36 bits of information, only a significant Friedman chi-square testified to the inverted U character of the function relating preference to degree of complexity. Likewise, in Study V of Munsinger & Kessen's (1964) monograph involving subjects who key-pressed for four levels of random shapes, the statistic reported regarding the nature of the relation between these variables was only a significant overall F.

Three arousal × blocks (2 × 2) analyses of variance were performed on the choice data. No main effects or interactions were significant. Three analyses of variance were performed on the GSR amplitude data for the 4 sec. period in which the WN was presented for the subjects in the WN condition and the corresponding 4 sec. period for the subjects in the no WN condition. The method used in analysing the GSR data was that of Wolfensberger & O'Connor (1967). The effect of blocks was highly significant ($P < 0{\cdot}001$) in all three groups (m–l, m–h, l–h), but in only the m–h groups was arousal significant ($F = 4{\cdot}85$; d.f. = 1, 38; $P < 0{\cdot}05$). Similarly, three analyses were performed on the GSR amplitude data for the 7 sec. period when a complexity slide was on the screen. The effect of blocks was highly significant in all groups ($P < 0{\cdot}001$).

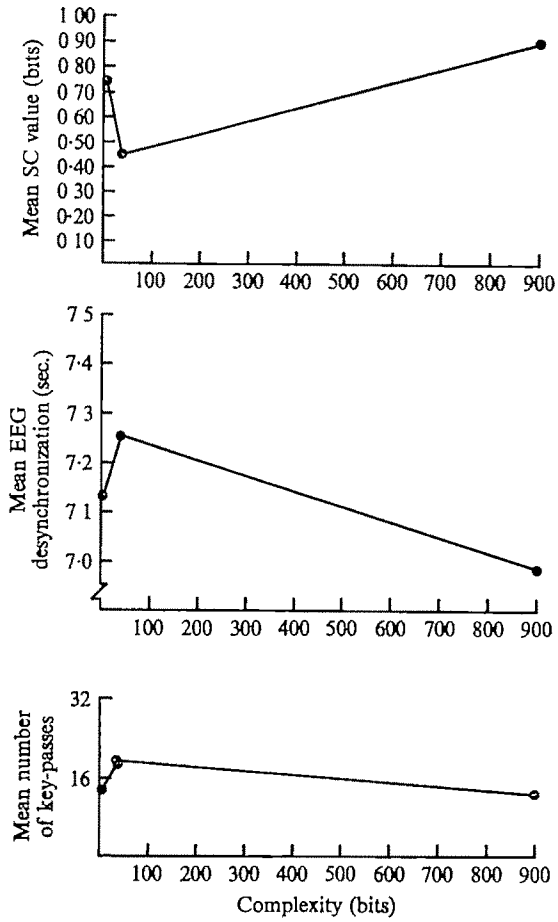

Fig. 1. The relation between subjective complexity (SC), EEG desynchronization, subject's preference, and pattern complexity.

The failure in general of finding significant effects of arousal level on preference for complexity may have been the result of the use of both a relatively insensitive design (see Lacey & Van Lehn, 1952) and arousal measure (see Berlyne & Borsa, 1968). In order to investigate the relationship between complexity and arousal more thoroughly, Expt. II was undertaken.

Experiment II

Berlyne & McDonnell (1965) found that more complex patterns evoked on the average more EEG desynchronization than less complex ones. However, degree of complexity was not systematically varied in this study. Furthermore, there was no quantitative measure of the objective information content of the complexity slides used.

Expt. II simply involved a subject being exposed to the series of complexity slides used in Expt. I, while his electroencephalograph was being recorded. It was predicted that EEG desynchronization would be an inverted U-shaped function of complexity.

The rationale for this prediction was based on Berlyne's hypothesis (1965, p. 246) that the common motivational effects of such variables as novelty, surprisingness, blurredness, complexity, etc., may be attributed to response conflict. Nicki (1970) found subjective uncertainty (SU), i.e. response conflict regarding the identity of a series of blurred objects to be an inverted U-shaped function of blurredness. Since Berlyne (1961) has shown one component of the orientation reaction, GSR, to be intensified by response conflict, one would expect, in accord with Berlyne's hypothesis (1965, p. 246), EEG desynchronization (or conflict) to be an inverted U-shaped function of complexity.

Method

Subjects

The subjects were 60 male and female students from the University of New Brunswick, St Thomas University and Teacher's College.

Apparatus

The materials consisted of a slide projector (Kodak Carousel, Model 550, 5 in., f/3·5 lens); a lenticular projection screen; 10 l complex, 10 m complex and 10 h complex slides of Expt. I; programming equipment; a polygraph (Grass Instruments Co.); three skin electrodes (Grass Instruments Co.) attached to the subject's left and right occipital lobes, and the nape of the neck, using electrolyte gel and disposable adhesive collars. One half of the subjects received one random order of slides; the other half, the first order reversed. Each complexity slide appeared on the screen for 7 sec. and was followed by a blank slide which remained on the screen for a duration which varied between approximately 1 and 30 sec. Recording techniques, criteria for discarding subjects, and methods for scoring responses were very similar to those used by Berlyne & McDonnell (1965), and Berlyne *et al.* (1967).

Procedure

The subject was seated in a recliner chair facing the screen about 10 ft. away. The experimenter stood behind him near the polygraph. The subject was simply told to look at the screen and relax as much as possible.

Results and discussion

The amount of EEG desynchronization associated with each slide of a given complexity is plotted in Fig. 1. Inspection of Fig. 1 reveals EEG desynchronization to be an inverted U-shaped function of complexity. Ferguson's (1965) non-parametric trend analysis indicated only a significant bitonic component ($Z = 2{\cdot}77$; $P < 0{\cdot}01$). The function relating EEG desynchronization to complexity may be described as consisting of two branches, one monotonically increasing and the other monotonically decreasing (see Ferguson, 1965, p. 8). Thus the prediction that EEG desynchronization is an inverted U-shaped function of complexity was fulfilled. This is consistent with the findings of Rietveld *et al.* (1967) of an inverted U-shaped relationship between parameters of the visually evoked potential and checkerboard pattern complexity. Furthermore, these results suggest that preference for an intermediate degree of complexity is a consequence of intermediate complexity being associated with maximal rather than intermediate arousal potential.

Experiment III

The purpose of this experiment was to determine how a measure of response conflict other than a physiological one would be related to pattern complexity. The measure used was subjective complexity (SC) similar to subjective uncertainty (SU)

(Nicki, 1970; Nicki & Shea, 1970), and based on the number of free associations a pattern occasions and a subject's estimate of how strongly the associations are occasioned.

Method

Subjects

The subjects were 36 male and female students from the University of New Brunswick.

Apparatus

The materials consisted of a slide projector (Kodak Carousel, Model 550, 5 in., f/3·5 lens); a lenticular projection screen; 10 l complex, 10 m complex and 10 h complex slides of Expts. I and II; a stopwatch.

Procedure

There were three orders of 30 slides; the particular order viewed by the subject was randomly determined. The sequence of slides for one order was obtained by randomizing each set of l, m and h complex slides. A Latin-square arrangement provided the sequence of slides for the other groups. Each slide was presented for 20 sec. The subject was instructed to utter all the names of objects that the pattern reminded him of during this period. Also, the subject was told that, if necessary, he could continue to respond while a subsequent blank slide was on the screen. Secondly, after he stopped responding, the same complex pattern reappeared on the screen and he had to estimate, from 1 to 100 per cent, how strongly the pattern reminded him of each object he had named.

Results and discussion

SC values were calculated for each slide using the Shannon–Weaver average information measure. Each percentage estimate was identified as p_i, normalized by dividing each p_i by $\Sigma_i^N p_i$ in order that $\Sigma_i^N p_i = 1$, a primary informational requisite. Inspection of Fig. 1 indicates SC to be a U-shaped function of complexity. A Ferguson (1965) non-parametric trend analysis yielded mainly a significant bitonic component ($Z = 3{\cdot}17; P < 0{\cdot}01$).

The apparent inconsistency between these results and those of Expt. II and Nicki's (1970) study may be resolved if one assumes that response conflict and SC are inversely related. Berlyne's (1957) findings that increases in response conflict result in longer reaction times lend credence to this notion. That is, owing to the nature of the concept 'conflict', when two or more response tendencies are occasioned simultaneously, the result is a delay or even an omission of an overt response.

Secondly, SU, as defined in Nicki's (1970) study, is a measure based on the number of guesses regarding the identity of a blurred object and a subject's estimate of his confidence of each guess. Guessing presumably reduces response conflict, at least to a partial extent. Thus the instrumental response of guessing should be energized to an extent corresponding to the amount of conflict generated by the blurred stimulus. Therefore it seems reasonable to expect SU and blurredness to be directly related. On the other hand, since free associating does not have the same consequence as guessing, many utterances should occur when response competition is low and few utterances when the converse holds. Thus SC should be inversely related to complexity. It should also follow that a measure similar to SU but based on free associations instead of guesses should also be inversely related to blurredness.

CONCLUSION

The results from Expts. I–III run counter to Berlyne's (1970) viewpoint that with respect to complexity, positive hedonic value, and presumably positive reinforcement value, reaches a maximum with moderate rather than maximal arousal potential. However, they are consistent with Berlyne's (1970) finding that the hedonic value, i.e. verbal judgements of pleasingness or interestingness of highly complex stimuli tended to rise as they became less novel, with Munsinger & Kessen's (1964) results indicating an increased preference for many-turn figures with experience, and with the finding of Jones *et al.* (1961) that over time subjects prefer higher levels of complexity. That is, increased exposure to stimuli of intermediate complexity should result in a decrease in the number of competing response categories, i.e. conflict engendered by these stimuli due to such processes as habituation and/or extinction (see Nicki & Shea, 1970). On the other hand, increased exposure to stimuli of high complexity should facilitate for a while, at least, the development of new and competing response categories, and consequently result in increased arousal increment and greater verbal preference for high complexity. Lastly, again consistent with the above, Haith *et al.* (1969) have found limb activity of 2- to 4-month-old infants to be facilitated or energized more so by stimuli, presented in a manner non-contingent upon the child's response on a panel above the child's head, of intermediate rather than low or high complexity.

This project was supported by Research Grant APA-262 from the National Research Council of Canada. The author is indebted to Mrs Jean Miller, who carried out the experiments and helped in the analysis of the data.

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(*Manuscript received* 5 *July* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 173–180
Printed in Great Britain

SHORT-TERM MEMORY IN THE DEAF: A TEST FOR SPEECH CODING

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A study was carried out to examine the feasibility of identifying by a short test whether or not any particular profoundly deaf school child uses a speech code in short-term memory for verbal material. Consonant sequences were visually presented, drawn alternately from a set of letters which had high acoustic/articulatory similarity (AS), and from another of low similarity on this dimension, but with high shape similarity (VS). Relative recall performance from the two vocabularies is taken to represent the extent to which speech coding is used.

Almost all control hearing subjects recalled more from the VS set; almost all deaf subjects recalled more from the AS set. The test thus provides a quick method of assessing the level of oral memorizing in one situation which has pedagogic relevance. Some validity is provided by the further fact that deaf subjects selected for oral facility yielded scores which were significantly different from those of deaf subjects not so selected – and different in the direction of hearing subjects. Just the same, the most oral deaf, as a group, approached nowhere near the level of speech coding used by the hearing subjects.

Caution is suggested when comparing memory span of deaf and hearing subjects using verbal material, since it can be shown that with some consonant vocabularies, the deaf may show larger spans than the hearing.

Evidence is now accumulating that profoundly deaf school children use quite different coding strategies from hearing subjects for immediate recall of consonants or words (Conrad & Rush, 1965; Conrad, 1970; Locke, 1970). Whilst hearing subjects seem largely to use a speech code – reported by Conrad (1971) to begin to emerge about the age of 5 years – strong evidence has been found in the deaf both for dactylic and visual (shape), as well as for speech coding. Conrad (1970) discussed some of the educational implications when teachers and pupils remember – and probably think – in different codes.

Conrad & Rush (1965) compared error matrices for large groups of hearing and deaf subjects, showing overall differences. Conrad (1970), examining error matrices of individual deaf subjects in one school, showed that this method could be used to determine the predominant code used by individuals. This procedure, though, is tedious and interpretation somewhat arbitrary, since generally relatively few errors per single subject may be distributed amongst many cells of an error matrix. Locke (1970) set up letter-pairs in a delayed recall paradigm using pairs which were similar either phonetically or visually or dactylically. In this case, coding strategy is determined simply by comparing the number of errors observed with different types of letter-pair. Grouping all subjects, Locke reported consistent effects which though statistically significant were quantitatively quite small.

Two practical developments from these studies now seem desirable. First, a simple more sensitive test which would permit a reasonable assessment of the code used by individual subjects, and from this, some large-scale screening of deaf populations. The latter, as well as providing pedagogic data, would be a necessary step in the search for correlates of coding strategy, such as degree of hearing loss, age, etc.

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METHOD

Studies by Conrad (1965) confirmed a hypothesis that hearing subjects use a speech code in short-term memory for visually presented verbal material, by comparing recall performance of test material with varying degrees of acoustic similarity. Effectively a similar rationale provided the basis for the present procedure.

Two consonant vocabularies were selected, each of six letters. One (B C D P T V) was designated AS, referring to assumed high inter-letter similarity on an acoustic/articulatory dimension. The second (K N V X Y Z) was designated VS, referring to visual similarity. The basis for the AS vocabulary was data for listening errors (Conrad, 1964). The VS vocabulary was essentially intuitive depending on the presence of the diagonals, but supported by data from Fisher *et al.* (1969). Though not ideal, it would nevertheless be useful merely to classify subjects into those using a speech code and those who clearly did not. The former would show more errors with the AS vocabulary. The validity of the VS designation is then not crucial. But if it is acceptable, then a subject giving more errors with the VS vocabulary would be assumed specifically to be using shape of letters as a principal memory code. The use of acoustic coding would effectively be ruled out with profoundly deaf subjects.

Using each vocabulary, letter sequences were constructed of length four to six letters. No letter was repeated in a sequence and each letter occurred equally often in each serial position. For each sequence length a test comprised 18 AS sequences and 18 VS sequences alternately presented. The letters themselves were 1·6 in. high, printed in black on white card. A sequence was presented by slotting the required cards into a concealed frame, which was then displayed to subjects. In this way all letters were simultaneously available for a duration equivalent to 1 sec. per card. At the end of this period the sequence was again concealed, and subjects reported in writing on prepared answer sheets, under a forced guessing instruction.

Hearing subjects ($n = 32$) were drawn from a state primary school and were aged 10–11. Hearing was assumed to be within normal limits. Deaf subjects were drawn from two schools. School 1 is a small private school to which entry at 11 years is governed by educational attainment, profound hearing loss and assessed ability to benefit from oral teaching. The entire school population ($n = 40$) aged 11–16 was tested. School 2 is a state school with pupils aged from 5 to 16 with no entry conditions except profound deafness, absence of mental subnormality or other known handicap. Fifty-six subjects aged 9–16 were tested. All subjects knew the names of the letters used in the test.

Subjects were tested in groups of up to eight with the deaf, and of 16 with hearing controls. Instructions were given by whatever means seemed appropriate and adequate, and enough practice trials were made to ensure that all subjects had fully comprehended, and that the experimenter knew which sequence length was most suitable for the particular group. Since the groups were approximately homogeneous for educational attainment, this presented no serious problem. Sequence length was arbitrarily assessed on the basis that there would probably be enough errors from each subject to make feasible comparison between the two vocabularies, but not so many that the task was clearly impossibly difficult. For each group, this length was of course the same for both vocabularies.

RESULTS

Although for each subject the number of letters per sequence was constant throughout the test, some subjects had four-, some five- and some six-letter sequences, reflecting age and ability. Since span itself was not an issue, sequence length is irrelevant; the crucial measure is relative recall performance. This is shown in Table 1.

Table 1 shows the mean percentage of wrong letters for the three schools according to vocabulary and sequence length. Wilcoxon matched-pairs signed-ranks tests were made to assess the difference between AS and VS sequences for all schools and all lengths (except for the subgroup where $n = 5$). For the Deaf 1 six-letter case, $P < 0{\cdot}05$; for all others $P < 0{\cdot}01$ (two-tailed). But the difference for the hearing subjects is in the opposite direction from that for the deaf. On average the hearing subjects

had much more difficulty in recalling AS letters; the deaf subjects much more difficulty in recalling VS letters.

In order, to some extent, to examine individual differences concealed in these mean scores, a simple Articulatory Index (AI) was used to permit comparison of subjects tested with different lengths of sequence. Computed for each subject, the AI was the proportion of all errors which were AS errors. Thus a high AI score would indicate a subject who found the AS vocabulary difficult to recall – a subject whom we infer to be using principally a speech (acoustic and/or articulatory) short-term memory code. A low AI value indicates a subject who, relatively, is not disturbed by acoustically similar items and indeed recalls them better than letters which may be acoustically dissimilar. The kinds of memory codes that might be used in this case will be discussed later.

What the AI gains in simplicity it does of course lose in accuracy, being particularly misleading when there are very few or very many errors with both vocabularies. For the middling range of scores, though, it reasonably reflects what it is meant to. For the purposes outlined in the introduction this is quite adequate. If the AI is to be used clinically with individual subjects, suitable error levels can be obtained by adjusting sequence length. For large screening exercises, some ambiguity can be tolerated; and where it cannot be, retesting of individuals can be resorted to.

Table 2 shows for each school the number of subjects whose AIs fall within different levels between 0 and 1. It may be noted that a Mann–Whitney U test shows the AIs for the two deaf populations to be significantly different ($z = 6{\cdot}04$; $P < 0{\cdot}001$). Nevertheless the most 'verbal' deaf group has a mean AI far below that of the hearing group. The distributions in Table 2 show clearly that the vast majority of hearing children were using a speech short-term memory code, and the vast majority of deaf children, regardless of school, were not. Only four hearing subjects had an AI below 0·50 (two of these were marginal). In the Deaf 1 school where all teaching was oral, 32 out of the 40 subjects had an AI below 0·50, and in the Deaf 2 school only a single child was not in this category.

In line with a previous indication (Conrad, 1970), there was no evidence that the degree to which profoundly deaf children use articulation in memorizing this kind of material was related to either age or intelligence. For example, at the Deaf 2 school, the 28 oldest children had a hearing loss in the better ear (averaged over the fre-

Table 1. *Percentage wrong letters for various sequence lengths*

	Four letters			Five letters			Six letters		
	n	AS	VS	*n*	AS	VS	*n*	AS	VS
Hearing	—	—	—	32	20·3	8·8	—	—	—
Deaf 1	—	—	—	25	9·8	20·0	15	13·3	17·9
Deaf 2	24	16·4	34·4	27	17·2	38·7	5	22·6	40·0

Table 2. *Number of subjects in each bracket of the Articulatory Index (AI)*

	n	0–0·20	0·21–0·40	0·41–0·60	0·61–0·80	0·81–1	Mean AI
Hearing	32	0	1	9	9	13	0·70
Deaf 1	40	9	12	14	4	1	0·37
Deaf 2	56	13	35	7	1	0	0·32

quencies 250, 500, 1000, 2000, 4000 Hz) of 94·9 db (and a mean AI of 0·32). The 28 youngest children had a mean hearing loss of 94·5 db and exactly the same mean AI. At the same school, subjects were classified into two IQ groups: 'high' and 'low'. The difference between the AIs of the groups was not significant. There are two obvious points to note here. The IQ scores were taken from school records; they represent several different tests and many different testers. A more refined classification than twofold would have been inappropriate. Secondly, few children with IQ above about 115 would be likely to be found at this school since selection at age 11 years would have placed most of such deaf children in one of the grammar-type schools for the deaf – such as Deaf 1.

The higher AI of the Deaf 1 school could easily be reflecting an intelligence difference; reliable assessment was not available. But oral ability was also a criterion for entry, and they are probably correlated. Degree of hearing loss was similar to that of the Deaf 2 school. Again, AI was not significantly related, at the 0·05 level, to a twofold age split, though at the 0·1 level it was; the older subjects having higher AI (0·34 *v.* 0·39). Although for the older subjects mean AI was still far below that of hearing children, the small age effect need not be surprising in an 'oral' school. But before accepting that this effect is due to emphasis on oral instruction, one would need at least to be sure that entry criteria had stayed constant over several years.

Few deaf subjects in either school had a mean hearing loss in the frequencies referred to above of less than 80 db. At these profound levels from a preverbal age deafness did not seem to be related to AI. A more detailed study of AI and deafness was made using the classification procedure proposed by Risberg & Mártony (1970) which takes into account hearing loss at each frequency tested. Again AI appears not to be related to hearing at any particular frequency.

DISCUSSION

In so far as this relatively simple short-term memory test can be accepted as indicative of types of code used in memorizing – at least at the level of whether or not it is acoustic/articulatory – the main purpose of the study has been achieved. That this is the crucial distinction has been discussed elsewhere (Conrad, 1970). The case is based principally on the fact that regardless of quantitative levels of recall performance the speech code is the one overwhelmingly used in this situation by hearing people. It was used by the hearing controls of this experiment, and it has been shown to have a developmental history starting at about 5 years of age (Flavell, 1970; Conrad, 1971). In this simple 'thinking' task, the deaf do not 'think' like the hearing.

Nevertheless, the use of speech coding is not all-or-none. It is inconceivable that it should be; inconceivable that subjects totally ignore all the other identification cues present in verbal material visually presented or not (Conrad *et al.*, 1965; Baddeley & Dale, 1966; Dale & Gregory, 1966). A few hearing subjects and a few deaf subjects have an AI close to 0·50, which – unless this merely reflects performance sampling error – suggests either an ill-defined code preference or some quite idiosyncratic code. In view of the simplicity of the test material, we prefer the former alternative. Then, of course, the test is one of memory, not perception. Regardless of code used, some items will be forgotten; speech coding will not preclude forgetting of items which are

phonologically dissimilar. The rationale requires no more than that in this case they would be more resistant to forgetting.

It would be surprising were there not AI differences within each of the three subject groups. That the AIs of the two deaf populations differ significantly is of greater interest. We have effectively ruled out effects of age (within the present limits). Degree of deafness, beyond the dichotomy, profoundly deaf/normally hearing, is not a variable here; there are practically no partially hearing subjects in the samples. But it is virtually certain that the two deaf populations are significantly different in intelligence because of the explicit selection criteria for entry into the schools. Learning the articulations of speech sounds is a skill which must present formidable problems to those lacking useful auditory feedback. Few hearing subjects are ever required to learn any analogous skill. So it is hard to believe that intelligence is not one critical factor in this. Again this does not necessarily mean that at the level of skill attained, articulation would provide these subjects with the most useful memory code. The results themselves are against this. Just the same we shall point out later that there are independent reasons pointing to an advantage for the use of speech coding when memorizing words, letters or digits. The two deaf populations are also known to differ in 'oral ability'; another selection criterion. We have no real idea whether this ability is independent of intelligence. With the deaf it seems unlikely to be. In any case these population differences, with age and hearing loss equated, would probably be enough to account for the AI difference. What needs to be emphasized is the very large difference in the short-term memory codes used by the 'best' deaf subjects and the hearing subjects.

It may be necessary to point out that the arbitrary device of the AI is quite irrelevant to the conclusions to be drawn from the results. Table 2 could just as well have been expressed in terms of the number of subjects who recalled more AS items and the number who recalled more VS items. The AI used here merely permits a finer classification, and highlights the extent of the deaf–hearing difference without the need to present all of the individual subjects' data.

A previous study carried out in an 'oral' school for the deaf (Conrad, 1970) suggested that about half of the pupils were using speech coding. The arbitrariness of the classification procedure then used was mentioned earlier. It now appears that this value may be an overestimate. Table 2 does show that about half of the Deaf 1 subjects overlap with the hearing controls. But most of this is in the 0·40–0·60 band of the AI which is the most equivocal region. What needs to be stressed is that the pupils at the Deaf 1 school are drawn from that section of the national deaf population most likely to be 'articulate', and that this section is the minority section of the national deaf population, those judged to be likely to profit from grammar-school type of education. The evidence of this study then is that it is probable that the vast majority of deaf children who have mean hearing losses above about 80 db use quite different memory codes from those of the hearing world.

By describing the two vocabularies used in the experiment as AS and VS the clear assumption has been made that in this test deaf subjects who do not use articulatory coding do use a visual code. This is a distinction with some *a priori* validity but which needs special qualification. At the Deaf 2 school finger-spelling and signing, as well as speech, were commonly used during teaching. Manual means of communication

were commonly used between pupils. During test administration many children were observed to finger-spell letters both during the presentation of sequences and during report of them. The extent of the use of a finger-spelling code in memory needs serious consideration.

Very little study has been made of the perceptual confusability of finger-spelled letters of the alphabet or of manually signed words. The symbols used in Britain are to a large extent different from those used in the U.S.A. No such study at all has been reported for Britain. Locke (1970) has reported a restricted study of finger-spelling confusions for the one-handed American alphabet, but for a British school population we do not know which letters seem 'like' other letters. Europe and the U.S.A. use the same printed alphabet and there are many studies of shape similarity (Cornog & Rose, 1967; Fisher *et al.* 1969; Thomasson, 1970). It is relatively easy then to specify a group of letters which have marked shape similarity. But because to some extent the configurations of a number of letters in the British finger-spelling alphabet resemble their printed appearance, interpretation of the memory code is equivocal with the vocabularies used in this study. One obvious example is the letter X, which in finger-spelling crosses the two forefingers. The VS categorization, whilst reasonably correct, quite definitely does not exclude similarities other than visual. Indeed no set of alphabetic characters can be entirely free of ambiguities of this kind. This weakness, though, does not bear on the limited objectives of this study. We have unambiguously shown that deaf and hearing children use different memory codes. Just what those used by the deaf are, is a separate issue. Elsewhere we have argued that it is likely that the profoundly deaf with poor oral facility do not have available for memorizing a single code as highly developed and adapted for the purpose as speech coding is for the hearing (Conrad, 1972). If this is so, we must expect more volatile coding systems in the deaf than in the hearing; more varied coding both between and also within subjects.

These results, together with many others, imply that for the hearing, speech coding is well adapted for short-term memorizing of verbal material. There has also been in the past a considerable research effort concerned with relative memory spans of deaf and hearing subjects (Furth, 1964). It is perhaps worth elaborating on these two observations. Typically, the deaf have a smaller digit-span than the hearing. Digit names are auditorally more discriminable than are those of the 10 most discriminable consonants (Conrad, 1972). But the curvilinearity of many of the digit shapes presents considerable difficulty in visual discrimination (Cornog & Rose, 1967). On a digit-span test with visual presentation, a subject naming the digits to himself would be using a more durable code than would a subject forced to rely on the appearance of the digits; and this would be true regardless of any intrinsic biological advantage for speech coding over visual. The conventional digit-span test, then, penalizes the deaf subject, and for the same reason this may also be true when letters are used. Similarly, whilst no spoken language could tolerate much homophony amongst its words, in English there is very considerable shape-similarity amongst common words: few different lengths, and few different letters occupying the crucial first two and last positions (Baddeley *et al.*, 1960). There are of course other language-based reasons why the deaf would have poor memory spans for verbal material, but these are largely independent of the coding question.

The hazards of using specialized vocabularies to make practical assessments of memory span can be perfectly exemplified by drawing from our own data. Using the scores from the VS vocabulary, we can match two groups, each of 18 subjects, one drawn from the Deaf 1 school, the other from the hearing subjects, and all tested on five-letter sequences. Both groups have a mean error score of 6·3 per cent. The hearing subgroup, as it happens, are a little better than the whole hearing group, whilst the deaf subgroup are the best of their population. But this is beside the point. On the AS vocabulary, the deaf subjects have an error score of 3·8 per cent; the hearing subjects, 13·7 per cent. The difference is highly significant. Now if for some reason the two groups had been tested only using the AS vocabulary we should have been able to report the unique, and bizarre, 'fact' that the profoundly deaf had a better memory span for letters than the hearing. It is simply that our AS vocabulary is well adapted for non-speech coding.

That some subjects, profoundly deaf from an age before they could have had useful spoken language experience, do learn to use articulation in their mental verbal behaviour must be indisputable. But even when we have included a selected 'oral' group we have comfortable evidence of not many more than 6 per cent. True, the total sample is small (96); true, the behaviour sample (short-term memory for letters) is narrow. But this restricted evidence could be pointing to a massive problem of 'oral' deficiency. We are neither assessing nor discussing speech quality nor verbal comprehension here. In some ways it may be thought that the pedagogic problem is more serious, since it is possible that the vast majority of these deaf children have no capability for thinking verbally. True, as Furth (1966) has shown, thinking does not require words. Furthermore, in effect, it is only the speech representations of words that the deaf may lack. But the nature of the educational procedures most likely to succeed with children who do not use speech forms in thinking – and are unable to – merits consideration. At a more tractable level, one might merely wonder what determines whether a profoundly deaf infant does come to learn to use articulatory correlates of words in thinking, and what are the consequences for, for example, reading in their absence?

Because of the remote possibility of ill-informed comparisons being made, which are not implied in the results, the author prefers not to name the schools where this study was made. But he is most grateful for the generous collaboration offered by the staffs and pupils of all of them. Mrs J. I. Phillips and Mrs B. C. Weiskrantz assisted at all stages.

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(*Manuscript received* 14 *August* 1971)

Br. J. Psychol. (1972), **63**, 2, *pp.* 181–189
Printed in Great Britain

INFORMATION SELECTION, AROUSAL AND MEMORY

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Two experiments were performed to investigate the effects of noise-induced arousal on immediate recall of visually presented paired-associate adjectives. Results of Expt. I suggest that the established finding that high arousal during learning impairs immediate recall is probably an artifact produced by the standard experimental technique of randomizing list order for each anticipation trial. When order is maintained over successive anticipation trials lists are better learned in noise, indicating that order cues receive heavier weighting in that situation. In a second experiment, which employs an A-B-A retroactive inhibition (RI) paradigm, RI is less when noise is present during the learning of list A, and greater when noise is present during the learning of list B. The results from the two experiments are interpreted as demonstrating increased processing of input in states of high arousal (as produced by temporary changes in environmental stimulation).

In memory experiments, no matter what the experimental paradigm, there is a presupposition that the nature of storage processes may be inferred from the relation between what is input and what is received in recall. But since the subject is not linked 'on line' to an input he has the freedom to attend selectively, both between alternative information sources and within a given stream of input information. This *information selection* stage, in the sense that it determines whether a particular chunk of input will even achieve full storage status, is the greatest single threat to the validity of the experiment. We may not infer differences in storage conditions between two situations if it cannot be assumed that the pattern of information selection was the same in each.

The intimate relations between attention and memory have been recognized and fully treated in recent literature (Trabasso & Bower, 1968; Norman, 1968), though research has been almost totally confined to the search for plausible *mechanisms* of attention (Broadbent, 1958; Treisman, 1969; Deutsch & Deutsch, 1963; Norman, 1968). Little is known of the *dynamics* of the selective process: those functions which determine the stimulus mode, the stimulus and the parts of that stimulus to be selected for processing at any given moment. Where selective behaviour has been examined in performance of monitoring tasks there is strong evidence that alterations in task load, the priority of subcomponents of the task, or the level of arousal of the subject cause changes in the information selection strategies he uses (Hamilton, 1969; Kanarick & Peterson, 1969; Hockey, 1969). These studies have indicated that as the load of the task is increased, more and more optimal dispositions of attention are made between the various information sources available to the subject (Hamilton, 1969), and the same effect is observed as noise-induced arousal is increased (Hockey, 1970*a*, *b*). Clearly if such changes occur as a result of experi-

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mental manipulation in the memory experiment, there will be a problem in deciding whether differences in memory output under two conditions are due to differences in selection of input information or to the action of decay, interference or consolidation processes during the storage period.

Hockey & Hamilton (1970) have presented evidence which indicates that this caution may be justified. Words were visually presented in sequence in a simple immediate memory paradigm. It was 'irrelevant' to the experiment that they appeared on different parts of the viewing screen. After recall of the words subjects who performed the task in quiet conditions were found to have a higher degree of incidental learning of word location than those who performed in noise. Overall word-recall scores, irrespective of order, did not differ between quiet and noise. This was taken as representing 'narrowing' of cue-selection to exclude irrelevant cues in high arousal. It was further found that when word recall was scored on a strict 'order of report' criterion performance in noise was considerably better than in quiet. It appears that the saving in sampling of irrelevant cues had been 'invested' in preserving order.

In the first experiment this finding is investigated in greater depth using a paired-associate learning paradigm. It is 'traditional' in these experiments to ensure that after a first exposure of the list of associates a randomized order of the list is presented on subsequent recall trials. This ensures that the strength of the stimulus-response link is tested free of any list order cues. If list order remains fixed for the duration of the test any difference in performance compared with the randomized order method presumably derives from the availability of such order cues. It is assumed that manipulation of noise level will create different levels of arousal, and that comparison of fixed and random order recall will give some insight into cue-selection behaviour at these levels.

EXPERIMENT I

Method

Materials

A list of 10 pairs of bisyllabic adjectives (drawn from the Thorndike–Lorge count, and all having a frequency of occurrence of over 50 per million) was made up in the form of slides. Each slide contained a stimulus word, printed at the top left-hand corner, and a response word, printed in the bottom right corner. Another set of slides was constructed, containing only the stimulus words.

Subjects

One hundred subjects were used in the experiment; in the first instance 43 housewives from the panel of the Applied Psychology Unit, Cambridge, and in a later complete replication 57 students at the University of Stirling.

Design

Subjects were tested in independent groups, performing under one of the four possible combinations of noise condition (noise (N) and quiet (Q)), and presentation form (fixed (F) and random (R)). The groups are denoted NF, NR, QF and QR.

The noise

Steady, broad-band noise was produced from a sound generator and projected into the testing room to give one of two sound pressure levels (measured at the centre of the group): 'quiet' – a background of 55 dBC, and 'noise' – a level of 85 dBC.

Procedure

Subjects were instructed that they would be shown 10 pairs of slides, as described above, and that they were required to learn to respond with the second word when presented with the first. In the test session, each stimulus word was presented alone for 5 sec. and then presented with its response member for a further 3 sec. A blank slide was used to mark the beginning and end of the list. Subjects were asked to make their anticipation response during the 5-sec. exposure of the stimulus word and to check it with the slide following. Responses were written down on separate pages of a specially prepared booklet. An invigilator checked that subjects did not write anything while the stimulus-response pair was on the screen. Subjects were told that the list would be shown right through once, then the anticipation trials would take place. Naturally, only one trial could be run before the subjects knew whether the list order was repeated or not. However, scores on the first two anticipation trials were taken, in order to observe any carry-over effects from trial 1 to trial 2.

For the 'fixed' groups (QF and NF) the order of pairs in the two anticipation trials was identical to that in the preliminary run-through. For the 'random' groups (QR and NR) the order was changed, no pair occupying the same position in the three presentations. No instructions whatsoever were given about order changes. The noise level remained at 55 dBC for the Q group, but was raised from 55 to 85 dBC after instructions for subjects tested under N conditions. Subjects in all groups were told that the experiment was concerned with how well people could learn when background noise was present.

Results

Recall scores. Table 1 gives the mean percentage correct anticipations on each of the first two anticipation trials for the four groups. Housewives and students results are shown separately but pooled for statistical purposes as the same trends are present in the two samples. It is evident that the pattern of performance is very similar on trial 2 to that on trial 1, though scores are higher. Recall tends to be better for fixed presentation, and there seems to be a marked interaction between noise condition and presentation form. An analysis of variance was carried out on the combined data and is summarized in Table 2. The largest main effect was that of trials ($F = 53{\cdot}20$; d.f. = 1, 96; $P < 0{\cdot}001$), and the fixed *v.* random main effect

Table 1. *Mean number of correct anticipations on two recall trials for all combinations of quiet and noise (Q, N) and fixed and random recall order (F, R)*

Condition ...	QF		NF		QR		NR	
Trial ...	1	2	1	2	1	2	1	2
Housewives (n = 43)								
Mean no. recalled	2·33	3·22	4·89	5·00	2·00	3·62	1·00	2·50
n	9		9		13		12	
Students (n = 57)								
Mean no. recalled	3·36	5·21	5·07	6·80	3·62	4·85	3·47	5·27
n	14		15		13		15	
Combined (n = 100)								
Mean no. recalled	2·96	4·43	5·00	6·12	2·85	4·27	2·37	4·04
n	23		24		26		27	

was also significant ($F = 5{\cdot}69$; d.f. = 1, 96; $P < 0{\cdot}05$). The most illuminating result was the significant interaction between noise conditions and presentation form ($F = 4{\cdot}85$; d.f. = 1, 96; $P < 0{\cdot}05$). From Table 1 this interaction can be interpreted as showing that recall in noise improves strongly relative to quiet when fixed rather than random order recall is demanded. Since there was no interaction ($F < 1$) between noise, presentation form and trials it can be assumed that this advantage for the noise group under fixed order conditions is still present on the second recall trial. Median tests were carried out between all relevant pairs of conditions. The only reliable differences found were between NF and NR ($P < 0{\cdot}01$), and NF and QF ($P < 0{\cdot}01$).

Table 2. *Summary of analysis of variance for anticipation data of Table* 1 (*housewives and students combined*)

Source of variation	S.S.	D.F.	M.S.	F	P
Between subjects					
Recall order (R)	63·70	1	63·70	5·69	< 0·05
Noise (N)	17·60	1	17·60	1·57	n.s.
R × N	54·27	1	54·27	4·85	< 0·05
Error	1073·81	96	11·19	—	—
Within subjects					
Trials (T)	117·05	1	117·05	53·20	< 0·001
T × P	2·70	1	2·70	1·23	n.s.
T × N	0·33	1	0·33	0·15	n.s.
T × P × N	0·16	1	0·16	0·07	n.s.
Error	211·50	96	2·20	—	—
Total	1541·12	199	—	—	—

Discussion

The interaction between noise condition and recall order suggests that more order information is stored in noise, with no significant decrement in the storage of S–R link information. A randomized recall paradigm thus underestimates the difference between high and low arousal performance. The fact that the effect persists over trials may indicate that it is intrinsic to the high arousal condition and not due to strategic differences in cue selection methods.

The process by which the additional information is acquired is not yet evident. One possible explanation is that there exists some limit to the momentary level of activity in a given trace (i.e. the S–R link), and if this is achieved and maintained during input the remaining attentional capacity is 'time-shared' to other sources of retrieval information – in this case order cues. Since Hockey & Hamilton (1970) report that attention is better 'centred' on the learning task when arousal is high it may be that the consequent increase in processing power is manifest only by stronger ordering of the input. We have as yet been unable to test this hypothesis directly.

Experiment II

In the second experiment, the nature of the advantage gained by learning fixed-order lists in noise is examined. This advantage appears to derive from an increment in order information rather than in the number of items stored above some recall threshold. Such information presumably has some 'cue' function which aids item retrieval – perhaps by 'reviving' the item trace. Where retrieval is aided or accomplished by such cueing longer term memory is likely to be the beneficiary. The distinction between long- and short-term components of memory may be crudely represented as one between trace activity which is conditionally evoked by the presence of activity at another part of the store (long-term), and activity which is extant and accessible in its own right (short-term). Where more complete ordering of a set of inputs is achieved we should expect that if in the future one of that set is raised to full strength a more thorough activation of other inputs will result. In the present paradigm, where fixed-order paired-associate lists are used, material learned under high arousal should not only be better recalled in the short-term, but display lower rates of loss over time. Since increased input processing is the proposed source of such effects, the material should also interfere more with previously learned lists (Waugh & Norman, 1965). These proposals may be examined by use of a retroactive inhibition paradigm of the form A-B-A_1, where A and B are separate paired-associate lists and A_1 is a retest of A. A or B or both can be learned in noise or quiet. In the present case A_1 is always carried out in quiet, since the data of Berlyne *et al.* (1966) indicate that the effects of arousal are concentrated in the learning process rather than at the point of recall.

Method

Materials

Two lists of 10 pairs of bisyllabic adjectives were used (lists A and B).

Subjects

The subjects were 66 undergraduates of Durham University, randomly assigned to four groups of 16, 16, 17 and 17.

Design

The four conditions compared were N-N-Q, N-Q-Q, Q-Q-Q and Q-N-Q where N and Q signify the conditions under which A, B and A_1 were recalled. Four independent groups were assigned, one to each condition. All groups had the adjective lists administered in the same order. The 'fixed' form of presentation was used throughout for all groups.

For each list, after a preliminary exposure, learning proceeded for three anticipation trials. Learning of the two lists was followed by two trials of recall of the first list presented.

Noise

Noise and quiet conditions were as specified for Expt. I.

Procedure

The presentation procedure was as described for Expt. I, and the interval between different list presentations was two minutes. Subjects were told of the next phase of the experiment only after the completion of the previous one. They were unaware that they were to be retested on list A until after they had completed list B.

Results

The four treatment groups are denoted QQ, QN, NN and NQ, signifying the learning conditions for lists A and B. The appropriate comparisons in assessing the effects of noise on recall of list A are as follows. (*a*) Resistance to interference: QQ *v.* NQ, QN *v.* NN, QQ and QN *v.* NQ and NN. That is, the effect of noise *v.* quiet during the learning of list A with list B conditions held constant. (*b*) Degree of interference: QQ *v.* QN, NQ *v.* NN, QQ and NQ *v.* QN and NN. That is, the effect of noise *v.* quiet during the learning of list B with list A conditions held constant.

The data obtained are shown in Table 3. Loss due to retroaction is expressed as a percentage of the score on the last trials of list A. Thus

$$\text{Percentage loss} = 100 \times \frac{(\text{Terminal trial A} - \text{first retroaction trial})}{\text{Terminal trial A}}\ \%.$$

Loss figures for the four conditions were submitted to analysis of variance. The results are given in Table 4. Significant differences are indicated due to noise at List A ($F = 4{\cdot}98$; d.f. = 1, 62; $P < 0{\cdot}05$), and to noise at list B ($F = 4{\cdot}32$; d.f. = 1, 62; $P < 0{\cdot}05$). From Table 3 these findings appear to indicate that the effect of

Table 3 (*a*). *Mean number of correct anticipations for lists A, B and A (retroaction) under four combinations of learning condition (QQ, QN, NN, NQ) for lists A and B*

Trial ...	List A			List B			A (retroaction)		
Condition ...	1	2	3	1	2	3	1	2	*n*
QQ	3·82	5·24	7·18	3·76	6·41	8·35	5·82	8·59	17
NQ	5·88	7·88	8·69	5·75	7·38	8·50	8·19	9·38	16
QN	4·25	6·12	7·44	5·81	7·69	8·69	5·62	8·25	16
NN	4·88	6·00	8·25	6·00	8·00	9·33	7·06	9·06	17

Table 3 (*b*). *Mean absolute and percentage losses in recall from trial* 3 *list A to trial* 1 *list A (retroaction)*

	Absolute loss	Percentage loss*
QQ	1·36	18·53
NQ	0·50	6·54
QN	1·82	24·54
NN	1·19	17·88

* Calculated on mdividual percentage losses.

Table 4. *Summary of anlysis of variance of percentage loss data of Table* 3

Source of variation	S.S.	D.F.	M.S.	*F*	*P*
List A noise conditions (A)	1,345·51	1	1,345·51	4·98	< 0·05
List B noise conditions (B)	1,166·96	1	1,166·96	4·32	< 0·05
A × B	200·74	1	200·74	0·14	n.s.
Error	16,756·98	62	270·27	—	—
Total	19,570·19	65	—	—	—

noise on list A with list B conditions held constant is to reduce the rate of forgetting, and that the effect of noise on list B with list A conditions constant is to increase the rate of forgetting. Furthermore, the lack of interaction ($F < 1$) between noise conditions and lists implies that the effects of noise on first list retention and second list interference potential may be symmetrical (i.e. equal and opposite). To assess this possibility more fully individual conditions were compared by median test. The results are summarized in Table 5. From these data it is apparent that a slightly larger contribution to the overall effect derives from the low loss rate of condition NQ than from the high loss rate of condition QN. Though this asymmetry is not sufficient to bring about a significant interaction it does tend to preclude the use of the 'equal and opposite force' notion mentioned above.

Table 5. *Summary of results of median tests between each relevant pair of conditions NQ, NN, QN and QQ, in their effects on percentage loss*

Comparison	Effect tested	P
NQ *v.* QQ	Q *v.* N during list A	$< 0{\cdot}02$
NQ *v.* NN	Q *v.* N during list B	$= 0{\cdot}05$
QN *v.* QQ	Q *v.* N during list B	$= 0{\cdot}1$
QN *v.* NN	Q *v.* N during list A	$> 0{\cdot}1$

Discussion

The data indicate that high arousal at the time of storage aids the preservation of input material in the long term. This finding agrees with that of other workers in the field who have reported that same effect whether arousal is manipulated by means of noise (Berlyne *et al.*, 1966; McLean, 1969) or by incorporating emotive words in the list to be learned (Kleinsmith & Kaplan, 1963, 1964; Walker & Tarte, 1963). These workers also report that high-arousal items tend to be less well recalled in the very short term. Since all use a type of paired-associate learning task with randomized order recall, like conditions are found in the comparison QR *v.* NR of Expt. I. Here, 'noise' recall is slightly but not significantly worse than quiet. Thus in their overall form these data are not unlike those previously reported. It is in accounting for the effects of list order that difficulty is met. The only current formulation which purports to explain arousal effects is trace consolidation theory (Walker, 1967). This states that each input is subject to consolidatory activity which operates to make the trace more permanently available. The higher the level of consolidatory activity evoked by a trace, the less accessible is that trace in the short term. Thus high-arousal items are poorly recalled immediately after learning, but well registered in long-term memory. Since it is shown in Expt. I that short term recall of noise-learned material is highly dependent on the order of recall demanded some modification of this theory seems necessary.

It must first be determined whether the order effect of Expt. I persists when arousal items are events occurring *within* a list (emotive words). If this is so then clearly some variable is being removed from the situation by randomization procedures, and this variable is a likely candidate for the function of ensuring long-term availability of the trace.

The relationship between arousal and memory becomes even more complex when

results from time-of-day and sleep deprivation studies are considered. Here, opposing trends have been observed between information processing ability (continuous arithmetic) and simple tests of immediate memory (see Blake, 1967; Baddeley *et al.*, 1970; Hockey & Colquhoun, 1971; Hamilton *et al.*, 1971). Most of the studies in this field have required ordered recall – from the data of Hockey & Hamilton (1970) and from Expt. I, we might expect performance to be better in aroused states. Yet at times of day when processing performance is good, and arousal is assumed to be high, short-term memory is at its poorest. Not enough data exist to help in unravelling this particular problem – it may even be that memory effects observed due to the shifts in the basal level of arousal are of a quite different origin from those achieved by imposing temporary arousal shifts on the subject. The naturally occurring diurnal changes in arousal may result in a trade-off between processing speed and retention, while phasic shifts, due to noise, or drugs, or other temporary changes in environmental stimulation, may serve the purpose of allowing the organism the possibility of over-riding this limitation, to meet specific processing demands. Clearly, however, there is a great need for careful experimental work in this area.

Original work for this manuscript was done at the Medical Research Council Applied Psychology Unit, Cambridge, with expansion and replication at the Universities of Stirling and Durham.

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(*Manuscript received* 11 *May* 1970; *revised manuscript received* 21 *April* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 191–194
Printed in Great Britain

VIGILANCE DECREMENT AND SPEED OF HABITUATION OF THE GSR COMPONENT OF THE ORIENTING RESPONSE

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The present study was designed to investigate the relationship between auditory vigilance performance and speed of habituation of the GSR component of the orienting response (OR) to auditory stimuli. Fast habituators ($n = 7$) displayed a greater rate of vigilance decrement than slow habituators ($n = 7$), suggesting a more rapid onset of a phase of low arousal in these subjects.

The maintenance of attention over relatively long periods has been conceptualized as vigilance, defined as the readiness to detect and respond to certain specified changes occurring at random time intervals in the environment (Mackworth, 1957). Generally, a vigilance task requires subjects to respond to a critical or 'wanted' signal which occurs randomly against a background of neutral or 'unwanted' events. Although the most common finding has been a decline in performance efficiency over time (vigilance decrement), the rate of decrement appears to be a function of the task used, the stimulus conditions, and certain organismic states.

Studies relating a number of variables to vigilance performance have been reviewed by Mackworth (1969, 1970). She suggests that vigilance decrement is due, in part, to habituation of the arousal or orienting response (OR) produced by the repetitive background events. The OR, which is defined as the complex of sensory, muscular, electroencephalographic and vegetative changes elicited by a novel stimulus (Sokolov, 1963), extinguishes or habituates following repeated stimulus presentations. Speed of habituation may be defined as the number of stimulus repetitions necessary for extinction of the response and, in neo-Pavlovian theory, is considered a prime index of speed of development of internal inhibition (Nebylitsyn, 1966).

However, there are large individual differences in speed of OR habituation (Lynn, 1966), and in terms of Mackworth's proposal, therefore, fast habituators might be expected to display a greater rate of vigilance decrement than slow habituators. The present experiment was designed to test this hypothesis.

Vigilance has also been conceptualized in terms of signal-detection theory (e.g. Broadbent & Gregory, 1963; Binford & Loeb, 1966; Swets & Kristofferson, 1970), where it is argued that ability to detect signals (d') may remain the same, while decision criteria (β) become more conservative. To aid data interpretation, a pilot study ($n = 8$) was conducted from which d' and β values were calculated in the manner described by Binford & Loeb (1966). Analysis of these data indicated a significant deterioration in d' as a function of time on task ($F = 4{\cdot}45$; d.f. = 4, 48; $P < 0{\cdot}01$), but no significant change in β ($F = 1{\cdot}24$; d.f. = 4, 48; $P > 0{\cdot}05$). Thus deterioration in performance on the present task, with the particular instructions used, reflects changes in observer sensitivity but not response criterion.

METHOD

Subjects

The subjects were 14 male students (age range 18–25 years), selected from a pool of 35 subjects as fast or slow habituators of the galvanic skin response (GSR) component of the OR. Subjects were allocated to fast ($n = 7$) or slow ($n = 7$) habituation groups depending on whether their scores were one S.D. (8·00) above or below the sample mean (12·00).

Apparatus and procedure

Electrodermal activity was measured in response to a stimulus of 2 sec. duration, presented from an audio-oscillator at randomly ordered intervals of 15, 20 and 25 sec. using a preprogrammed magnetic tape driven by a Sony tape-recorder. Tone frequency was 1000 Hz, and the intensity 60 db SPL at the ear-phones through which it was presented. GSRs were measured from the palmar surface of the middle finger of the subject's non-preferred hand, using a silver electrode; a second (ground) electrode was attached to the palmar surface of the same forearm, 2·5 cm above the wrist. A current of 20 μA d.c. was impressed across the electrodes by an E & M Physiograph Six preamplifier. A preamplifier sensitivity of 1 mm of pen deflexion, equal to 500 Ω, was used, and GSRs of less than 500 Ω were not accepted as responses.

Each subject was seated in a semi-reclining position and the ear-phones adjusted to give a firm but comfortable fit. After a 5 min. rest period, the stimulus programme was commenced without prior warning to the subject, and continued until the criterion for habituation, three consecutive responses of less than 500 Ω, had been reached. GSRs of 500 Ω or greater, occurring from 1 to 4 sec. after stimulus onset, were considered responses evoked by the stimulus.

The vigilance task, which was performed 3–4 weeks later, required subjects to listen to a tape-recording of 1000 Hz tones of 1 sec. duration, occurring at a rate of one every 2 sec. The stimuli were delivered through a speaker 120 cm in front of the subject, and their intensity was 55 db SPL at the chair in which he sat. The signals consisted of tones 2 db louder than the background events. During the main testing session, there were 1500 stimulus presentations, 25 of which were signals arranged so that five occurred at random intervals in each 10 min. period of the 50 min. task. The subject's task was to press a response bar on the arm of the chair when he was reasonably sure a signal had occurred; this activated a buzzer, detectable to the experimenter but not to the subject.

At the beginning of testing, the nature of the task was explained to the subject and his wrist-watch and cigarettes removed. A short segment of tape (1 min.) containing 28 events and two signals was presented with feedback of results. Practice continued until the subject could clearly detect the signals. A 10 min. pretest session containing five signals and 295 events was then performed, and after a 3 min. rest pause, the main 50 min. session was commenced. At the end of this, there was a 5 min. rest period followed by a 10 min. post-test session containing five signals and 295 events. During the pretest, main, and post-test sessions, there was no communication between the subject and experimenter.

The GSR habituation and vigilance sessions were performed in a sound-dampened room, and in both cases, the apparatus was monitored by the experimenter from an adjoining room.

RESULTS

A two-factor analysis of variance with repeated measures on one factor (Winer, 1962) was performed on the number of correct detections during the five 10 min. intervals of the main session for the fast and slow habituation groups. This analysis yielded a significant intervals effect ($F = 3{\cdot}44$; d.f. = 4, 48; $P < 0{\cdot}05$), indicating significant performance decrement over time, as well as a significant habituation × intervals interaction ($F = 4{\cdot}31$; d.f. = 4, 48; $P < 0{\cdot}01$). Fig. 1, which shows the performance of fast and slow habituators plotted over time, clearly demonstrates the nature of the habituation × intervals interaction. Tests for simple habituation effects indicated that the performance of slow habituators was superior to that of fast

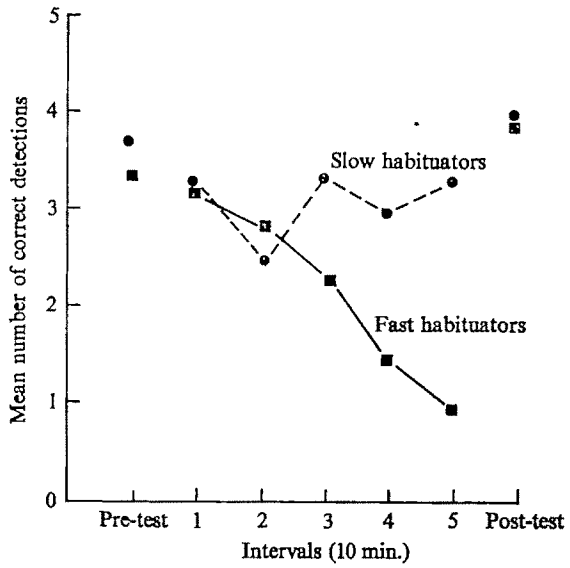

Fig. 1. Detection performance of fast and slow habituators during successive 10 min. intervals.

habituators at intervals four ($F = 5{\cdot}94$; d.f. = 1, 60; $P < 0{\cdot}05$) and five ($F = 9{\cdot}30$; d.f. = 1, 60; $P < 0{\cdot}01$), although there was no difference between the groups at interval one ($F = 0{\cdot}10$; d.f. = 1, 60; $P > 0{\cdot}05$).

For each subject, number of correct detections in each 10 min. interval was regressed on interval number, the slope score thus obtained reflecting rate of vigilance decrement. Fast habituators displayed a significantly greater rate of decrement than slow habituators ($t = 5{\cdot}82$; d.f. = 12; $P < 0{\cdot}001$).

Discussion

The present study provides clear evidence that with the task used here, rate of vigilance decrement is related to speed of GSR OR habituation in the modality of the vigilance task, and therefore, according to Nebylitsyn (1966), to speed of generation of internal inhibition.

The most parsimonious explanation of the data is that as the OR evoked by the signals is extinguished, fewer signals are detected. One implication of this notion, which is similar to Mackworth's (1961) suggestion that vigilance decrement can be viewed as extinction of a conditioned voluntary response, is that higher signal rates will produce more rapid decrement. The evidence, however, indicates the reverse: less decrement occurs with higher signal rates (Kappauf & Powe, 1959).

An alternative interpretation may be offered in terms of habituation of the OR to the background events. It is generally accepted that drowsiness or a state of low tonic arousal follows OR habituation (Sokolov, 1963; Gastaut & Bert, 1961), and rapid habituators may be expected, therefore, to show an early onset of this state of low arousal. The existence of such a state during vigilance performance is well docu-

mented; skin conductance (e.g. Stern, 1966), EEG (Daniel, 1967) and blood concentration of adrenalin (O'Hanlon, 1965) indices of arousal have all been shown to decrease during a vigilance task. Thus the present results may be due to the more rapid onset of a low arousal phase in fast than in slow habituators.

This investigation was carried out at the University of Queensland, Australia.

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(*Manuscript received* 23 *July* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 195–203
Printed in Great Britain

ACQUISITION AND REVERSAL OF ORIENTING RESPONSES TO WORD SIGNALS

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Acquisition, reversal, adaptation and inhibition of the orienting response were investigated in a mentally retarded sample and its CA and MA matched normal groups. GSR frequency, magnitude and latency were used as indices of the OR. The retarded subjects maintained and modified their ORs as efficiently as the normals during acquisition and reversal. OR decrements to non-signals and to former signals were distinguished as adaptation *v.* inhibition.

Maltzman and his associates have recently demonstrated that the orienting response (OR) can be conditioned and measured like any motor response (Maltzman & Raskin, 1965; Maltzman, 1968; Maltzman *et al.*, 1970). It can be conditioned simply by instructing the subject to attend to every word presented to him and asking him to push a button in response or by asking him to count the number of auditory signals presented. Although, Maltzman and his co-workers have examined the OR to verbal stimuli in a variety of tasks, they apparently have not studied the relative ease or difficulty of reversing ORs to verbal stimuli. The present study examines both acquisition and reversal. It should also throw light on developmental differences, as well as on possible normal and retardate differences.

The retarded child is sometimes characterized as having a weak verbal system leading to difficulties in maintaining an instructed set or in inhibiting or reversing a learned response (Luria, 1963). Several researchers have also assumed an attentional deficit in the retardate (Luria, 1963; O'Connor & Hermelin, 1963; Zeaman & House, 1963). Both verbal and attentional inadequacies should be critical factors in an experiment on the acquisition and reversal of the OR. Recently, however, Das & Bower (1971) found that these deficits of the retarded child could be largely circumvented through appropriate instructions so that gross differences in OR evocation did not appear when normals were compared with retardates of equal age on OR acquisition. Would it be possible to minimize group differences, especially in reversal where verbal mediation seems to play a more important part than it does in acquisition (Das, 1969)?

Another concern of this study is to examine the conditions under which decrement and extinction of OR occur. These are as important for an understanding of the nature of OR as is its sustained evocation (Lewis *et al.*, 1969). A weakening of the OR to neutral stimuli can be distinguished theoretically from OR decrement to stimuli which have been discontinued as signals. The former could be described as habituation or adaptation of the OR, and the latter as inhibition of the OR, comparable to conditioned inhibition. The neuronal model of Sokolov (1965, 1969) states that on repeated presentation of a stimulus without reinforcement, a conditioned adaptation response develops, so that every time the stimulus is presented, it acts as a signal for the activation of adaptational mechanisms. The adaptational response thus weakens

the effect of the stimulus. Moreover, Sokolov provides evidence which indicates that a conditioned inhibition of the OR requires both the preliminary activation of the adaptational response plus the 'reflex blocking of impulses that enter the reticular system' (Sokolov, 1965, p. 148). If the impulses do not reach the reticular formation, ORs cannot be evoked. In the present experiment, both processes may be apparent in reversal where the ORs to some verbal signals must be inhibited (conditioned inhibition) and the ORs to 'neutral' words should decrease gradually (adaptation).

Briefly, the experiment examines (1) the acquisition and reversal of ORs (indexed by the GSR) to verbal stimuli whose signal values have been established through instructions; (2) the adaptation of unconditioned ORs to 'neutral' stimuli; and (3) the conditioned inhibition of the OR to signals that become non-signals during reversal. Retardates are compared to both a mental and a chronological age matched group of normals. Further, the normals are compared with each other to focus on any developmental differences in the previously listed variables.

Method

Subjects

Three groups of male subjects, educable mentally retarded (EMR) and children of normal intelligence matched on MA and CA, took part in the study. Following the loss of 12 subjects scattered equally over the three groups due to apparatus problems or non-responding (less than six GSRs to the first 12 meaningful signals presented following the instructions to button press), the final group totals were 27 subjects each. Table 1 shows the mean ages, IQs and MAs for the three groups. Two-tailed t tests indicated that the EMRs were comparable to their MA and CA controls. All subjects were drawn from the special (EMR) and regular classes of the city's public schools.

Stimulus material

The verbal stimuli to which the subjects listened consisted of a taped series of four familiar words (boy, baby, hand, foot) and four nonsense syllables (CVCs) of low association value (gex, yuf, wug, zuk). Six random presentations of the eight stimuli followed one another on the tape. A word or CVC was never allowed to follow itself, nor did more than two words or CVCs come in succession. The same order of stimuli was used in both the acquisition and reversal phases. The interstimulus interval was 12·5 sec. (range 10–15 sec.). Hence each cycle through the eight stimuli required 100 sec. For both acquisition and reversal there were 24 word presentations, six of each word, and 24 CVC presentations, six of each CVC. The acquisition and reversal stimuli were separated by a 2 min. interval during which reversal instructions were given. It will be noticed that of the four words 'baby', 'boy', 'hand' and 'foot', 'boy' and 'foot' were common associates of 'baby' and 'hand' respectively. During reversal, 'boy' and 'foot' became non-signals, thus requiring the inhibition of the OR and the motor response, which made the task somewhat difficult.

Table 1. *Age and intelligence data for the three groups*

		CA		MA		IQ	
Group	n	Mean	S.D.	Mean	S.D.	Mean	S.D.
EMRs (educable mentally retarded)	27	169·81	7·78	123·96	12·20	72·93	6·42
MA controls	27	114·33	10·56	123·00	17·29	107·37	9·49
CA controls	27	168·85	7·27	190·59	16·65	112·85	8·80

Procedure

The experiment consisted of four discrete parts: adaptation, practice, acquisition and reversal – which occurred in that order. In *adaptation* the subject listened to two presentations of the eight verbal stimuli mentioned earlier. This phase of the experiment was designed to reduce initial ORs to the stimuli so that subsequent ORs would be largely a consequence of instructions. For the *practice* period which followed, the subject was presented with the eight stimuli, but was instructed to press the button only for the four words: baby, boy, hand and foot. The subjects were prompted, when necessary, during this presentation. (The motor response was acquired by most subjects by the end of practice.) The *acquisition* session followed immediately. It consisted of six presentations of the eight words and CVCs. *Reversal* instructions were given next, asking the subject to press only for 'baby' and 'hand'. The same six random presentations of the eight stimuli were given again, as in acquisition.

GSRs were continuously recorded on a Hewlett–Packard 7700 series polygraph. The marker pen was used to indicate the verbal signal and button press as well as time in seconds. The paper speed was 5 mm/sec. Flat silver electrodes filled with Beckman paste were attached by adhesive collars to the palm and back of the subject's left hand, leaving the right one free to press the button.

The subject sat in a padded leather chair with wide arms, separated from the experimenter and the experimental apparatus by a partition containing a one-way mirror. The space in which the subject sat was moderately darkened.

Scoring

A GSR was defined as any downward pen deflexion of 500 Ω change or better, commencing from 1 to 5 sec. after the onset of the stimulus. To determine the ohms change, the highest point in the second before the stimulus was compared to the lowest point of the pen deflexion following the stimulus. For each GSR so identified, the latency from stimulus onset to initial pen deflexion was measured, as also the duration of the response (time from start to the greatest depression), magnitude (conductance scores including zeros for no response) and amplitude (conductance scores without zeros). Conductance was expressed in micromhos.

Design

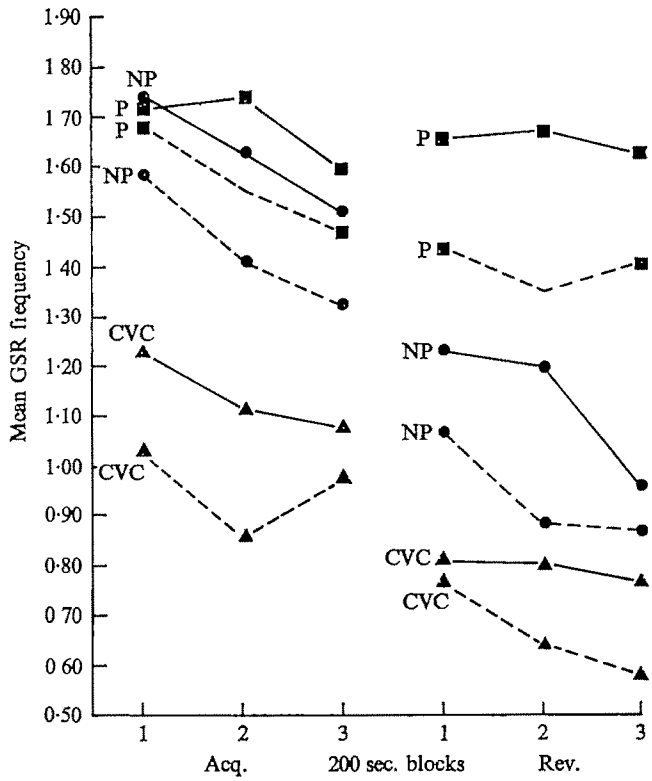
The general design involved three subject groups (EMRs and matched MA and CA controls), two classes of verbal stimuli (familiar words and CVCs), two learning conditions (acquisition and reversal) and three 200 sec. trial blocks. It was thus a $3 \times 2 \times 2 \times 3$ design with the last three factors repeated. The effects of these variables were assessed by the analysis of variance technique.

In the main analyses the subject groups were compared in pairs: retardates *v.* MA controls, retardates *v.* CA controls, and the MA *v.* CA group. Also, the familiar words were divided into press and no-press words for the analyses. Words and CVCs were contrasted in separate analyses. The same procedure was used with each of the GSR measures.

Results

Adaptation period

Initial ORs to the words and CVCs should be considerably reduced at the end of the adaptation period if one wishes to establish differential ORs to the two kinds of stimuli during acquisition. Inspection of the means in Table 2 for words and CVCs over the two adaptation blocks (100 sec.) and the 100 sec. practice period which followed adaptation suggests that the adaptation period successfully reduced the novelty value of all stimuli. But when instructions to press for only words was given, the words clearly aroused strong ORs, whereas the CVCs remained closer to the adaptation level. An analysis of variance for GSR frequencies compared the last block of adaptation



Figs. 1 and 2. Mean number of GSRs to words – press (P) and no-press (NP) – and nonsense syllables (CVCs). The maximum frequency is 2 if the subject gives one GSR to every stimulus. Each 200 sec. block mean is the average of the means of two 100 sec. blocks. CVC frequencies have further been halved to provide a comparison with P and NP words. ——, Retardates; —, MA control in Fig. 1 and CA control in Fig. 2.

trials with the first trial block in the practice period, and confirmed this in all groups of subjects.

Acquisition/reversal task: GSR frequencies

Press v. no-press words. First consider the retardate and MA control analysis. All main effects were statistically significant: groups ($F = 4{\cdot}51$; d.f. $= 1, 52$; $P < 0{\cdot}05$);

Table 2. *Mean GSR frequencies for words and CVCs for all adaptation and practice blocks*

	Adaptation				Practice	
	Block 1		Block 2		Block 1	
Group	Word	CVC	Word	CVC	Word	CVC
Retardates	2·85	2·78	1·96	1·85	3·26	2·85
MA controls	2·85	2·85	2·11	2·00	3·44	2·37
CA controls	2·56	2·44	2·19	1·96	3·56	2·41

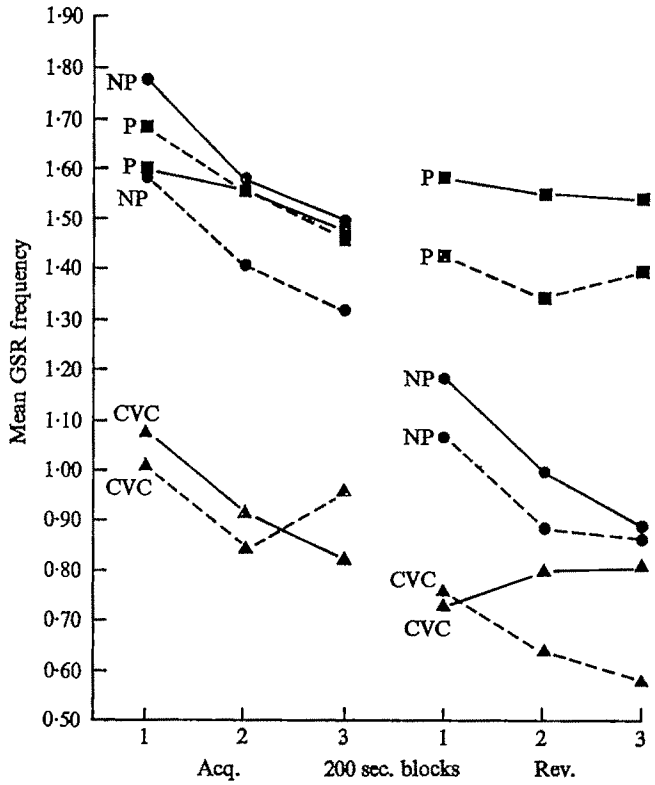


Fig. 2. For legend see facing page.

blocks ($F = 11{\cdot}79$; d.f. $= 2, 104$; $P < 0{\cdot}01$); press/no-press ($F = 54{\cdot}69$; d.f. $= 1, 52$; $P < 0{\cdot}01$). Only the press/no-press × acquisition/reversal interaction was significant ($F = 27{\cdot}23$; d.f. $= 1, 52$; $P < 0{\cdot}01$). Fig. 1 compares the retardate and MA group, where the means are the means of two adjacent 100 sec. blocks.

Examination of the figure reveals that the MA group gave more ORs than the retarded group. There was a decrease in the number of ORs, over blocks, and more were evoked during acquisition than reversal. On the whole, there were more ORs to press than to no-press words due to the fact that during reversal there were many more ORs to the press words than to the no-press words. This is as would be expected, since both press and no-press words are signals in acquisition (no difference occurs) but in reversal a response is required for only the press words (see Fig. 1). Fig. 1 also shows that the press words persistently evoke ORs for both groups without any tendency to habituate.

A comparison between the retardate and CA group revealed no significant group effect but all other main effects as in the retardate and MA comparison were significant. The main effect for blocks ($F = 8{\cdot}56$; d.f. $= 2, 104$; $P < 0{\cdot}01$), press/no-press words ($F = 55{\cdot}82$; d.f. $= 1, 52$; $P < 0{\cdot}01$) and acquisition/reversal ($F = 46{\cdot}62$; d.f. $= 1, 52$; $P < 0{\cdot}01$) were statistically significant. The press/no-press words × acquisition/reversal interaction was also significant ($F = 37{\cdot}77$; d.f. $= 1, 52$; $P < 0{\cdot}01$),

reflecting the fact that the press words do not habituate during reversal (see Fig. 2).

There was also a significant press/no-press × blocks interaction ($F = 3{\cdot}75$; d.f. = 2, 104; $P < 0{\cdot}05$) which appears to result from minimal habituation for the press words in reversal as compared to the no-press words (see Fig. 2). Fig. 2 also shows that more ORs were evoked in acquisition than reversal; and that there were more ORs for press words than no-press words. Inspection of Fig. 2 indicates that reversal instructions had an immediate effect upon the evocation of ORs to no-press words as seen earlier in Fig. 1, but that the inhibition was not complete as a subsequent decrement was still evident.

The final contrast is between the MA and CA normal groups. The results of the analysis of variance show a significant main effect for press/no-press words ($F = 37{\cdot}11$; d.f. = 1, 52; $P < 0{\cdot}01$); blocks ($F = 8{\cdot}96$; d.f. = 2, 104; $P < 0{\cdot}01$); and acquisition/reversal ($F = 35{\cdot}52$; d.f. = 1, 52; $P < 0{\cdot}01$). The group main effect is not significant. There is a press/no-press × blocks interaction ($F = 4{\cdot}27$; d.f. = 2, 104; $P < 0{\cdot}05$) and a significant press/no-press × acquisition/reversal interaction ($F = 54{\cdot}18$; d.f. = 1, 52; $P < 0{\cdot}01$). Figs. 1 and 2 reveal the direction of these effects to be the same as those of the two previous analyses of variance.

Words v. CVCs. In order to compare the number of ORs to the non-signal CVCs with those occurring to the word signals, analyses of variance were conducted in the same manner as with the press and no-press words. The only difference was that the press and no-press words were added together and contrasted with the CVCs. In all three analyses the word signals resulted in significantly more ORs than the CVCs. There were significant main effects for blocks and acquisition/reversal as before. Groups did not differ significantly. Fig. 1 depicts the mean CVCs for the retardate and MA group over blocks in acquisition and reversal. CVCs clearly evoked fewer ORs than the words did. A gradual adaptation of the ORs to CVCs can be seen over blocks. In Fig. 2 the means for the retardates and CA controls are shown. Again the discrepancy between words and CVCs is obvious.

Analyses of magnitude and latency scores

Conductance change, expressed in micromhos, was computed for each GSR occurrence. Since subjects did not respond to all presentations of neutral stimuli (CVCs) or when the no-press words in reversal were presented, *amplitude* scores for the evoked GSRs would not be as representative as magnitude scores. Magnitude scores are mean conductance changes calculated separately for press, no-press words and CVCs. Thus if the subject had given only one GSR during the 200 sec. period when four no-press words were presented, his magnitude score would be the amplitude of the one GSR divided by four. Prokasy & Ebel (1967) have used this advantageously where many missing GSRs were evident.

The results of the analyses of variance on the magnitude scores are summarized below. The analysis of variance design was identical to that of the frequency analysis. On the whole, the magnitude results agreed with the frequency analyses. Comparing the main effects, no group differences were significant other than the retardate and MA contrast. Words had a higher magnitude than CVCs, and within words, the press words had a higher magnitude score during reversal than the no-press words, as

expected; block effects, where significant, implying a decrement in magnitude over the three 200 sec. blocks.

The *latencies* were of two kinds as described before – response latency calculated from stimulus onset to evocation of the GSR, and the duration of the GSR response. Both measures appeared to be less sensitive than frequencies and magnitudes. The analyses of variance using a design identical to that for the frequency and magnitude analyses revealed no overall group differences in latency for GSR evocation. Generally, latency increased over blocks, and was longer for reversal than for acquisition. On the other hand, duration of GSR showed group differences: the young normals had shorter mean duration than both the retardates and the older normals. But the two older groups did not differ in GSR duration. The duration of GSR to words increased consistently over blocks, whereas it decreased for CVCs. The duration was also shorter for CVCs when compared to words during reversal. ORs to the words showed longer duration during reversal than they did in acquisition.

Motor response

All subjects were required to press the button for the four meaningful words during the acquisition phase of the task and to inhibit these presses to two of the words during the reversal phase. (Of special interest would be the number of presses made during reversal to the two words that were correct earlier. This would allow an appraisal of the correspondence between the inhibition of a motor response and an OR.) However, very few false detections occurred: nine retarded subjects, six MA controls and three CA controls made an error. Thus it is apparent that subjects could inhibit the motor response almost completely following instructions, whereas this was not true of the autonomic response.

Discussion

The results show that instructions can serve as an effective tool in the development of ORs to verbal stimuli under conditions of both acquisition and reversal. In acquisition, ORs were evoked by the signal stimuli immediately following instruction, as evidenced by a sudden increase in GSR frequency after adaptation. Similarly, the reversal instructions requiring the subject to inhibit a motor and autonomic response to two of the signal words resulted in a sharp reduction in the ORs to the non-signals, whereas the ORs to the signals were maintained at a high level. Obviously, during both learning conditions subjects were able to interiorize the instructions so that they would effectively control both their motor and autonomic responses. The process involved was probably one of implicit verbal mediation (Kendler, 1963). As Wolff (1967) suggests, all discrimination learning situations, verbal and non-verbal, owe a heavy debt to verbal mechanisms.

Also of considerable interest is the lack of crucial subject group differences. For, in spite of the assumed deficit of the retarded subjects in attention, motor response execution and verbal mediation, they were very similar in their performance to normals of equivalent CA and MA levels. (MA subjects gave more ORs to press than to no-press words and had shorter GSR durations than the older retarded subjects. This reflects a higher arousal level.) The lack of group differences between retardate and CA comparisons agrees with our previous work (Das *et al.*, 1971; Das & Bower,

1971) which showed retardates to perform as well as their CA controls in the maintenance of ORs to verbal signals in a vigilance-type task. Thus no evidence against the retardate's ability to make use of the rudimentary verbal mediation involved in these tasks was obtained. Hermelin & O'Connor (1958) also found that imbeciles could use verbal concepts. The present experiment, however, would appear to be more difficult than our previous tasks in that reversal was involved. The reversal task was made more complex by requiring the common associates of two signal words (baby and hand) to be inhibited. Nonetheless, the retarded subjects did as well in acquisition and reversal as the normal CA controls.

A possible explanation of the lack of group differences may be suggested here. The experiment was designed to minimize IQ differences in the use of verbal mediation as mentioned earlier. This might have made the task too easy to reveal any inferiority in attention or mediation on the part of the retardates. Whenever differences have been noticed (Luria, 1963; O'Connor & Hermelin, 1963), the retardates were in the imbecile category having IQs lower than the present group. Thus group differences would be seen if we utilize more complex tasks, as has been obtained, for instance, in an experiment on distractibility in the retardates (Sen & Clarke, 1968). However, in increasing task difficulty, one should ascertain that the retardates were performing at the same initial level as the normals before the experimental variable was manipulated. For example, in the Sen & Clarke task, even during non-distraction condition, the low MA group had, by contrast with the high MA group, a considerably inferior capacity to carry out the animal-naming task (p. 58). While such a difference may not be detrimental to achieving the objectives of their study, in our experiment this would confound attentional capacity with pre-experimental learning levels. The lack of group differences suggests that EMRs with appropriate instructions are as efficient in maintaining the inhibiting ORs as normals.

It was stated earlier that our understanding of the OR can be enhanced by studying the processes leading to the extinction of the OR. In the present study we made an attempt to examine adaptation and conditioned inhibition in terms of Sokolov's (1965, 1969) neuronal model. As Figs. 1 and 2 show, there is evidence of an adaptation of ORs over blocks for all groups of subjects. Likewise, immediately following the reversal instructions, there is evidence of an inhibition of ORs to the no-press words. In the present case, however, it is not possible emphatically to state that the processes involved here are adaptation and conditioned inhibition, although a logical analysis of the task supports such a distinction.

This research was supported by the National Research Council, Ottawa, grant APA-292 awarded to J. P. Das. An earlier version of the paper was presented in a symposium: Attention in the Mentally Retarded at the annual meeting of the Canadian Psychological Association, Winnipeg, 1970.

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(*Manuscript received* 24 *October* 1970; *revised manuscript received* 20 *March* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 205–212
Printed in Great Britain

DEGREES OF INSIGHT

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This experiment investigated performance on a reasoning task in which the subjects had to say which of four half-hidden cards they needed to see fully in order to determine the truth or falsity of a conditional sentence. In one experimental condition examples of each of the four types of card were fully displayed to remove any difficulty due to limitation of memory or imagery. No more correct solutions were obtained under this condition than under a control condition in which no such aid was given. After they had made their selections all subjects wrote down their reasons for them. Analysis of the different selections, and the reasons given for them, corroborated a previously proposed information-processing model, postulated to explain the cognitive processes involved in tasks of this type.

The problem is a deceptively simple one first studied by Wason (1968, 1969). It involves selecting partially concealed information in order to determine whether a conditional sentence is true or false. The following (Wason, 1968) is an example. The subjects know that every card has a letter on one side and a number on the other side. They are then shown four cards, each placed so that only the letter, or only the number, is face upwards, e.g. D, K, 3, 7 respectively. The experimenter then makes the following claim about the four cards: *If there is a D on one side of a card, then there is a 3 on its other side.* The subject's task is to say which cards he must see fully in order to determine whether the claim is true or false. The correct answer is 'D' and '7', since if these two stimuli were to occur on the same card the claim would be disproved. The logical form of the claim is that of a conditional sentence: 'if p then q', where p refers to the stimulus mentioned in the antecedent (D); $\bar{p}$, i.e. not p, refers to the stimulus which negates it (K); q refers to the stimulus mentioned in the consequent (3) and $\bar{q}$, i.e. not q, refers to the stimulus which negates it (7). Thus the correct selection consists of the two cards displaying stimuli equivalent to the logical values p and $\bar{q}$.

Previous experiments, summarized by Johnson-Laird & Wason (1970*b*), have yielded the surprising result that, despite its apparent simplicity, this task is extremely difficult and very few intelligent subjects are capable of attaining the correct answer unaided. This result has been found to hold over a considerable range of variants of the basic problem. The difficulty is evidently very deep and is not easily removed by procedures aimed at making the subjects examine the problem more fully.

Johnson-Laird & Wason (1970*b*) offer two information-processing models to explain the cognitive processes involved, derived from the results of 'remedial procedures' introduced in order to try to enable the subjects to gain insight into the problem. Their 'revised model' postulates that increasing insight into the task is correlated with increasing awareness that falsification is more relevant than verifi-

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cation. 'No insight' is assumed to be associated with the selection of cards which could only verify, and 'complete insight' with cards which could only falsify, the rule. The two main errors, the selection of q and the omission of $\bar{q}$, are given a different conceptual status. In earlier experiments, when subjects were allowed to change their choice (Wason, 1969; Wason & Johnson-Laird, 1970), there was a frequent transition from the initial selection of just p to p, q and $\bar{q}$, but the transition from the initial selection of p and q to just p hardly ever occurred. This seems to suggest a simultaneous gain of one insight (selection of $\bar{q}$) and loss of another (selection of q), which is psychologically most implausible. Hence it is assumed in the model that the omission of q has two independent sources: (*a*) the result of a superficial interpretation that the rule is 'asymmetric' (i.e. converse not assumed), and (*b*) the result of the non-trivial insight that, although q could verify, it could not falsify. This justifies the definition of 'partial insight' as synonymous with the selection of p, q and $\bar{q}$: the subject selects all cards which could verify, or falsify, the rule.

The difficulty in the task comes from the need to overcome a very strong 'verification set', or tendency to seek only confirming evidence. The two levels of insight are successive stages in the escape from this maladaptive strategy. A similar 'verification set' was found in the attempts to solve an inductive reasoning problem (Wason, 1960).

Recently, Wason & Shapiro (1971) found that if the problem was presented in 'thematic' terms, i.e. when the rule referred to real life events, e.g. *Every time I go to Manchester I travel by car*, and the cards (each representing a journey) had the name of a town on one side and a mode of transport on the other side, it became markedly easier to solve, compared with presentation in terms of abstract, unconnected material, i.e. when the rule referred to letters and numbers on either side of the cards. They suggested that this result may be due to the activation of appropriate cognitive structures, or schemata, by the thematic material. They also suggested that the thematic material may form a more coherent, or unified whole, and hence allow the subjects to distribute their attention more readily over the components of the task, rather than fixating on items mentioned in the claim. The use of thematic material also avoids reference to the sides of the cards. It has always been possible that some of the difficulty of the problem may be due to confusion in construing the 'other side of the card' as being the side which could be face upwards *or* face downwards. This was avoided in another experiment (Wason & Johnson-Laird, 1970), but only by using a complex set of masks to conceal different parts of the card. In the present experiment the problem is greatly simplified by having both stimuli on the same side of the card, and using a simple cover to conceal one of its halves. Thus, instead of 'the other side of the card', reference is made in the claim to 'the other half of the card'.

The present investigation had three aims. First, it intended to establish whether a set of each of the four possible types of card, fully visible throughout the experiment, would facilitate the correct solution. If the difficulty of the problem (with abstract material) is due to the difficulty of attending to the different components, in order to manipulate them symbolically, then this aid to memory and imagery would be of considerable help. However, if the difficulty is due to the failure of abstract material to induce the *idea* of a combinatorial analysis for the solution, then the exposed cards

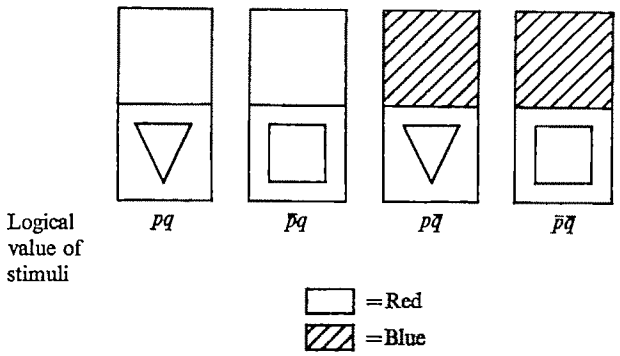

Fig. 1. The four types of card.

would be of little help. The second aim was to find out what reasons the subjects would give for their selections, if they were left to write these down without assistance or interference from the experimenter. This would give an important test of the Johnson-Laird & Wason model, since it would be based on the evidence of spontaneously produced explanations. The third aim was to see whether the task of justifying their selections would lead the subjects, through a fuller examination of the problem, to correct their own errors and gain spontaneous insight.

Method

Design

Two independent groups were used. The experimental group performed the task with a set of the four possible types of card fully visible throughout the experiment. The control group did not have this extra set of exposed cards. In all other respects their treatment was the same.

Subjects

Thirty-two unpaid student volunteers from Nottingham University were allocated alternately to the groups and tested individually. They had no previous experience with tasks of this type.

Materials

A total of 24 (5 × 3 in.) cards were used, consisting of six sets of the four types of card illustrated in Fig. 1, i.e. triangle or square on one half, and red or blue on the other half.

Procedure

In the experimental group one card of each type was placed on the table and remained fully visible throughout the experiment. All the subjects (in both groups) were given a pack of 20 cards, consisting of five examples of each of the four types. They were instructed to inspect the pack and confirm that each card had a triangle or a square on one half, and was coloured red or blue on the other half. The subjects in the experimental group were further instructed to verify that each card was identical to one of the four cards placed on the table.

When the subjects had examined the cards, the experimenter selected four from the pack of 20 (without allowing the subjects to see which ones), and arranged them on the table with one half of each card completely hidden by a sheet of cardboard, and the visible halves displaying 'triangle', 'square', 'red' and 'blue' respectively.

The subjects were then instructed as follows: 'I have taken four cards from the pack which you

have just examined and arranged them with one half under this cover. I am now making the following claim about these four cards: *Every card which has a triangle on one half has red on the other half.*' (A typed copy of this sentence was then handed to the subjects.)

The instruction continued: 'I want you to tell me those cards, and only those cards, that you must see fully in order to find out if my claim is true or false. This claim refers to these four cards, and only to these four cards.'

The instructions ended with a warning intended to make the subjects consider the problem carefully before responding: 'The problem is not quite so easy as it looks, so please think carefully before giving your answer as you may not change your mind.'

When they had made their selections, the subjects were given paper and pencil and told to write down their reasons for choosing to examine, or to ignore, *each* card. (The correct solution is, of course, 'triangle and blue'.)

Finally, the experimenter asked if they were still satisfied with their choices, and if they wished to give any introspective report. The subjects performed at their own pace, and no attempt was made to hurry them at any stage in the experiment, which lasted an average of 6 min.

RESULTS

Quantitative results

Table 1 shows the frequencies with which the cards were selected before the subjects had written down their reasons. Under 'others' all the selections either included $\bar{p}$, or omitted p. Previous experiments suggested that such selections are rare, and appeared to result from guessing. It will be noted that the number of correct solutions is the same in both groups, indicating that the presence of the fully exposed cards in the experimental group did not facilitate insight.

Three subjects in the experimental group and two in the control group, after giving initial incorrect solutions, spontaneously gained complete insight, changing their selections to p and $\bar{q}$, during, or immediately after, giving their reasons for their initial selections. In two cases this occurred after a selection of p, q and $\bar{q}$, and in one case after each of the following selections: p and q; p; q.

One subject in the experimental group, after initially making the correct selection, then lost insight and changed to a selection of p and q. Another subject, from the control group, changed from his initial selection of p, q and $\bar{q}$ (partial insight) to p and q. No other spontaneous alterations occurred.

Qualitative results

Since there were no apparent differences between the groups in the reasons given for particular selections, the protocols from both groups will be considered together. They may be taken as typical of their respective types. With the exceptions noted, none of the quoted protocols deviated in any significant respect from the other protocols.

Table 1. *Frequency of logical values selected in the two groups*

	$p,\bar{q}$	$p,q,\bar{q}$	p,q	p	Others
Experimental group ($n = 16$)	3	5	6	1	1
Control group ($n = 16$)	3	1	5	2	5
Total ($n = 32$)	6	6	11	3	6

1. *Selection: p and q*

Experimental group: male law student.

'The problem only refers to red and triangle. Therefore square and blue may be ignored.'

p: 'Triangle: only interested if it has red on.'

$\bar{p}$: 'It is irrelevant if the square has red or blue on it.'

q: 'Red: want to know if it has triangle on it.'

$\bar{q}$: 'It is irrelevant if blue has a triangle on it.'

The explanations for this selection all show a strong set for verification which leads to the selection of only those stimuli mentioned in the claim. It is evident that this subject does not entertain the idea of falsification, even with respect to the triangle (p). This total lack of insight is, however, sometimes revealed with an evident awareness of falsification, which leaves the selection unaffected.

Experimental group: male mathematics and philosophy student.

p and q: 'Examine the triangle and the red, because if these have blue or square on the other half, the claim is false.'

$\bar{p}$ and $\bar{q}$: 'Ignore the other two because their only relevance would be to a converse of the proposition.'

It will be noted that although blue ($\bar{q}$) and square ($\bar{p}$), on the other half of p and q respectively, are acknowledged to disprove the claim, they are not selected. This is a classic case of the lack of 'reversibility', so frequently noted in previous experiments. But in the present example the issue is more complex because he is aware of what would make the claim false. In addition, he has evidently confused the converse (if q, then p) and the inverse (if $\bar{p}$, then $\bar{q}$) of the proposition. However, his reasons are consistent with the acceptance of the converse ('square on the other half of red would falsify the claim') which he subsequently disavows.

2. *Selection: p*

The explanations for this selection reveal a comparable lack of insight, and a verification set, together with an 'asymmetric' interpretation of the sentence, i.e. appreciation that the converse is not assumed.

Experimental group: male student of economics.

p: 'The triangle card must be looked at to see if the statement is true.'

$\bar{p}$: 'The square is obviously discounted because it cannot have a triangle on it.'

q: 'The red card need not be looked at as the statement only says that every card with a triangle on one half has red on the other half, not that every red card has a triangle.'

$\bar{q}$: 'The blue card is discounted because the statement only applies to red cards.'

3. *Selection: q*

The verification set on two occasions led to a selection of q alone. This rare selection evidently arises when the set for truth extends to a presupposition of the truth of the claim to be tested.

Control group: male student of civil engineering.

p: 'The card with the triangle on it – rejected becauseI accepted the statement and assumed that it would be red on its other half.'

$\bar{p}$: 'Rejected because of the rectangle on it.'

q: 'The red card. The other side could be a triangle or a rectangle. You will have to see it to make sure which one.'

$\bar{q}$: 'Blue card rejected because it conflicts with the statement.'

4. *Selection: p, q and $\bar{q}$*

These subjects are assumed to be in a state of 'partial insight'. They are aware of the need to look for possible disconfirming evidence (and hence select $\bar{q}$), but also retain a verification set which leads them to include q. The protocols reflect the corresponding cognitive processes assumed by Johnson-Laird & Wason to be associated with this selection.

Experimental group: male student of English.

p and q: 'The experiment mentions triangle and red, therefore we must look at both cards which show one of these features to see if its other half has the second feature required if the statement is true.'

$\bar{p}$: 'The experiment does not mention a square, so the card with a square on it need not be looked at.'

$\bar{q}$: 'We must also look at the blue card in case it has a triangle on its other half, in which case the statement woud be untrue.'

5. *Selection: p and $\bar{q}$*

Four of the six subjects making the correct selection mentioned the need to seek only possible falsifying evidence. As predicted by the model, in these cases the state of complete insight was associated with an absence of the need to seek verifying evidence.

Control group: male student of economics.

p: 'I chose the triangle because if it has blue on the other half, it will disprove the claim.'

$\bar{p}$: 'I ignored the square because it has nothing to do with the claim.'

q: 'I ignored the red card because it may have a triangle or a square on the other half.'

$\bar{q}$: 'I chose the blue card because if it has a triangle on the other half, it will disprove the claim.'

One of the other two subjects stated that he chose 'triangle' (p) on the grounds that it could either verify or falsify the claim. Thus his reasons were inconsistent with those postulated by the model for complete insight. However, significantly enough, this subject then apparently lost complete insight, first of all regressing to partial insight (selection of p, q and $\bar{q}$), and then to no insight (selection of p and q). Thus the initial inconsistency between his selection and his reasons, in terms of the model, together with the subsequent alterations in his selection, strongly suggests that his initial apparent complete insight was tenuous.

The protocol of the remaining subject gave the only evidence in the whole experiment which was inconsistent with the model since his reasons exhibited verifying as well as falsifying processes.

6. *Others*

One example of the protocols of the two subjects who chose only q has already been discussed. No further protocols in this category will be presented because most of them are infected with serious confusion and an apparent misunderstanding of the problem. They yield evidence neither for, nor against, the arguments we have advanced.

Discussion

The proportion of correct solutions in the present experiment is greater than that obtained in previous experiments, and this suggests that our presentation of the problem in terms of ***halves*** of the card, as opposed to ***sides*** of the card, allows subjects to overcome initial difficulties in considering the problem. However, the results show that the provision of fully exposed examples of each possible type of card did not facilitate the attainment of complete insight in the solution to the problem.

It seems to follow that we may reject any theory which suggests that the difficulty of the task is due to the ***inability*** of subjects to perform a combinatorial analysis on the stimuli. On the contrary, the protocols suggest that when the subjects do decide to perform such an analysis they do it efficiently, whether or not the fully exposed cards are present. This argues against the hypothesis of Wason & Shapiro (1971) that it is the lack of unity in the abstract material, and the difficulty of examining different aspects of it, which makes the problem so hard. Their other hypothesis, that such material fails to activate appropriate cognitive structures, gains some support from both the quantitative and the qualitative results. It is not that the subjects cannot perform a combinatorial analysis with abstract material, but that the idea of doing so seldom occurs to them.

There are, however, differences between the groups in the relative frequencies with which incorrect selections occur. It will be seen from Table 1 that five subjects in the experimental group attain a solution consistent with partial insight (p, q and $\bar{q}$), compared with only one in the control group. This suggests a trend, which is of course short of statistical significance. The fully exposed cards may have been helpful in some respects, despite introspective comments to the contrary. This is perhaps supported by the fact that five subjects in the control group produced selections classified under 'others', which we have suggested indicates confusion and misinterpretation, compared with only one in the experimental group.

One novel feature in the present results is the selection of only $\bar{q}$ (two cases). The respective protocols show that the verification set is so strong that it extends to assuming the truth of the claim to be tested, and hence precludes the selection of p. In a previous experiment (Wason, 1969) this presupposition was sometimes revealed, but always in conjunction with the selection of p, i.e. the subjects stated that q 'must be' on the other side of p. In the present experiment these two subjects appear to be more rational because, if they assume q is associated with p there is no reason to select p. This selection, of course, goes beyond the predictions of the model which assumes that the truth of the claim is not presupposed.

The elicitation of written justifications for the selections had only a limited

effectiveness in inducing greater insight. Only five out of 26 subjects attained the correct solution after trying to give reasons for their incorrect selections, and two of them had already attained partial insight. On the other hand, two subjects regressed, from partial insight and from complete insight respectively, to a total lack of insight, revealed by changing their selections to p and q. This suggests that the levels of insight in the problem may be more labile than were originally supposed. It accords well with some unpublished observations by the second author that the understanding of the solution appears to 'fluctuate' in some subjects, when it is explained to them, and with some results (Johnson-Laird & Wason, 1970*a*), which show that the degree of insight may vary as a function of the cognitive load imposed.

In summary, the present results corroborate empirically the existence of the three levels of insight postulated by the information-processing model devised to explain performance in tasks of this type (Johnson-Laird & Wason, 1970*b*). There is a close relation between the degrees of insight indicated by the protocols and the corresponding selections of the cards. This support is all the more impressive because the explanations in the protocols were offered spontaneously by the subjects, and hence were unaffected by any possible experimenter induced bias.

Grateful acknowledgements are due to Dr P. N. Johnson-Laird for a critical reading of this paper, and to Mrs Diana Shapiro for technical assistance.

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(*Manuscript received* 4 *December* 1970; *revised manuscript received* 26 *May* 1971)

Br. J. Psychol. (1972), **63**, 2, *pp.* 213–219
Printed in Great Britain

REASONING WITH NEGATIVES

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Two experiments are reported on the effect of negation on reasoning with conditional rules. In Expt. I significantly more affirmative than negative statements were denied by a valid inference (*modus tollens*), and significantly more negative than affirmative statements were affirmed by a fallacious inference (*affirmation of the consequent*). Subjects made their inferences by choosing from a list of possible conclusions, so that, for example, an affirmative statement, *p*, could be denied by selecting the negative conclusion, *not p*. It was hypothesized that the greater difficulty experienced by subjects in the denial of *not p* by a choice of *p* might be due to the additional intermediate step of double negation, i.e. *not not p*, which is involved. The results of Expt. II suggested, however, that the difficulty lies in subjects' inability to infer that a negative statement is false, rather than in the process of transforming a double negative into an affirmative.

The present study is concerned principally with the effect of negation on *modus tollens* (MT) reasoning. A concrete example of MT is

If the sun is shining then it is daytime	(*If p then q*)
It is not daytime	(*not q*)
(Therefore) The sun is not shining	(*not p*)

The rule of MT allows the conclusion below the line to be inferred from the two premises above. This particular deductive inference is specific to conditional rules: the falsity of the consequent clause implies the falsity of the antecedent. Clearly, the introduction of negatives in no way affects the logic of the inference. For example:

If it is not Sunday then the shops are open	(*If not p then q*)
The shops are not open	(*not q*)
(Therefore) It is Sunday	(*p*)

The first hypothesis tested in this study was that the valid denial of a negative antecedent, *not p*, resulting in the affirmative conclusion, *p*, would be more difficult than the valid denial of the affirmative antecedent, *p*, resulting in the negative conclusion, *not p*. The second hypothesis was that when a negative consequent, *not q*, was falsified by an affirmative statement, *q*, it would be more difficult to make the MT inference than when an affirmative consequent, *q*, was falsified by a negative statement, *not q*.

Previous research has shown that negation causes difficulty over a wide variety of cognitive tasks (Wason, 1959, 1961; Hovland & Weiss, 1953; Donaldson, 1959; Wallach, 1959; Jones, 1966). Of more specific relevance to the hypotheses tested here, however, is the work of Wason (1965) and Greene (1970), who claim that the 'natural' semantic function of a negative is to contradict a possible misconception or to 'signal a change in meaning'. The first hypothesis stated above is concerned with an active denial made by the subject, and the second with the perception of a

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contradiction in the structure of the problem. Both were made on the assumption that a negative is 'naturally' used to indicate the falsity of a prior affirmative, whereas an affirmative is normally used for assertion, implying nothing about a prior negative.

Experiment I

The subjects were required to make inferences about conditional rules where the presence or absence of a negative was systematically varied in each component. For each type of rule they were given an MT problem where the additional premise falsified the consequent (Table 1, column (*a*)). In addition, each type of rule was given with a premise which affirmed the consequent (Table 1, column (*b*)). The latter set of problems, introduced to vary the subject's task, incidentally allows a test of the subject's tendency to make the fallacious inference known as the ***affirmation of the consequent*** (AC), by which the truth of the consequent is taken to imply the truth of the antecedent. For example, given (i) *If p then q*, and (ii) *q*, one might fallaciously infer *p*. No hypotheses were made concerning the effect of negation on the tendency to make AC inferences.

Table 1. *The eight logical types of problem used in Expt. I, together with the conclusions which might be drawn in each case*

	(*a*) *Modus tollens*		(*b*) Affirmation of the consequent	
Rule	Additional premise	Conclusion (valid)	Additional premise	Conclusion (fallacious)
1. If *p* then *q*	Not *q*	Not *p*	*q*	*p*
2. If *p* then not *q*	*q*	Not *p*	Not *q*	*p*
3. If not *p* then *q*	Not *q*	*p*	*q*	Not *p*
4. If not *p* then not *q*	*q*	*p*	Not *q*	Not *p*

Method

Design

The subjects were tested on each of the eight types of problem twice. The order of presentation of the eight problems was randomized, and then each subject received a second block with the eight types of problems in exactly the opposite order to that in which they appeared in the first block.

Subjects

Sixteen undergraduate students of University College London served as subjects on a paid volunteer basis, and were tested individually.

Task and materials

Subjects were told that they would be required to make inferences from rules which defined which capital letters were allowed to go with which digits in imaginary letter–number pairs. The subjects were given, on each problem, a conditional rule with an additional premise and asked to choose one from a list of three possible conclusions. The following example of an MT problem will serve to illustrate the materials:

Given: (1) If the letter is not G, then the number is 9, (2) Not 9.

Conclusion: G, not G, indeterminate.

The correct answer here would be 'G'. If the second premise were '9' then the correct conclusion would be 'Indeterminate'; a choice of 'Not G' would indicate susceptibility to the AC fallacy.

The letters and numbers used were varied as much as possible between the problems. In order to simplify the subject's task the order of premises (conditional rule, extra premise) and the order of possible conclusions (affirmative, negative, and 'indeterminate') were kept constant throughout.

Procedure

The subject was given a pretest in which he was given a rule of the form, *If p then q*, and asked what would follow given (i) *q*, and (ii) *not q*. As the pretest was intended for practice and familiarization with the task, subjects were told whether or not they were correct. No such feedback was given on the test problems which were presented in a booklet, with each problem on a separate page. The time taken to reach a conclusion was measured with a stopwatch.

Results

On the pretest only three of the 16 subjects made the fallacious AC inference, whereas 14 of the 16 made the valid MT inference.

Although there were always three choices of conclusion open to the subject on each test problem, subjects' responses indicated that each problem involved effectively only a binary choice. Thus on MT problems subjects invariably made either the correct choice of conclusion or else chose 'indeterminate'. Similarly on the AC problems subjects either chose the 'appropriate' fallacious conclusion, or else (correctly) chose 'indeterminate'.

The percentages of valid MT and fallacious AC inferences made on each type of problem appear in Table 2. It is evident that, as predicted, negating the antecedent of the conditional premise renders the MT inference more difficult ($P < 0{\cdot}001$, one-tailed, Wilcoxon matched-pairs signed-ranks test). Contrary to expectations, however, negating the consequent had no significant effect.

Table 2. *The percentage* of valid MT and fallacious AC inferences made on each type of rule in Expt. I*

Rule	(*a*) *Modus tollens*	(*b*) Affirmation of the consequent
1. If *p* then *q*	91	32
2. If *p* then not *q*	75	35
3. If not *p* then *q*	38	61
4. If not *p* then not *q*	41	55

* Each percentage is based on two responses from each of 16 subjects.

A significantly larger number of fallacious AC inferences were made when the antecedent was negated ($P < 0{\cdot}01$, Wilcoxon test) but there was again no effect due to negating the consequent.

The analysis of response times is not included as comparisons were confounded by the large differences in errors between conditions. The trends observed, however, did suggest that response times were positively correlated with errors.

Experiment II

The second experiment was designed to test an interpretation* of the main finding of Expt. I. When the subject is denying an affirmative antecedent, *p*, the conclusion, *not p*, follows directly. However, when he is denying a negative

* Suggested by Dr P. N. Johnson-Laird.

antecedent, *not p*, an extra step of 'double negation' would seem to be involved in concluding *p*. The subject must presumably reason, 'not not *p*, therefore *p*'. There may be an inability to see that a double negative is equivalent to an affirmative, which is responsible for the lower frequency of MT inferences made on these problems.

In order to test this hypothesis the task was modified so that instead of selecting from a list of conclusions, the subject selected from a list of evaluations (of a single statement): 'true', 'false', and 'indeterminate'. The antecedent of the conditional premise was either affirmative or negative, but the consequent was always affirmative (since in Expt. I negating it had had no effect). The sentence to be evaluated was either the same as the antecedent or its logical opposite.

Table 3. *The eight logical types of problem used in Expt. II together with the conclusions which might be drawn in each case**

(*s* = same as antecedent; *o* = opposite of antecedent.)

Rule	Evaluate	(*a*) *Modus tollens* Conclusion (valid)	(*b*) Affirmation of the consequent Conclusion (fallacious)
1. If *p* then *q*	*p* (*s*)	F	T
2. If *p* then *q*	Not *p* (*o*)	T	F
3. If not *p* then *q*	Not *p* (*s*)	F	T
4. If not *p* then *q*	*p* (*o*)	T	F

* The appropriate additional premises for the MT and AC problems, which may be read from Table 1, have been omitted in this table.

On the MT problems (Table 3, column (*a*)) the antecedent could be denied either by evaluating the 'same' statement as 'false' or the 'opposite' statement as 'true'. Now in the case of a negative antecedent, *not p*, evaluating its opposite, *p*, as 'true' would seem to be equivalent to selecting the affirmative conclusion in Expt. I, and involves conversion of a double negative into an affirmative. To evaluate the 'same' statement, *not p*, as 'false', however, is a direct denial which in no way requires the subject to convert a double negative. If 'double negation' is the main cause of difficulty in denying the negative, then the evaluation of the 'opposite' statement as 'true' should be harder than the evaluation of the 'same' statement as 'false'.

It is possible that evaluating the opposite of a statement may cause other difficulties. These should, however, be reflected in a comparison of 'same' and 'opposite' evaluations of an affirmative antecedent. Hence, inclusion of affirmative antecedent problems was a necessary control. Three hypotheses were formulated:

(i) More MT inferences would be made with affirmative than with negative antecedents;

(ii) More MT inferences would be made with 'same' than with 'opposite' evaluations, and

(iii) This reduction in the number of MT inferences on 'opposite' (as opposed to 'same') evaluations would be greater on problems where the antecedent was negated (owing to the extra difficulty of 'double negation').

A set of equivalent AC problems (Table 3, column (*b*)) was again included in order to vary the task. It was hoped to replicate the previous unexpected finding, that more AC inferences were made with negative than affirmative antecedents.

Method

Design

Each subject was given three blocks of problems, each containing each of the eight types of problem once only. The order of presentation was counterbalanced using three 8×8 Latin squares for the first eight subjects, and a further three Latin squares for the next (and final) eight subjects.

Subjects

Sixteen subjects on a Postgraduate Certificate of Education course at the Institute of Education in the University of London served as subjects on a paid volunteer basis, and were tested individually.

Task and materials

The conditional rules presented to the subjects were identical in form to those used in Expt. I except that negatives were introduced only into the antecedent part of the rule. The following example of an AC problem, with an 'opposite' evaluation, will illustrate the materials:

Given: (1) If the letter is R, then the number is 1.

(2) The number is 1.

Evaluate: The letter is not R.

True, false, indeterminate.

The subject's task was to choose the evaluation which was logically implied by the premises given. The correct choice here would be 'Indeterminate'. A fallacious inference would be indicated by a choice of 'False'.

Procedure

The subject was introduced to the problems in a similar manner to that used in Expt. I, except that no pretest was given. Instead the subject was shown examples of the two logical types of rule, with and without the negative antecedent. In all other respects the procedure was similar to that used in Expt. I.

Results

The subjects' choices again proved to be effectively binary, as in Expt. I. The percentages of valid MT and fallacious AC inferences made on each type of problem are presented in Table 4.

Table 4. *The percentage* of valid MT and fallacious AC inferences made on each type of problem in Expt. II*

Rule	Evaluate	(*a*) *Modus tollens*	(*b*) Affirmation of the consequent
1. If p then q	p (*s*)	98	90
2. If p then q	Not p (*o*)	71	94
3. If not p then q	Not p (*s*)	40	100
4. If not p then q	p (*o*)	6	100

* Each percentage is based on three responses from each of 16 subjects.

The results showed firstly that there were more MT inferences made on affirmative than on negative antecedent rules ($P = 0{\cdot}000015$, one-tailed sign test), and, secondly, that there were more MT inferences on 'same' than on 'opposite' evaluations ($P = 0{\cdot}00012$, one-tailed sign test). Contrary to expectations, however, the difference in frequency of MT inferences between 'same' and 'opposite' evaluations was no greater on the negative than on the affirmative antecedent problems (a comparison

of the two differences with a Wilcoxon test detected no significant trend). An interaction of the kind predicted might, however, have been suppressed by a 'floor' effect in the data (Table 4, problem 4(*a*)), though equally an interaction in the opposite direction might have been suppressed by a 'ceiling' effect (Table 4, problem 1(*a*)). There is, then, no statistical evidence that extra difficulty, due to 'double negation', is present in the 'opposite' evaluation of a negative antecedent, but the possibility of the existence of such difficulty cannot be ruled out.

Any effects which negation might have exerted on the subject's reasoning with the AC problems (Table 4, column (*b*)) would also have been suppressed by a 'ceiling' effect on all conditions, i.e. most subjects made the maximum number of fallacious inferences on all the problems.

The analysis of response times has been omitted for the reasons given when reporting the results of Expt. I.

Discussion

In Expt. I it was found that the MT inference was very much easier when the antecedent of the conditional was affirmative, compared with when it was negative. Contrary to expectations, however, no extra difficulty was caused by negating the consequent of the conditional. The latter prediction was made on the assumption that the subject would have to consider, in these cases, that a negative statement was being denied by an affirmative. If, however, the subject reasons that a *contradiction* would be inconsistent with the truth of the antecedent, and that therefore the antecedent must be false, the order of acquiring the components of the contradiction would be irrelevant. The author has found that logically naive subjects will use this kind of *reductio ad absurdum* reasoning on a more complex logical task.

The lower frequency of valid MT inferences made on negative antecedent problems, observed in Expt. I, might still have been thought due to the difficulty of denying a negative antecedent, *not p*, with an affirmative conclusion, *p*, especially since this appears to require the subject to recognize that a double negative is equivalent to an affirmative. In natural language a double negative frequently does *not* imply an affirmative. For example, to be 'not bad' is not necessarily to be 'good'. A tendency for subjects to see *not p* as the converse of *p* without seeing *p* as the converse of *not p*, would therefore be quite understandable.

The results of Expt. II, however, do not support this explanation. From Table 4 we observe that 98 and 40 per cent correct MT inferences were made on 'same' evaluations of affirmative and negative antecedent rules respectively, where the subject needed only to say, in effect, that the antecedent was false. The difficulty of denying a negative appears to lie in the subject's inability or unwillingness to infer that a negative statement is false, rather than in the process of converting a double negative into an affirmative. Before considering possible reasons for this apparent lack of insight, a few comments should be made about the incidental findings on the AC problems.

In Expt. I there were far more fallacious inferences made on rules which had negative rather than affirmative antecedents. This might be due to a greater tendency for subjects to interpret a conditional rule with a negative antecedent as expressing equivalence (*If and only if not p then q*) rather than implication: an interpretation

which would make the AC inference valid. The author has recently obtained evidence from a quite different experimental situation which suggests that the presence of a negative antecedent may effect such a difference in interpretation.

The fact that in Expt. II a large number of fallacious AC inferences were made on all types of problem probably reflects the difference in the nature of the task. That the two tasks were not equivalent is illustrated by a comparison of the findings of the two experiments on the MT problems. It was assumed that evaluating the opposite of the antecedent as 'true' in Expt. II would be equivalent to selecting the correct conclusion in Expt. I. In fact, comparing the frequencies of correct MT inferences on the rules *If p then q* and *If not p then q*, in the first experiment these were 91 and 38 per cent respectively, while the corresponding figures on 'opposite' evaluations in the second experiment were 71 and 6 per cent respectively. The second task is clearly more difficult.

Returning to the observation that subjects appear to experience difficulty in inferring that a negative is false (on the MT problems), we should remember that negation is normally used to express falsity. How, a subject might think, can one start from an assumption of falsity and prove it more false, as it were, as a result of contradiction? Is one to be surprised at contradictions when one starts off assuming things to be false? And how, as required on some problems, could all these falsities and contradictions lead one to a true conclusion?

The research reported in this paper forms part of the work for a thesis to be submitted for the Ph.D. degree in the University of London. The help and encouragement of Dr P. C. Wason, who supervised the research, are gratefully acknowledged.

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(*Manuscript received* 17 *October* 1970; *revised manuscript received* 26 *March* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 221–228
Printed in Great Britain

BERLYNE'S DEMONSTRATION OF EPISTEMIC CURIOSITY: AN EXPERIMENTAL RE-EVALUATION

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Berlyne demonstrated the existence of epistemic curiosity by showing that subjects given a questionnaire and then a set of statements including answers to the questionnaire recalled more of these answers than subjects not given the questionnaire. Methodological limitations of this experiment suggested that it should be replicated, and certain extensions of the design were also made. The three experiments reported here confirm Berlyne's findings with improved methods and also show that recall was not influenced by the completion of an irrelevant questionnaire but was facilitated to an uncertain and minor degree by completion of a questionnaire containing questions related to but not identical with the items on the recall test. The present results support Berlyne's concept of epistemic curiosity, but they suggest that it is relatively specific to the content of the questions used to arouse it.

The experiments reported in this paper were designed to investigate the drive known as epistemic curiosity (Berlyne, 1954*a*). For reasons indicated below, we have thought it worthwhile to repeat Berlyne's (1954*b*) basic demonstration of epistemic curiosity in its essentials, because of the opinion expressed elsewhere (Cofer & Appley, 1964, pp. 298–9) that the postulation of epistemic curiosity may not be necessary to the explanation of the results Berlyne obtained. Further, we wished to determine the range of effects of this drive, given that its postulation is necessary, beyond its scope as demonstrated by Berlyne (1954*b*).

Berlyne (1954*a*) has suggested that epistemic curiosity 'is reduced by the reception and subsequent rehearsal of knowledge . . .' (p. 180). The drive has two sources. One is the question, or thematic probe (Skinner, 1953), which functions as a 'motivational stimulus' in the evocation of 'a learned drive state' (p. 182); the drive is reduced by the occurrence of the answer to the question, and thus the association between question and answer is reinforced. Even when the occurrence of the answer is delayed, the association can be reinforced, because later occurrence of the answer will cause, through reintegration, an internal rehearsal of the question; when the answer is recognized as the one sought on the earlier occasion, there will be drive reduction and the association between question and answer will be strengthened. The other source of epistemic curiosity is the occurrence of strange, unusual or puzzling stimuli. These stimuli arouse the drive because they conflict with expectations. The reduction of conflict will be reinforcing and thus promote learning (p. 185).

We are concerned here with the first of the experiments Berlyne (1954*b*, 1962) has reported concerning epistemic curiosity, as this is the one usually cited as the reference experiment demonstrating the existence of this drive. In this experiment the subjects were college students,* divided into an experimental and a control group. There were three essential steps in the experimental group: (1) The subject completed

* In one publication, Berlyne (1963, p. 326) refers to the subjects of this study as high school students, but the article itself indicates that they were students at Brooklyn College.

a questionnaire which contained four questions about each of 12 invertebrate animals by checking one of two answers to each question. This step presumably aroused epistemic curiosity, and it was omitted in the control group. (2) The subject read 72 statements, six concerning each animal, the statements being presented in a random order different from that used in the first questionnaire. (3) The subject was given the same questions as those used in the first questionnaire, in a new random order, but was required to write his answers. The control subjects went through steps 2 and 3 only. The experimental subjects averaged 32·41 correct answers in step 3, the control subjects 27·15, a significant difference. As there was no feedback as to correct and incorrect answers in step 2, we presume that answers provided in step 3 were marked correct if they accurately represented any of the statements presented in step 2.*†

Berlyne interpreted the difference obtained in the following way. Reading an answer in step 2 would cause the subjects of the experimental group to recall the question they had read previously. Curiosity would then be rearoused, and rehearsal of the answer would reduce it, thus reinforcing the association between question and answer.

Another interpretation of the result, however, is possible. The subjects in the experimental group had more exposure to the items than the control group. Berlyne's design confounds exposure and the arousal of epistemic curiosity and, in addition, there were other differences in exposure in the experimental and control groups (see second footnote below). Hence it has seemed wise to repeat the experiment in an attempt to reduce the confounding and to eliminate the other sources of differences in exposure. We also wished to determine whether the effects of aroused epistemic curiosity would extend beyond the information pertinent to the specific, arousing questions.

Table 1. *Design of Expts. I–III*

Condition	Questionnaire	Statements	Recall tests
Same Questions A	A	Given	A
Related Questions B	A	Given	B
Same Questions B	B	Given	B
Related Questions A	B	Given	A
Irrelevant Questions A	C	Given	A
Irrelevant Questions B	C	Given	B
Control A	—	Given	A
Control B	—	Given	B

* This is our interpretation and could be incorrect. If so, however, there was either some feedback unspecified in the published report or a remarkable perspicacity on the part of the subjects which enabled them to detect the correct answers to the extent that they could achieve scores as high as those indicated. At any rate, the present authors counted as correct the statements from the series that subjects produced on the after-questionnaire.

† The subjects were asked to do some things additional to those just described. Subjects in both groups rated the 12 animals for familiarity and read one paragraph about each of two animals. These tasks preceded step 1 in the experimental group and step 2 in the control group. The experimental group subjects, after completing step 1, went back over the questions and marked 12 about which they would like to know more and the ones that surprised them. In step 2 all subjects marked the statements they found surprising, and subjects in the experimental group marked each answer they recognized as an answer to an item in the questionnaire. In step 3 all subjects checked which of the 12 animals they would like to know more about. Recall scores related to the surprising statements and to the ones marked as ones about which more information was desired were better than for the ones not so marked. In addition 30 different subjects were used as judges to check off the two phrases that seemed to them to be least applicable to each animal, and these items were better recalled than others.

To accomplish these purposes, three questionnaires were prepared for step 1. Consultation of Table 1 will help in understanding the design and the names of groups. Questionnaires A and B concerned the same set of animals but the questions were different. Questionnaire C concerned a different content altogether, its questions being concerned with Indian tribes. Eighty statements were prepared for step 2, half pertinent to the questions in Questionnaire A and half pertinent to those in Questionnaire B. Recall Tests A and B were used in step 3. In each of these the questions were the same as those in the corresponding questionnaire of step 1 but written answers were required. The ancillary procedures Berlyne used (see second footnote, p. 222) were eliminated. There were two control groups, in which step 1 was omitted; both had the 80 statements, but one took Recall Test A, the other Recall Test B (control B).

From the results obtained with this design, we can examine the results for the following comparisons.

1. A partial replication of Berlyne's experiment is provided by the comparison of scores made on Recall Test A and Recall Test B following the corresponding questionnaires (A and B) with the control groups which took Recall Tests A and B, respectively. The two experimental groups are referred to as Same Questions A and Same Questions B and the control groups as Control A and Control B.

2. The extent to which related questions can arouse epistemic curiosity can be estimated by comparing scores on Recall Test A by the group which had Questionnaire B (Related Questions B) and the scores on Recall Test B by the group that had Questionnaire A (Related Questions A) with the scores made by the control groups A and B, respectively.

3. The extent to which epistemic curiosity carries over to unrelated content can be tested by comparing the scores on Recall Tests A and B by the groups which took Questionnaire C (Irrelevant Questions A and Irrelevant Questions B) with the scores made by Control Groups A and B.

Expt. III is similar to Expts. I and II save that in step 1 no answers were provided to the questions; the subject was asked to write answers. Exposure to answers, therefore, should not differentiate groups in this case.

The general design applicable to all three experiments is shown in Table 1.

Method

Questionnaires

Questionnaires A and B concerned the following exotic animals: peripatus, squilla, sea squirt, Venus's girdle, anolis, tapir, grison, tayra, sea anemone, and cyclops anteater. The familiarity of these animals was probably less than those used by Berlyne. There were four multiple-choice questions about each one, two choices per question. One question, in Questionnaire A, for example, was: 'The favourite food of the tayra is (*a*) honey, (*b*) berries'. Questionnaire B included this question: 'The patch of distinctive colour is found on the tayra's (*a*) throat, (*b*) stomach'. The subject was to check which of the two answers to each question he thought correct even if he had to guess. Questionnaires A and B, then, involve the same animals but ask different questions about each.

Questionnaire C asked four questions, again each with two answers, about each of 10 Indian tribes. One question follows: 'The name Mohawk means (*a*) man eaters, (*b*) first people'. The information on which the questionnaires were based came from Sanderson (1945), Verrill (1940) and Brandon (1961). The 40 questions in each questionnaire were ordered randomly.

Statements

There were 80 statements which were identical for all groups. Forty of the statements presented facts relative to the questions of Questionnaire A and 40 to Questionnaire B. In Expt. I there was only one random ordering of the statements for all subjects, but in Expts. II and III five different random orderings of the statements were employed. Subjects who took Questionnaire C read the same statements as the subjects who took the other questionnaires.

Recall tests

Again there were two tests, designated Recall A and Recall B. The questions were the same as those in the corresponding questionnaires (but in a new random order), but their form required the subject to write his answer to each one.

Questionnaire in Expt. III

Questionnaires A and B were used, but in the form in which the subject wrote his answer to each question rather than choose between two alternatives. Thus the subject was not exposed to the correct answer in taking the questionnaires of step 1. The random order of the questions in these questionnaires was different from those in Recall Tests A and B.

Subjects

All subjects were recruited from sections of the class in introductory psychology at the Pennsylvania State University. Participation in experiments is rewarded by means of credit towards the course grade. The numbers of subjects were: Expt. I, 64 males and 41 females; Expt. II, 84 summer school students, predominantly females; and Expt. III, 131 subjects, about equally divided between males and females. The division of these subjects into subgroups resulted in groups of from 14 to 16 in Expt. I, eight to 12 in Expt. II, and 15 to 17 in Expt. III.

In each experiment the subjects were asked to sign up for participation on any one of four days. On day 4 the control groups, which did not receive step 1, were run. Subjects were run in groups and subjects from all the experimental groups were run simultaneously on each of the first three days. The two control groups were also run simultaneously.

Procedure

On arrival at the testing room the subjects in Expt. I were handed a packet containing a questionnaire and Recall Test and statements for their conditions; these were randomly distributed within the group to be tested. In Expts. II and III the questionnaire, statements and Recall Test were given out one at a time, since it was observed in Expt. I that subjects would look ahead to the statements or Recall Test, despite contrary instructions. Subjects had 6 min. to read the 80 statements, but there was no time limit for the completion of the questionnaire or Recall Test. Instructions for the questionnaire, statements and Recall Test were printed, so that it was unnecessary for the experimenter to give any instructions orally.

Expts. I and II were essentially identical except in three respects. In Expt. I, prior to the administration of the questionnaire, subjects were asked to rate each of the animals for familiarity. A four-point scale was used (with 1 indicating low and 4 indicating high familiarity), and each level was defined by a statement, e.g. '1. Completely unknown to you, first time you have heard of it' (see Berlyne, 1954*b*). As there was no reason to assume that familiarity ratings provided by the subjects of Expts. II and III would differ from these, this procedure was omitted in them. The second difference lay in the use of one randomized order of the statements in Expt. I and five in Expts. II and III. This change was dictated by the results of Expt. I. The third difference was that all materials were given out at once in Expt. I but separately in Expt. II. Expt. III was identical to Expt. II, except for the kind of answer required for the questionnaires of step 1 as already indicated.

Results

The results of the three experiments agree in confirming Berlyne's findings and show that curiosity arousal facilitates to a small and uncertain degree the answering of related questions. Answers to the questions, however, are not facilitated by the irrelevant questionnaire of step 1.

The mean familiarity ratings of the animals obtained in Expt. I were for the most part between 1 and 2, with one mean at 2·00 (cyclops anteater) and one at 2·54 (sea anemone). It is clear that the subjects were unfamiliar with these animals. Further evidence is indicated by the means of 20·86 and 19·89 correct answers for the Questionnaires A and B obtained in Expts. I and II. These values are at chance performance for 40 items with two alternatives each.

The mean scores for the Recall Tests for the eight conditions in each experiment are shown in Table 2. It is necessary to analyse the results of the experiments separately, because of the differing procedures involved in them. Analyses of variance, adjusted for unequal n (Winer, 1962), were employed.

In Expt. I, there were significant results for the Recall Tests ($F = 10{\cdot}526$; d.f. = 1, 97; $P < 0{\cdot}01$), the treatments ($F = 5{\cdot}943$; d.f. = 3, 97; $P < 0{\cdot}01$) and the interaction ($F = 4{\cdot}310$; d.f. = 3, 97; $P < 0{\cdot}01$). Performance on Recall Test A was superior to that on B. The treatment effect arose in the comparisons of condition Same Questions A *v.* condition control A, with means of 25·3 *v.* 16·6, and in the comparisons of condition Related Questions B with condition control B and of condition Related Questions A with condition control A. The means here were, respectively, 19·5 *v.* 14·2 and 19·7 *v.* 16·6. The means for conditions Irrelevant Questions A and B were essentially the same as those for the control groups.

There was no difference between conditions Same Questions B and Control B, and the significant interaction arises because of this fact, together with the large effect found in the comparison of conditions Same Questions A and Control A. The failure of the first of these two comparisons to show a difference can probably be explained by the fact that in the single random-ordering of the statements used in this experiment, most of the answers for Recall Test A were located in the first 40 statements. This could confer an advantage in condition Same Questions A as compared with condition Same Questions B or Related Questions B. The appearance of these items in the first half of the statements, in conjunction with the immediately preceding questions for which they were the answers, must be the critical factor, because other comparisons of Recall Tests A and B show equivalent scores for the groups treated

Table 2. *Mean scores on the recall tests for the eight groups of the three experiments*

Condition	Expt. I	Expt. II	Expt. III
Same Questions A	25·3	19·7	17·0
Related Questions B	19·5	9·0	15·3
Same Questions B	15·5	17·7	17·5
Related Questions A	19·7	12·9	15·6
Irrelevant Questions A	15·6	9·6	13·8
Irrelevant Questions B	15·2	8·8	12·0
Control A	16·6	8·6	12·8
Control B	14·2	8·8	9·1

comparably. Because of this defect in the set of statements, Expt. II was run with five random orders of the statements.

In general, the results of Expt. I replicate Berlyne's result (leaving aside the problematic finding with condition Same Questions B) and indicate that performance is better for the groups with different but related questions in steps 1 and 3 than it is for the control groups, suggesting a moderately general effect of curiosity arousal. The Irrelevant Questionnaire C, however, generates performances on the Recall Tests essentially equivalent to those in the control groups.

The pattern of results in Expt. II is similar to that in Expt. I, except that scores on the Recall Tests are depressed (possibly because in Expt. II summer session subjects were used), that the interaction and the Recall Test effects are no longer significant, and that the comparison of conditions Same Questions B and control B now gives a difference comparable to that for conditions Same Questions A and control A. The means of the first pair are 17·7 and 8·8 and for the second pair are 19·7 and 8·6. The conditions in which the related questionnaire was administered show means of 9·0 and 12·9, somewhat better than those for the control groups (8·6 and 8·8). The means for the groups with Questionnaire C are essentially the same as those for the control groups. For this experiment, the treatment effect yields $F = 15{\cdot}929$ (d.f. = 3, 73; $P < 0{\cdot}01$). The values for the Recall Tests of $F = 2{\cdot}097$ (d.f. = 1, 73) and for the interaction of $F = 0{\cdot}542$ (d.f. = 3, 73) are not significant. It should be noted that the scores on the Recall Tests differ markedly from those for the questionnaire of step 1 in Expt. III in which the mean number correct was only 2·95.

A similar pattern appears for Expt. III, which replicated Expt. II, except that the subject wrote answers to the questionnaire of step 1 rather than checking one alternative out of two. The treatment effect yields $F = 5{\cdot}718$ (d.f. = 3, 123; $P < 0{\cdot}05 > 0{\cdot}01$), the Recall Tests $F = 2{\cdot}112$ (d.f. = 1, 123) and the interaction $F = 1{\cdot}332$ (d.f. = 1, 123). Neither of the last two values is significant. The results show substantial superiority for conditions Same Questions A and Same Questions B over their controls (A and B) and a relatively small effect for conditions Related Questions B and Related Questions A over the controls. Conditions Irrelevant Questions A and B show means close to those for the controls.

DISCUSSION

It seems to be reasonably clear that Berlyne's results were confirmed, i.e. when the same questions were given in the questionnaires and the recall tests, performance on the latter was typically superior to the performance of the control groups. In the six comparisons that the three experiments permit, the direction of the difference in all was consistent and was substantial in five. Newman–Keuls tests confirmed the significance of these comparisons. The confirmation of Berlyne's result in Expt. III is especially important, as in this case, since the subject wrote his own answers to the items in the questionnaires (and achieved, on average, only 2·95 correct responses), rather than checking one of two alternative answers, there was little opportunity for him to rehearse the correct answer after completing Questionnaire A or B. The groups with the same questions in the questionnaires and recall tests were also superior to the Related Questions conditions in five of six comparisons, there being

one reversal (Same Questions B *v.* Related Questions B) and the differences in Expt. III being small. The groups with the same questions in steps 1 and 3 are again substantially superior, in five out of six comparisons, to the conditions with irrelevant questions, the exception being again for Same Questions B in Expt. I.

The data of the Irrelevant Questions groups show no differences of any size from the control groups in any experiment. Hence, while it may be presumed, on theoretical grounds, that epistemic curiosity was aroused when subjects took Questionnaire C, this motivational arousal did not lead to better learning of answers pertinent to the two recall tests, A and B. This suggests that the arousal of epistemic curiosity can promote learning in the case where specific questions and their answers are juxtaposed, as is the case for the groups whose questionnaires and Recall Tests had the same questions.

It is of interest that there was a consistent though relatively small superiority in the scores of the groups whose questionnaires contained content related to their Recall Tests to those of the control groups. The Newman–Keuls test of this difference was significant in Expts. I and III but not significant in Expt. II. The groups receiving Related Questions A or B were also superior in five out of six comparisons to the groups which received the Irrelevant Questionnaire C, although the differences were small and reached significance by the Newman–Keuls test only in Expt. I. The consistent pattern, however, does suggest a slight effect of a questionnaire on performance on the Recall Test with related content. The effect is unfortunately not a robust one, and hence gives only very limited support to the idea that arousal of curiosity promotes learning of answers in a general content area even when the questions for which the answers are directly pertinent have not yet been asked.

It is possible to interpret this finding as follows. The statements did, of course, provide the answers for Questionnaires A and B, which, given the results for the Same Questions groups, the groups under discussion (Related Questions groups) must have learned. However, the Recall Test did not require these answers but rather another set of answers which the subjects had also studied. They were able to produce a few of the latter, in comparison with the control conditions in each experiment, suggesting that the initial arousal had spread a little beyond the specific answers to the questions in their questionnaires. Had the statements given to the subjects in the Related Questions groups included answers for only one of the two questionnaires (A or B) mixed with irrelevant items (say, for example, answers to questions about Indians), it is quite possible that the performance of these subjects on the related recall test would have been substantially superior to that of the control and the irrelevant questions groups.

The results of this investigation, then, offer confirmation for Berlyne's findings in the case where answers to specific questions are provided between the questionnaires and the Recall Tests. Arousal of curiosity by means of questions does not encourage learning of answers to questions about another topic. There is limited and uncertain support for the idea that arousal of curiosity will encourage learning pertinent to more than one set of questions, so long as the questions relate generally to the content area for which epistemic curiosity is aroused.

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(*Manuscript received* 16 *April* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 229–234
Printed in Great Britain

A METHOD FOR OVERCOMING THE PROBLEM OF CONCEPT–SCALE INTERACTION IN SEMANTIC DIFFERENTIAL RESEARCH*

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Data collected in a study of hospital staff attitudes to drug addicts and other types of patient are used to illustrate the problem of concept–scale interaction in semantic differential research. Results of factor analysis of the four within-concepts correlation matrices diverged sharply from the results of the factor analysis of the across-concepts correlation matrix. Although the first two unrotated orthogonal factors could be identified in the ratings of all concepts, some of the rotated factors did not appear for some concepts. For the purposes of comparing the concepts in terms of the factors, scores were computed for each factor from the across-concepts factor-score estimation weights, but only for those concepts to which the factors applied. It is concluded that this procedure has some advantages over other methods for overcoming the problem of concept–scale interaction which have been proposed in the past.

The problem of concept–scale interaction in semantic differential research was recognized by Osgood himself (Osgood *et al.*, 1957) and has been commented on by other writers, e.g. Gulliksen (1958), Marks (1966), Bannister & Mair (1968) and Heise (1969). But not until Presly's recent article (1969) were the far-reaching psychometric implications of this interaction driven home. Presly pointed out that because correlations between SD scales vary from concept to concept, the averaging of ratings across concepts to obtain the correlation matrix from which the factor-score estimation weights are derived is not justified. Factors scored by this method may have no validity for the individual concepts. In order to ensure factor validity each concept could be factor analysed separately; but this procedure would rule out comparisons *between* concepts in terms of factors, often the main purpose of the study. A way out of the dilemma may be found by carrying out both across-concepts and within-concepts factor analyses. By inspecting the factor loadings it should then be possible to decide empirically which concepts the factors are valid for. We present below an example of the application of this method.

METHOD

Full details of the design of the investigation are given in Romney & Bynner (1972). The research instrument contained four concepts – the hard-drug addict, the alcoholic, the mentally ill patent, the soft-drug addict – and 23 seven-point semantic differential scales on which the concepts were rated. The scales were derived from comments made about drug addicts by hospital staff in exploratory interviews. Two hundred and nine members of the staff of a Canadian hospital (including 64 nurses and 60 doctors) took part in the study.

* The opinions expressed in this article are those of the authors and do not represent the views of the organizations in which they worked.

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Five factor analyses of the ratings were carried out, one of the correlations between ratings averaged across the four concepts and the other four of the correlations between the ratings for each concept separately. For each analysis communalities calculated as the square of the multiple correlation of each scale with all the other scales were inserted in the diagonal of the correlation matrix, and principal components analysis was then carried out followed by Varimax rotation to orthogonal simple structure. The criterion adopted for the number of factors to be rotated in the across-concepts analysis was that their variance should approximate as closely as possible to the total communality (Harman, 1967, p. 168). For the within-concepts analysis the same number of factors was rotated as in the across-concepts analysis.

RESULTS

Table 1 shows the rotated and unrotated factor loadings for the factors which were obtained from the across-concepts factor analysis. The scales have been rearranged so that scales with the highest rotated factor loadings on a particular factor are next to each other. All loadings above 0·3 are in italics. The unrotated factor loadings are presented as well as the rotated ones, because an unrotated factor solution was used in the original work with the semantic differential (Osgood *et al.*, 1957).

It can be seen that all five factors have at least three scales with high loadings on them, and that the factors are clearly interpretable in terms of the content of these scales. On the other hand, only the first two of the unrotated factors are interpretable, as the third factor has only one loading above 0·3. The five rotated factors were labelled: Assertiveness, Dangerousness, Nonconformity, Attractiveness and Educability, and the first two unrotated factors were labelled Extraversion and Psychopathy.

Although no attempt was made in this study to replicate Osgood's primary dimensions of semantic differential data, Evaluation, Potency and Activity, some resemblance can be seen between our Psychopathy and his Evaluation, and our Extraversion and a combination of his Potency and Activity.

The above analysis is all that would be done in the classical application of the semantic differential technique, and the next step would then be to compute factor scores for each individual on each concept, and finally mean factor scores for each concept. When we turn to Table 2 we see the dangers in this procedure. This table shows the rotated and unrotated factor loadings for the factor analysis of the ratings of each concept separately. For the unrotated factor solution only the loadings for the first two factors are shown corresponding to the two unrotated factors which were considered significant in the across-concepts analysis.

It can be seen that marked differences occur in the different sets of factor loadings. Although factor I can be identified in all analyses, factor II does not appear clearly for 'the alcoholic'; factor III does not appear for the 'mentally ill patient'; factor IV does not appear for 'the mentally ill patient' and only very weakly if at all for the 'alcoholic patient', and factor V does not appear clearly for any of the four types of patient. For each analysis where an across-concepts factor does not appear, the loadings on the factor which is closest to it are much lower on some variables and much higher on others, than they are in the across-concepts analysis. In contrast, the first unrotated across-concepts factor appears in much the same form in all four within-concepts analyses, and to a lesser extent the second unrotated factor.

Table 1. *Across-concepts factor loadings*

(Decimal points omitted)

	Scales	Rotated factors I	Rotated factors II	Rotated factors III	Rotated factors IV	Rotated factors V	Unrotated factors I	Unrotated factors I	Unrotated factors III	Unrotated factors IV	Unrotated factors V
Factor I											
6	Self-assertive/timid	*67*	−03	13	05	02	*65*	−05	−18	06	13
23	Dominating/submissive	*65*	08	07	01	16	*62*	−03	−24	07	−05
18	Shy/self-confident	*60*	−06	07	14	−03	*−56*	10	19	02	−18
10	Aggressive/placid	*55*	25	17	−10	01	*56*	24	−18	05	−04
16	Mild/forceful	*55*	25	17	−10	01	*−56*	−16	22	17	21
17	Tends to be elated/depressed	*41*	*−32*	17	07	04	*42*	−25	05	15	21
14	Strong influence on others/weak influence	*37*	−15	27	13	13	*47*	−17	11	04	04
Factor II											
1	Neat appearance/scruffy	21	*−39*	−27	15	16	07	*−50*	−20	10	12
5	Harmless/dangerous	13	*55*	08	−12	−12	−16	*−51*	15	15	16
12	Friendly/menacing	07	*51*	02	−01	−27	−07	*−49*	15	27	02
2	Frank/evasive	15	*−41*	−04	*41*	07	14	*−53*	05	−14	22
Factor III											
9	Unconventional/conventional	02	10	*59*	02	−18	27	*30*	*46*	−08	09
13	Adventurous/cautious	*33*	−08	*47*	08	11	*52*	−04	28	04	01
15	Generally promiscuous/chaste	16	06	*38*	10	14	*35*	02	25	−06	−12
3	Imaginative/down-to-earth	09	04	*33*	−08	11	24	09	22	09	−11
7	Criminal/law-abiding	17	27	*33*	−28	−27	24	*52*	10	10	09
8	Impetuous/takes time to decide	26	06	*30*	−25	−09	*32*	27	10	21	07
Factor IV											
21	Sexually impotent/sexually potent	13	0	09	*48*	21	−25	*31*	−07	*36*	10
19	Selfish/unselfish	−02	−20	−15	*44*	08	03	*42*	02	27	−08
22	Follows fashion/trend-setting	10	04	27	*34*	−06	−24	04	−21	*30*	−08
Factor V											
20	Cultured/uncultured	10	−24	06	08	*56*	20	*−46*	09	16	*−32*
11	Stupid/intelligent	07	−07	04	06	*53*	−16	*32*	−05	−10	*39*
4	Incurable/curable	04	−08	−01	19	25	−09	27	−02	98	14

Table 2. *Within-concepts factor loadings*

	Rotated factors																				Unrotated factors							
	Hard-drug addicts					Alcoholics					Mentally ill patients					Soft-drug addicts					Hard-drug addicts		Alcoholics		Mentally ill patients		Soft-drug addicts	
Scale	I	II	III,	IV	V	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV	V	I	II	I	II	I	II	I	II
6	72	00	13	04	08	76	06	−09	15	−13	58	18	−06	14	13	48	08	05	−26	−15	65	−29	70	11	62	08	50	06
23	70	09	−06	−18	09	63	06	08	−22	02	61	08	−13	04	18	58	06	16	−01	−05	64	−35	60	17	63	02	51	14
18	−62	01	−03	−02	−02	−59	−01	10	−01	35	−57	−05	−08	−22	16	−42	05	08	23	43	−54	07	−54	−29	−51	−23	−44	−08
10	64	24	14	−09	01	66	14	−10	04	14	57	11	−07	−14	02	56	−18	19	−16	08	70	−09	65	−09	56	−04	43	41
16	−70	−26	12	24	11	−48	−30	08	37	−05	−28	−23	41	04	−28	−65	−00	−06	13	07	−71	18	−59	−05	−43	36	−53	−19
17	36	−19	07	03	13	25	18	02	02	−24	55	05	45	05	−05	28	09	16	−01	−38	25	−30	30	18	48	47	43	−07
14	26	03	10	−30	05	34	25	19	−13	−16	33	10	18	24	20	39	31	29	04	−11	30	−12	39	26	36	29	52	−12
1	17	−35	−46	13	31	17	−41	19	−02	−22	24	−27	21	35	04	13	46	−19	11	04	−16	−61	−05	39	10	48	06	−41
5	−20	−44	−11	10	16	−25	−06	31	34	−25	−07	−39	54	08	11	−30	41	03	−12	−06	−40	−29	−32	21	−24	57	−01	−44
12	−13	−51	−07	16	27	−26	−05	56	13	−03	05	−01	47	07	10	−08	48	18	02	−33	−37	−39	−36	26	02	41	28	−49
2	−03	−46	−24	−03	03	10	−11	08	03	−49	08	16	37	49	09	19	52	05	−26	09	−27	−39	03	42	13	49	35	−34
9	−19	21	48	−13	−14	16	56	−14	23	07	17	37	−16	−31	−24	01	−10	63	−17	14	09	46	38	−30	27	−37	30	31
13	27	−04	37	−20	−06	46	38	18	−01	−10	25	36	03	02	22	21	10	70	−03	−07	36	01	54	16	40	−04	56	14
15	19	−03	42	−05	03	11	33	09	−14	−06	13	25	−14	−23	22	25	01	47	−24	−07	28	06	24	09	26	−29	51	19
3	26	06	15	12	02	10	48	07	−10	12	05	03	− 4	−50	−02	04	−03	49	06	−32	28	−00	29	−11	05	−28	37	06
7	12	46	40	21	−05	24	30	−24	01	27	32	41	−05	−18	−31	17	−41	04	−04	09	37	46	37	−35	41	−19	−01	44
8	12	20	45	19	−05	38	06	−07	14	24	48	−08	15	−27	−05	26	−27	32	−11	−11	29	31	36	−20	38	11	33	37
21	−20	38	−10	20	−01	−02	−06	05	34	38	−02	−15	50	−14	−27	−16	−20	−16	52	18	−08	31	−08	−34	−16	37	−47	10
19	09	46	03	20	−01	07	06	−07	10	61	05	−00	03	−39	19	−04	−42	14	22	12	24	29	08	−51	07	−16	−19	41
22	05	02	−07	42	−00	−14	−27	13	−22	14	−01	−38	10	−16	−07	−17	07	−13	54	02	−02	02	−24	08	−18	12	−33	−18
20	09	−25	−05	−16	66	06	04	52	−06	−03	08	12	08	−04	59	20	51	12	20	−27	−06	−56	−01	33	21	01	37	−45
11	−06	02	08	04	−58	−04	05	−51	35	07	02	06	06	09	−55	−06	−41	−11	10	49	02	36	04	−46	−05	08	−42	45
4	−09	09	07	25	−09	05	00	−10	49	11	10	02	−04	20	−03	01	−06	00	04	35	−05	21	02	−28	10	08	−15	15

Discussion

The results of this study have important implications for semantic differential research. First, it is notable that the unrotated factor solutions give two factors which appear fairly clearly in the ratings of all four concepts. There is some justification, therefore, in scoring the concepts on these factors by means of the across-concepts factor-score estimation weights. In view of the resemblance of these factors to Osgood's primary dimensions of connotative meaning, Evaluation, Potency and Activity, the results may be taken as encouraging to those whose aim is to compare concepts in terms of these factors. But it should be emphasized that this conclusion may not be valid unless the concepts and scales are selected from a limited domain of interest as in this study.

Turning now to the results of the rotated factor solution, we have a problem in scoring the factors for certain concepts. Although factor I appeared for all of them, some of the other factors did not. There is no justification, therefore, for computing factor scores for all concepts from the across-concepts factor-score estimation weights. An example of the dangers of doing so will make this point more clearly. On the basis of a preliminary analysis of the data we concluded that factor III, Nonconformity, was valid for all four types of patient. Comparison of 'the mentally ill patient' with the others in terms of this dimension led us to conclude that he is seen as far more conformist (cautious, conventional, sexually chaste) than the hard and soft drug addicts. This did not seem to make a lot of sense, but it was not until we inspected the results of the within-concepts factor analysis that the explanation became clear; namely the factor has little relevance for mentally ill patients and their scores on it are therefore relatively meaningless. How can we get round the difficulty of obtaining scores for factors that do not apply to all concepts? The ideal way would appear to be to obtain the factor-score estimation weights for each factor from a further factor analysis of the ratings averaged across just those concepts for which the factor appeared. But such a laborious procedure was not possible within the confines of this study. Instead, as a compromise, it was decided to use the factor-score estimation weights from the across-concepts factor analysis, but to restrict the scoring to just those concepts to which the factors applied. Comparison between concepts could then be limited to factors which had validity for them.

The above method is open to the criticism that the factor-score estimation weights contain an element of error due to the presence of irrelevant concepts in the analysis. But in comparison with some of the other procedures for scoring semantic differential factors which have been adopted in the past it appears to have some merit. One common method is to summate the ratings with unit weights across the scales with highest factor loadings. Heise (1969) maintains that only two or three scales are required to measure semantic differential factors reliably. But Presly (1969) points out that such a scoring procedure unequally weights those scales 'with proportionately lower loadings'. It also has the disadvantage of all 'incomplete' factor-scoring procedures that the computed factors are likely to lose the relationship to each other which the factor analysis revealed (Moseley & Klett, 1964; Horn & Miller, 1966). Another procedure, used by Clark & Kerrick (1967), which Presly favours is to weight the scales for each concept by their loadings on the within-concept principal com-

ponents. But such a procedure appears to destroy one of the main aims of semantic differential research which is to compare the concepts in terms of a *common* set of dimensions; for although these computed scores for each factor will probably be related to each other, they can by no means be considered measures of the same factor. Finally, another quite different approach to the problem would be to carry out one factor analysis of the whole data set, i.e. the 92 scale-concept ratings. The main disadvantage of such a procedure would appear to be that the analysis of a 92×92 correlation matrix is still beyond the scope of most people. In addition, as far as identifying common dimensions for comparing the concepts is concerned, it would tell us no more than the much more feasible within concepts factor analysis.

It seems opportune to draw attention to the factor-scoring problem in this study, because of the extension of the semantic differential as a procedure for analysing judgements of people. In the best examples (e.g. Kuusinen, 1969; Warr & Haycock, 1970), Osgood's qualifier elicitation technique is employed at an exploratory stage of the investigation to derive a set of scales which can all be used to describe the concepts (individuals, types of individual) under investigation. But in such studies, in common with our own, five factors are usually required to account for the across-concepts variance (Heise, 1969), and these factors are generally rotated to some simple-structure criterion. It should be clear from the preceding discussion that the scoring of such factors is unlikely to be justified from the results of the across-concepts factor analysis alone.

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(*Manuscript received* 1 *September* 1970; *revised manuscript received* 20 *March* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 235–236
Printed in Great Britain

CONCEPT–SCALE INTERACTION IN SEMANTIC DIFFERENTIAL RESEARCH: SOLUTIONS IN SEARCH OF A PROBLEM

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Bynner & Romney (1972) have proposed a solution to the problem of concept–scale interaction in semantic differential research. Several alternative solutions are offered in the present note. Choice among these awaits a clearer statement of the problem.

Bynner & Romney (1972) gathered data for four special concepts using the same 23 rating scales from over 200 respondents in the manner of semantic differential research. They wished to explore both those factors which are common to all concepts and those factors which are uniquely associated with particular concepts. The solution they proposed is to analyse the data relating to each concept separately as well as the data pooled across concepts. Their solution creates the considerable conceptual problem of identifying the presence of common factors and concept-specific factors across five separately rotated analyses, which are linked only by inspection. Several alternative solutions are offered which share the advantage that all the data are handled within a single analytic framework.

First consider the total body of data to be analysed as a respondents by concept-scales matrix (200×92). Factor analysis of the concept-scales intercorrelation matrix (92×92) would identify those factors which are common to at least two concept-scales. While this analysis would not necessarily distinguish between *inter-* and *intra*-concept common factors, at least the problem lies within one analysis rather than five analyses. Although the analysis is large, clarity of analytic intentions should have priority over ease of computation.

For the second approach, regard the preceding 92×92 matrix of intercorrelations as partitioned into a four by four supermatrix in which the four groups of scales are identified. The four diagonal submatrices contain the intra-concept scale intercorrelations. The twelve off-diagonal submatrices contain the correlations between the scales for different concepts. One tactic would be to require, first, a factor solution which best satisfies the data in the off-diagonal or inter-concept submatrices, and, secondly, a factor solution for the variance which remains unique to each diagonal or intra-concept matrix. A problem associated with this approach is that some factors arising at the first stage may be common to just two or three of the concepts, but again the problem arises within the one analytic framework. This type of solution has been discussed most recently by McDonald (1970).

Thirdly, suppose the investigators were to gather additional data for the same respondents on standard concepts and scales that may be trusted to identify well-understood factors, for example, the original Osgood factors. First, analyse the data for the standard concepts; next, project the data relating to the special concepts into that factor space; and finally, analyse the special concept variance not accounted for in this way.

Fourthly, since it is clear that semantic differential data has three bases of organization, namely respondents, concepts and scales, some version of three-mode factor analysis (e.g. Tucker, 1966) might be illuminating. Unlike the preceding analyses, three-mode factor analysis would recognize the true structure of the present data in which the scales used for each concept are nominally the same.

Finally, we might consider the general point raised by Bynner & Romney's reference to standardization of the ratings for each concept. It might be thought that differing means and standard deviations of ratings across concepts, scales and respondents are important features of the semantic space. If this is the case then consideration ought to be given to methods which do not arbitrarily attempt to eliminate such differences. The analysis of raw cross-products and of covariances has been discussed most recently by Horn (1969) and provides, by interaction with the above methods, several further approaches to solution. Choice among the alternative solutions suggested here awaits a clearer statement of the problem.

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(*Manuscript received* 17 *April* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 237–242
Printed in Great Britain

OPERANT FACILITATION DURING A PRE-REWARD STIMULUS: DIFFERENTIAL EFFECTS IN HUMAN SUBJECTS

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Thirty-two human subjects received contingent or random presentations of a conditioning stimulus (CS) and an unconditioned stimulus (US) whilst working on one of two time-related baseline schedules of positive reinforcement. The US consisted of a free reward of high or low magnitude. The two levels of each of these three variables were incorporated in a $2 \times 2 \times 2$ factorial design. A response facilitation in the contingent CS–US groups was found. Results are discussed with reference to the difficulties of using simple operant techniques with human subjects.

Ongoing operant behaviour may be disrupted by the presentation of a stimulus terminating in a non-contingent positive reinforcer. In most studies this stimulus (CS) has caused response facilitation, whilst occasionally a suppression of responding has been observed.

Estes (1943, 1948) demonstrated CS-produced operant acceleration during extinction of fixed interval (FI) responding in rats. The effect was similar regardless of whether the Pavlovian conditioning had taken place in extinction or before the operant response had been learned. Herrnstein & Morse (1957) showed a large facilitatory effect of CS which led to free reward for pigeons responding on a schedule which differentially reinforced a low rate of responding (DRL 5 min.). Henton & Brady (1970) have confirmed this result using rhesus monkeys working on DRL 30 sec. However, Azrin & Hake (1969), using variable interval (VI) baseline schedules, found response suppression during a CS in rats.

Apart from the baseline schedule and species used, these studies varied in several other ways. Henton & Brady used an unconditioned stimulus (US) quantitatively different from the reinforcer maintaining the behavioural baseline. Azrin & Hake used USs which were different in size, or both size and type, from the response-produced reward. Herrnstein & Morse used the same stimulus as both US and reinforcer, and presented the US *during* the CS period. More usually (cf. Azrin & Hake, Henton & Brady), the US has been presented on the termination of the CS. Furthermore, Henton & Brady only found a facilitatory effect with a long duration CS, whereas Azrin & Hake found suppression with a short (10 sec.) CS.

The present study was designed to investigate the effects of signalled non-contingent reward on the operant behaviour of human subjects, whilst systematically varying some of the more relevant parameters. Short experimental sessions were used since they had proved efficacious in a previous study (Remington & Strongman, 1970). Subjects worked on a baseline reinforcement schedule of either VI 30 sec. with a short limited hold (LH 5 sec.) or DRL 15 sec. They worked to score points which had a cash value, but also received free reward in the form of cash points.

A $2 \times 2 \times 2$ factorial design was employed, the three main factors being baseline

schedule (VI or DRL), magnitude of free reward (1 or 10 points) and CS–US contingency (normal experimental conditions or Rescorla control procedure; Rescorla, 1967).

METHOD

Subjects

Thirty-two male college students (mean age 20 years) were assigned randomly to one of the eight treatment groups. The subjects were told that they could earn money by taking part in the experiment, but were unaware of how much. All subjects were volunteers.

Apparatus

The subjects sat individually at a table in a sound-attenuated room facing a 20 cm lever mounted vertically in a rectangular case. The lever served as a manipulandum. A response was defined as the displacement of the lever through 50° towards the subject and back again. Behind the lever, easily visible to the subject, was a stimulus panel which displayed an impulse counter, a red pilot light and two green pilot lights. Concealed behind this panel was a clicker, which operated when a response was made, and a buzzer which served as CS. The red light pulsed briefly if the subject scored a point on the counter. The counter also registered free rewards. However, when one free reward was presented, both red and green lights pulsed for 1 sec. Ten free points were signalled by both red and green lights pulsing rapidly 10 times in conjunction with delivery of the points. Again, delivery of free reward took 1 sec. The pairing of red *and* green lights with free reward served to discriminate this type of reward from that available under the baseline reinforcement conditions. Thus the possibility of adventitiously maintained behaviour during the CS was minimized.

Procedure

Each subject was seated in the experimental room and his watch was removed. He was then asked to read the following instructions, which, typed on a stiff card, remained in the room throughout the session.

'As you know, this situation has been arranged so that you can make money. The counter gives you a measure of how much you have made. Every time a point is scored on the counter you have made ⅔d. (i.e. 3 points = 2d.), and you will be paid according to the number of points scored at the end of the session. The lever is there to help you earn money. You can pull it any way you want, but only when you pull it such that it produces a distinct "click" from within the wooden box will it be maximally effective in earning money. You can earn a lot or a little; how much depends largely on what you do. Nevertheless, the only requirement we make as to what you do is that you will remain in the room for 90 min.; the duration of the session. However, if you are interested in making money, it will be worth your while to stay alert. Read this through as many times as you wish, but please do not ask any questions.'

These instructions were designed to give minimal information about both the task itself and the monetary value of participation. The experiment then proceeded in two phases.

1. *Acquisition.* All subjects received 15 min. exposure to the VI or DRL baseline schedule, starting from the time that the door to the experimental room was closed. This period was free of both classical conditioning stimuli (CS and US).

2. *Test phase: signalled free reward.* Following acquisition the subjects received 18 presentations of CS and US in 75 min. The CS consisted of the operation of the buzzer for 30 sec. The subjects in the four experimental groups experienced 18 CS–US pairings randomly spaced over the 75 min., whereas control subjects received 18 presentations of the CS and 18 presentations of the US randomly and independently. Four treatment groups received 18 × 10 free points, whilst the other four received only 18 × 1. The subjects were not made to serve as their own controls, since it was thought that the experimental conditions to be used would lead to adequately stable response rates during CS for control subjects, thus enabling meaningful comparisons to be made.

During this phase, response rate was recorded during each CS, each 30 sec. period after a US (post-US), and each 30 sec. period preceding a CS (pre-CS).

Overall response rates and reinforcements gained were monitored during both phases of the experiment. All programming and data recording was controlled using conventional electromechanical equipment housed in an adjacent room.

Results

Median response rates for each subject during the 18 pre-CS, CS and post-US periods are shown in Table 1. This particular measure of central tendency was used to reduce the wide individual variability shown especially during the first few presentations of the CS. Although the study was designed factorially, the experimental conditions used made it clear that inflection ratio scores (Hunt *et al.*, 1952) would not follow a normal distribution. Non-parametric analyses (Wilcoxon T test; Siegel, 1956) were therefore made. These centred on within-subject comparisons of (1) pre-CS response rate and the rate during CS, and (2) pre-CS response rate and post-US response rate. The fact that ratio measures are not comparable over changing baseline conditions (cf. Millenson & De Villiers, 1971) provides a further justification for this type of analysis.

An overall comparison between pre-CS and CS rate for all subjects showed a significant facilitation during CS ($n = 32$; $T = 63{\cdot}5$; $P < 0{\cdot}05$, two-tailed). All ns reported are before the elimination of ties. This effect was maintained across the experimental groups ($n = 16$; $T = 13$; $P < 0{\cdot}025$, one-tailed), whereas there was no significant difference across control groups ($n = 16$; $T = 19{\cdot}5$; $P > 0{\cdot}05$). Further, the CS rate was significantly greater than the pre-CS rate in the VI experimental subjects ($n = 8$; $T = 4$; $P = 0{\cdot}025$, one-tailed), but this was not the case for DRL experimental subjects ($n = 8$; $T = 6$; $P > 0{\cdot}05$). Thus CS facilitation was largely due to the contribution of the experimental groups, particularly those which had been established on a VI baseline.

Experimental and control DRL groups taken together displayed a significant CS facilitation ($n = 16$; $T = 11$; $P = 0{\cdot}025$, one-tailed), although this effect was not shown across the VI groups ($n = 16$; $T = 14$; $P > 0{\cdot}05$). None of the remaining statistically possible comparisons between the pre-CS and CS median response rates

Table 1. *Median response rates for all subjects during pre-CS, CS and post-US intervals*

Ten free points						One free point					
Experimental			Control			Experimental			Control		
Pre-CS	CS	Post-US	Pre-CS	CS	Post-US	Pre-CS	CS	Post-US	Pre-CS	CS	Post-US
VI 30 sec. LH 5 sec.											
11·0	10·0	10·5	16·0	16·0	19·0	26·0	28·5	28·5	12·0	12·0	15·0
15·5	13·5	16·5	18·0	20·5	16·0	29·5	32·5	29·0	15·0	18·5	26·0
43·5	50·5	43·0	7·0	7·5	4·0	9·5	12·5	14·0	53·0	48·0	51·5
11·5	13·0	13·0	19·0	20·0	19·0	17·0	20·0	19·0	51·0	54·0	49·0
DRL 15 sec.											
1·0	1·0	1·0	2·0	2·0	2·0	2·0	2·0	2·0	3·0	4·0	3·0
1·0	1·5	2·0	2·0	1·5	1·0	2·0	11·0	3·0	2·0	2·0	2·0
1·0	1·5	2·0	2·0	2·0	2·0	2·0	2·0	2·0	12·0	3·0	13·0
2·0	2·0	2·0	12·0	20·0	29·0	1·0	0·0	2·0	2·0	2·0	2·0

yielded significant results. It should be noted that this included the various analyses of magnitude of free reward.

There was no significant difference between median pre-CS and post-US response rates when this comparison was made across all subjects ($n = 32$; $T = 70{\cdot}5$; $P > 0{\cdot}05$). However, for the experimental groups alone there was a significant post-US acceleration ($n = 16$; $T = 6$; $P < 0{\cdot}01$, two-tailed). All further possible comparisons of pre-CS and post-US response rates showed no significant differences.

Table 2 shows mean response rates for all subjects during (1) the 15 min. acquisition period and (2) those parts of the test period outside the times during which CS and post-US measures were taken (i.e. 57 min. 'normal test time'). As can be seen, the response rate during acquisition was higher for the VI groups than for the DRL groups (Mann–Whitney U test: $n_1 = 16 = n_2$, $U = 53{\cdot}5$; $P < 0{\cdot}01$, one-tailed). This effect was maintained during normal test time ($U = 11$; $P < 0{\cdot}005$, one-tailed). Comparisons within VI and DRL groups showed there to be a significant increase in response rate from acquisition to normal test time for VI subjects ($n = 16$; $T = 6$; $P < 0{\cdot}01$, two-tailed) and a significant decrease over this same transition for DRL subjects ($n = 16$; $T = 19$; $P < 0{\cdot}01$, one-tailed).

DISCUSSION

Operant acceleration during pre-reward stimulus

The results indicate that a CS, paired with free reward, causes response facilitation in human subjects working for money reinforcement on two time-related schedules. However, comparisons with the appropriate control groups show that the effect is mainly attributable to subjects in the VI 30 sec. LH 5 sec. group, control subjects on either VI or DRL schedules showing no significant effect. Apart from the operant acceleration attributable to the pairing of CS and US, a significant facilitation was found for all subjects working on DRL 15 sec., although there was no differential

Table 2. *Mean response rates for all subjects during the* 15 *min. acquisition period and* 57 *min. of normal test time*

VI 30 sec. LH 5 sec.		DRL 15 sec.	
Acquisition	Test normal time	Acquisition	Test normal time
19·1	21·2	7·7	2·5
16·9	29·8	4·9	3·1
39·4	80·6	11·7	6·5
13·3	25·9	14·5	3·6
25·9	35·9	23·7	3·8
18·5	33·5	12·8	3·4
27·9	16·5	4·0	3·5
23·7	34·4	19·2	40·6
40·7	49·0	5·0	3·2
41·5	60·2	15·2	7·6
9·7	25·5	4·9	4·0
23·3	32·7	14·6	2·7
13·6	43·7	7·6	9·3
1·9	75·4	8·3	3·5
30·7	92·6	30·0	25·4
85·0	99·3	14·3	4·4

effect between experimental and control subgroups. This effect may be due to the disruption of overt or covert collateral behaviour (cf. Bruner & Revusky, 1961) at the onset of the auditory CS employed. Interference with this non-criterial behaviour could produce a limited effect over the reasonably large sample of subjects used, but this effect would perhaps not be great enough to be maintained for either subsample (i.e. experimental and control DRL groups).

The CS facilitation effects contradict most earlier findings in this field. Previously, an operant acceleration has been shown on DRL schedules, and a suppression on VI schedules. Here, experimental subjects on DRL showed no effect, but experimental subjects on VI showed facilitation during the CS. These somewhat paradoxical findings may be the result of a species by schedule interaction.

Kaufman *et al.* (1966) have shown that when inaccurate instructions (regarding the required response and reinforcement schedule) have been given to human subjects, the effect of these far outweighed those of the contingencies actually in operation. More recently, Remington & Strongman (1970) have shown that instructions which defined a time-out period annihilated facilitation effects during a pre-time-out stimulus. Rosnow & Rosenthal (1970) suggest that volunteer subjects may seek approval for their performance in a task, a need presumably fulfilled by obeying experimental instructions accurately or attempting to perform an experimental task skilfully.

A DRL 15 sec. schedule produces continuous reinforcement if a subject responds exactly every 15 sec. The rate which maximizes the rate of reinforcement (RMR; Lane, 1960) is thus 4 responses/min. Results show that, during the test phase, most DRL subjects decreased their rate until it approached the RMR (see Table 2). Inspection of the cumulative records for these subjects showed that response bursts – a relatively high frequency of short inter-response times, commonly found with infra-human subjects on DRL (e.g. Brady & Conrad, 1960) – were absent. Thus human subjects responding optimally on the DRL schedule were receiving virtually continuous reinforcement. Viewed as positive feedback this may have exerted as precise a control over DRL responding as would be given by definitive experimental instructions. Compared with this strong control of the behavioural baseline, the effects of a signal leading to a small monetary reward would be overshadowed.

Clearly, this sort of control cannot be exerted by a VI schedule. For VI 30 sec. LH 5 sec. the most efficient rate is 12 responses/min. spaced at 5 sec. intervals, but a subject responding thus will be reinforced for only one response in six *on the average*, with this reinforcement delivered at virtually unpredictable intervals. In the present experiment, subjects on this schedule responded at a higher rate than 12 responses/min. during acquisition, and response rates in general continued to increase during normal test time. VI subjects therefore were not under such precise schedule (i.e. instructional) control as DRL subjects. Against this baseline, stimuli associated with relatively small monetary reward had a greater, significant, effect.

Perseverative effects

Post-US measures were taken in an endeavour to explore any possible transient perseverative effects of free reward. Were these to be apparent, they should have been reflected mainly as differences between groups receiving varying magnitudes of free reward. Such differences were not found. This may imply that, under the

experimental conditions used, neither magnitude of reward appeared strongly attractive to subjects. At present it is difficult to account for the apparent perseverative post-US effect found across experimental groups, unless it may have been simply response overspill from their significantly higher, immediately preceding, rate during the CS.

Control procedure

The results raise some doubts as to the suitability of the Rescorla control procedure for CER-type studies using human subjects. In order to produce measurable emotional effects in humans responding on simple operant tasks for monetary reward, a brief experimental period is desirable. If the experiment requires many sessions to complete, then the absence of some (e.g. social) reinforcers may have more control than those which may be manipulated experimentally. Subjects become bored. However, the Rescorla control procedure requires many independent and random CS and US presentations spaced over a long period of time. If, in rats, trace conditioning can occur during CER procedure (Kamin, 1961) it seems likely that human subjects will form some association between CS and US in the Rescorla control condition, an effect which can be countered only by using long experimental sessions.

This study was supported by Medical Research Council grant G969/267/B to the second author.

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(*Manuscript received* 17 *March* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 243–254
Printed in Great Britain

THE ATTENUATION OF VISUAL PERSISTENCE

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Haber & Standing (1969, 1970) have shown that visual persistence (VP), which it is suggested corresponds to Sperling's 'Visual Information Store', can be directly measured. The present investigation extends this approach with another direct measure of VP. This involves recording the number of phenomenally simultaneous lines that are experienced when viewing a single trace moving in a straight line, at a sweep speed above the fusion threshold, across the face of a rotating CRO. The results from three experiments suggested that VP for the apparent movement produced by this display was a decreasing function of field brightness, a curvilinear function of sweep speed, and an *increasing* function of rotation speed and of the spatial separation of the lines. One implication of the latter findings, namely that termination of VP may be brought about by lateral inhibition, received additional support from the finding that disruption by a superimposed noise field *increased* VP.

There has been much interest in the possibility of visual persistence since Sperling (1960) demonstrated the phenomenon using a method of partial report. Since then other experiments along similar lines (e.g. Averbach & Coriell, 1961; Keele & Chase, 1967) have confirmed his findings. All indicate that the duration of the effect is of the order of 250–350 msec.

Recently Haber & Standing (1969, 1970) have measured VP using direct methods, with comparable results. Their first method (Haber & Standing, 1969) consisted of presenting the same stimulus in a repeating cycle. They used an oscillating slit through which the stimulus (an outline circle) could only partly be seen at any time. In a second demonstration they repeatedly exposed the stimulus using a tachistoscope. In either case the cycle rate was adjusted until the subject reported that the whole stimulus was phenomenally present at any one time. At this stage the cycle time provided a measure of the duration of persistence necessary for the perception of phenomenal continuity.

In a further study (Haber & Standing, 1970) they successfully adopted a method suggested by Sperling (1967). The subject saw a brief flash and at the same time heard a click. The subject was required to adjust the timing of the click until it appeared to coincide with the onset of the flash. It was then adjusted so that it appeared to coincide with the offset of the flash. The interclick interval provided the measure of the phenomenal duration of the flash.

Efron (1970) has also measured VP directly by setting a click to coincide first with the onset of a flash, and then with the offset. He also found that VP exceeded the physical duration of the flash.

Some conclusions to emerge from these studies are: (1) Persistence is greater for short exposures than for long exposures, the net effect being to guarantee a minimum effective duration of perception. (2) Persistence varies with the brightness of the stimulus or surround, but in general it is 250 msec. or less for light pre- and post-exposure fields, and about 400 msec. for dark pre- and post-exposure fields. (3) Persistence is abruptly terminated by the occurrence of a following visual noise mask

(Haber & Standing, 1970). Thus there seems to be a mechanism which will prevent earlier images interfering with later images.

The present investigation continues in this tradition with a different direct measure of VP. Apart from its ease of application, it promises certain methodological advantages in that it permits the continuous investigation of factors that result in a less abrupt termination of VP than with masking visual noise.

METHOD

Apparatus. In the first two experiments a rotating cathode-ray oscilloscope was used to generate the stimuli. The electron beam traversed a 10 cm path along the diameter of the screen. The sweep speed could be varied, but the flyback time was constant at 5 msec., after which the beam was triggered internally to retrace the same path. When the CRO was rotated about its axis, a fan of intersecting, phenomenally simultaneous lines resulted. The effect is similar to that recently described by Allport (1970).

The screen of the CRO was coated with P–1 phosphor, with a decay rate such that brightness was reduced to 0·01 per cent of its initial value within 180 msec. The tube was mounted between three rollers spaced 120° apart. One of these was coupled to a variable speed motor, and rotated the tube. A commutator with 12 concentric copper rings was mounted co-axially with the end of the tube. A flat springy silver-alloy brush pressed upon each ring. Each brush was wired to its original place in the circuit of the oscilloscope. This arrangement permitted uninterrupted functioning of the CRO even during rapid rotation. A graduated control permitted the experimenter to vary the sweep speed of the CRO. The sweep speed and flyback time were continuously monitored using a second oscilloscope. The calibration of the sweep speed control was checked before and after the experimental sesssion.

EXPERIMENT I

This experiment, carried out by N.F.D., was to establish the practicability of the method, and to determine some of the factors that affect VP. The method, like others, depends on the phenomenal simultaneity of events. When the CRO is rotated, the phenomenal result is the apparent rotation (phi movement) of a number of intersecting lines. Though this is illusory, the number of lines seen permits an inference about the VP in that situation. For example, if at a given speed of rotation the sweep speed is increased until the subject reports that he can just see three lines, then it seems reasonable to suppose that the whole configuration has been stored visually for the time required for each line to be traced, and also for the interval between successive sweeps. Similarly, if sweep speed is held constant, but trace brightness is changed, then any change in the number of lines seen will indicate changing persistence.

Method

Design. The independent variables were the speed of rotation of the CRO, the brightness of the trace, and the brightness of the field (i.e. overall illumination of the room and screen). There were two levels of trace brightness: 'bright' and 'dim'. These were not quantified, but were unambiguous enough for the purposes of the experiment. There were two levels of field illumination. These were measured as the light reflected from the CRO screen using a SEI photometer, and were 'bright', 2·0 ft.-lm.; and 'dim', 0·02 ft.-lm. Three speeds of rotation of the CRO were used, 0·4, 0·8 and 1·0 rev./sec. Thus there were twelve experimental conditions.

Procedure. Each subject was tested on every experimental condition in randomized order. There was m addition a 'dummy' replication factor, which was based on the number of lines seen: within each experimental condition, sweep speed was gradually increased until the subject

could just see two lines, when he would respond 'two', then until he could just see three lines, and so on until he could just see five lines when that trial would be terminated. At each judgement point, the experimenter noted the sweep speed that resulted in the change in the number of lines seen. It was believed (erroneously as it turned out) that each of these judgements would provide an independent estimate of VP.

The range of sweep speeds required to elicit judgements of from two to five lines varied depending on the conditions. The overall range was from about 1000 to 100 cm/sec. Thus the duration of a complete traverse of the 10 cm path varied from 10 to 100 msec. The actual measure of interstimulus interval was based upon the rationale that if the CRO was stationary, then a given retinal receptor would be cyclically stimulated with a period equal to the duration of the sweep, plus the flyback time. In these terms, ISIs ranged from 15 to 105 msec.

Subjects were tested individually in a session lasting from 30 to 45 min. The subject was seated 50 cm from the CRO. He was shown the rotating display to establish that he could see the multiple lines, and also how the number of lines varied with sweep speed, although the mechanism was not explained to him. He was told to fixate the centre of the rotating screen (i.e. the point of intersection of the lines) while making his judgements. The subject was told that in each trial the number of lines would increase from less than two to five or more. He was to respond verbally with the appropriate number whenever the apparent numerosity of the lines changed. Judgements of greater than five were not elicited because of the subjective difficulty of counting past this number. Exposure time was not controlled, in that the subject viewed the CRO screen continuously during trials. Allport (1970) found that prolonged viewing resulted in a spread of the fan of lines over the display, but this did not occur here.

During each trial the experimenter gradually and continuously increased the sweep speed from a low rate that resulted in less than two lines being seen, until at least five lines could be seen. The sweep speed that resulted in each reported change in the number of lines seen was noted.

Subjects. The seven subjects had normal or corrected to normal vision. They were undergraduate or postgraduate students, and were paid for their participation.

Results

VP was computed according to the following rationale: however many phenomenally simultaneous lines the subject saw, his visual system must have stored this information for at least the duration of each sweep, and the duration of the interval between each sweep, viz. the $(N-1)$ flybacks between N sweeps. The equation for computing VP was

$$\mathrm{VP} = (N \times \tau) + (N-1) \times \tau_0,$$

where N = number of lines seen, τ = duration of each sweep, τ_0 = flyback time.

Since there is some indeterminacy about VP for sweep speeds between those resulting in a change in perceived number of lines, VP was estimated at the points of change only.

The results for different speeds of rotation of the CRO are given in Table 1, and for the different illumination conditions, in Table 2. These data were submitted to an analysis of variance (speed of rotation × trace brightness × field brightness × subjects × replications). This showed that VP was an increasing function of speed of rotation of the CRO ($P < 0{\cdot}01$). Increased trace brightness decreased VP, but not significantly – however, the effect is in the same direction as found by Haber & Standing (1969) and Allport (1970). VP was a decreasing function of field brightness ($P < 0{\cdot}01$) which agrees with Haber & Standing's (1969, 1970) finding that VP is greatest when both pre- and post-exposure fields are dark.

Perusal of the data revealed a further factor which had not been anticipated, but which has since been reported by Allport (1970). This is that VP is a curvilinear

function of ISI, being greater for intermediate values than for short or long ISIs. However, what did not emerge from his results, since he used only one speed of rotation (1·0 rev./sec.), was that there is a clear interaction between ISI and speed of rotation. At low speeds of rotation, VP is virtually invariant with respect to ISI

Table 1. *The mean duration of visual persistence (in msec.) for different speeds of rotation of the CRO*

Speed of rotation (rev./sec.)		
0·4	0·8	1·0
114·8	149·9	208·6

Table 2. *The mean duration of visual persistence (in msec.) for different conditions of trace (stimulus) and field (surround) illumination*

	Illumination	
	Bright	Dim
Trace	153·3	162·1
Field	129·1	186·3

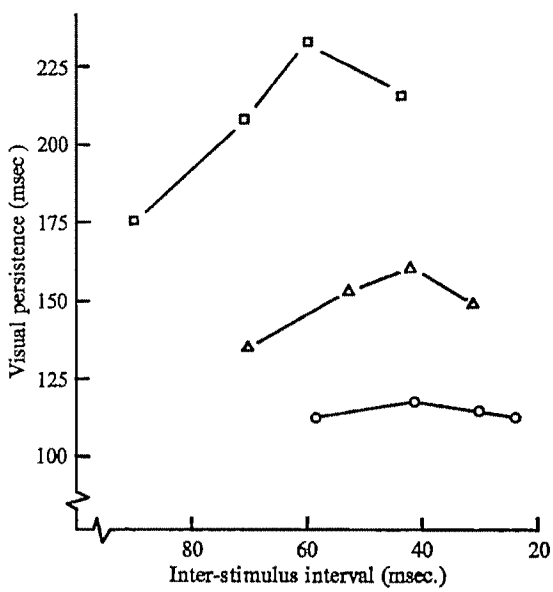

Fig. 1. Visual persistence as a function of the speed of rotation of the CRO and ISI (the period of the sweep). Speed of rotation (rev./sec.): ○—○, 0·4; △—△, 0·8; □—□, 1·0.

(as was originally anticipated). At higher speeds of rotation, the relationship becomes more and more curvilinear, as shown in Fig. 1. Fig. 2 shows the effect of the different conditions of illumination on VP. In both Figs. 1 and 2 the plotted points, left to right, represent the estimates of VP based on two lines seen, three lines seen, four lines seen, and five lines seen, respectively.

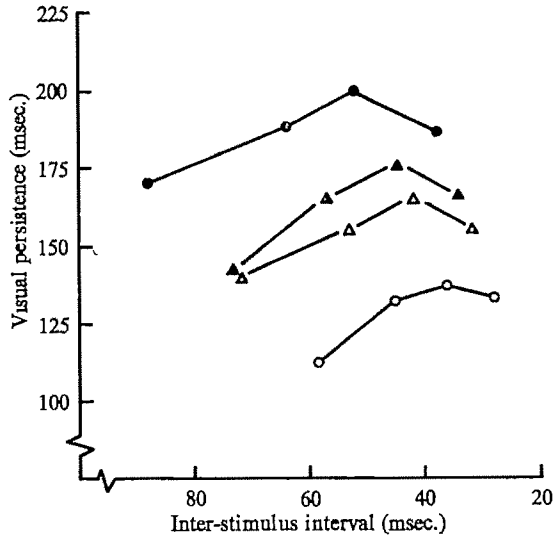

Fig. 2. Visual persistence for different conditions of field and trace illumination. Trace: △—△, bright; ▲—▲, dim. Field: ○—○, bright; ●—●, dim.

Discussion

The changes in VP found as a consequence of illumination changes are comparable to those found by Haber & Standing (1969, 1970), using different direct methods. This strengthens the probability that the same phenomenon is under investigation in each case. The effect of changes in stimulus brightness is relatively slight (not significant in this study) while the effect of field, or surround illumination is greater. In both cases, an increase in illumination results in a decrease in persistence. This may be a consequence of a decrease of the critical duration for visual integration of luminance (Barlow, 1958).

However, while the *changes* in VP are comparable with those found previously, the absolute magnitudes of VP are not. In the present study they are smaller than might be expected. For example, in Haber & Standing's fourth paradigm (1970, figs. 1 and 2), where the pre- and post-exposure fields were dark, persistence was found to be about 400 msec. In the present experiment, the analogous (dim field) condition resulted in VP of only 186 msec. One possibility is that persistence is being curtailed in some way in the present experiment. This seems quite likely when the effects of speed of rotation of the CRO are examined. This variable resulted in the largest absolute change in VP, an increase of almost 100 msec. as speed of rotation was increased from 0·4 to 1·0 rev./sec. The effects of an increase in rotation speed are twofold. First, there is an increase in the mean spatial separation of successive sweeps, and hence between the phenomenally simultaneous lines. Secondly, there is an increase in the speed of apparent movement of the fan of rotating lines.

The spatial factor is an obvious one in visual experiments. There is much evidence that lateral inhibition is an inverse function of the spatial separation of stimuli (e.g. Leibowitz *et al.*, 1953; Ratliff, 1961). More particularly Alpern (1953) has shown that the extent to which an earlier stimulus is suppressed by a later one depends,

among other things, on their separation (the after-flash effect, or metacontrast). On the other hand, the changes in the apparent movement *per se* may be important. Kahneman (1968) has shown that the degree of metacontrast suppression, and the quality of apparent movement, are closely linked. These arguments are extended in the General Discussion.

The effect of spatial separation as a consequence of speed of rotation is difficult to assess on the basis of the present results. This is because the separation between successive sweeps is also a function of ISI. For example, if the speed of rotation is doubled, and the ISI is halved, the spatial separation between successive sweeps remains constant. Nevertheless, in such instances, VP was still found to increase (see Fig. 1). This is not really surprising. Decreasing ISI in this experiment meant a shorter duration for each sweep, and persistence is greater for short durations (Efron, 1970; Haber & Standing, 1970). However, this does make the role of spatial separation as a factor rather difficult to interpret.

To determine the extent to which spatial separation *per se* was important, especially in terms of possible lateral inhibition between successive sweeps, two additional experiments were conducted by E. J. H. In the first of these, the independent variables were (*a*) speed of rotation of the CRO, (*b*) viewing distance and (*c*) the presence or absence of a disruptive pattern superimposed on the CRO screen.

EXPERIMENT II

The purpose of this experiment was to determine whether varying the spatial separation of successive stimuli on the retina by a method other than changing speed of rotation would have a comparable effect on VP. The simplest way to vary the mean angular separation between sweeps, at a given ISI and speed of rotation, was to vary the viewing distance. Though expedient, this was not ideal, since the angular separation was confounded with retinal locus. The whole display subtended a smaller visual angle when viewed from further away.

It seemed likely that if the curtailing of VP is due to lateral inhibition, itself an inverse function of the distance between successive sweeps, then it could be reduced by disrupting the sweeps in a random fashion. This would reduce the point-to-point correspondence between successive lines, and hence lateral inhibition. To achieve this, a random pattern was superimposed on the CRO screen. Structurally this resembled the noise patterns often used as masks in visual experiments. Functionally it was quite different. Its only purpose was to break up each sweep into a series of straight line segments of varying length. Put simply, the display was unchanged except that the lines had 'bits missing'. While this reduced the total light output of the display to an extent, Expt. I had shown that this was not likely to be a significant factor.

The speed of rotation of the CRO was varied as before, but only two speeds were used.

The predictions were that increased viewing distance would result in less VP since successive lines would be less separated on the retina; that the presence of the disruptive pattern would facilitate VP through a reduction in lateral inhibition; and that interactions might be expected (*a*) between speed of rotation and the presence

of the disruptive pattern, since the latter should have a greater effect at low rotation speeds (less separation between lines) than at high rotation speeds (with more separation), and (*b*) between viewing distance and the presence of the disruptive pattern for the same reason.

Method

Apparatus. The apparatus was as for Expt. I. Only two speeds of rotation of the CRO were used: 0·5 and 1·0 rev./sec. There were two viewing distances, 50 and 100 cm, and at these distances the whole display subtended angles of 11° 30′ and 5° 45′ respectively. The characteristics of the disruptive pattern were: mean length of line segments = 0·18 cm; mean length of gaps between segments = 0·14 cm. These lengths ranged from 0·02 to 0·50 cm and from 0·01 to 0·30 cm, respectively.

Design. There were eight experimental conditions (two distances × two speeds of rotation × presence or absence of the disruptive pattern). Each of eight subjects was tested on every condition. The order of treatments was randomized according to an 8 × 8 Latin square.

Procedure. The procedure followed that of the first experiment with the following modifications, introduced to obviate the possibility of response biases: (*a*) both ascending and descending methods of limits were used; (*b*) sweep speed was not varied continuously, but in discrete steps – ISI was effectively increased or decreased in 4 msec. steps; (*c*) the subjects did not view the display continuously, but closed their eyes between judgements, and made the judgements as swiftly as possible, within about 5 sec.; (*d*) as a simplification, only the threshold for seeing four lines was used to compute VP. Within each experimental condition there were four replication trials, two ascending and two descending, to provide an indication of intra-subject variability. In spite of these changes, the results of the two experiments corresponded very closely.

Instructions to the subjects were as before, except that they were told that they would only be concerned with three and four lines. For ascending trials, the experimenter adjusted the sweep speed until the subject could see three but not four lines. The subject closed his eyes, the experimenter increased the sweep speed (i.e. reduced ISI) by one step, and gave the signal 'right'. The subject opened his eyes, made his judgement, and closed his eyes until the next trial. This continued until the subject reached a criterion of three successive judgements of four lines seen. The reverse procedure was followed for descending trials.

All the experimental conditions took place in the 'dim field' and 'dim trace' conditions of Expt. I. The subjects were dark adapted for 5 min. before trials began. A typical session lasted for about 45 min.

Subjects. The eight subjects had normal or corrected to normal vision. They were undergraduate or postgraduate students, and were paid for their participation.

Results

VP was computed as before. In this experiment N was always four. The subjects were consistent in their judgements: the average range of persistence values for subjects, across the four trials (replications) within each condition, was only 15 msec. These ranges were subjected to an analysis of variance to determine whether some conditions produced more variability in judgements than others. There were no significant F ratios. The results of the experiment are shown in Table 3. These data were submitted to an analysis of variance (distance × speed of rotation × disruptive pattern × replications × subjects). The results again showed that VP was an increasing function of speed of rotation ($P < 0{\cdot}025$), and that the presence of the disruptive pattern increased persistence ($P < 0{\cdot}025$). Viewing distance was not significant. Of the interactions, only the speed of rotation × pattern interaction was significant ($P < 0{\cdot}05$). The effects of speed of rotation and the presence of the disruptive pattern are shown in Fig. 3, which also shows the interaction.

Table 3. *The effects of viewing distance, the speed of rotation of the CRO, and the presence or absence of the disruptive pattern superimposed on the screen of the CRO, on the mean duration of visual persistence (in msec.)*

Condition	Persistence
Distance	
50 cm	227·8
100 cm	236·6
Speed of rotation	
0·5 rev./sec.	212·0
1·0 rev./sec.	252·4
Disruptive pattern	
Absent	222·8
Present	241·6

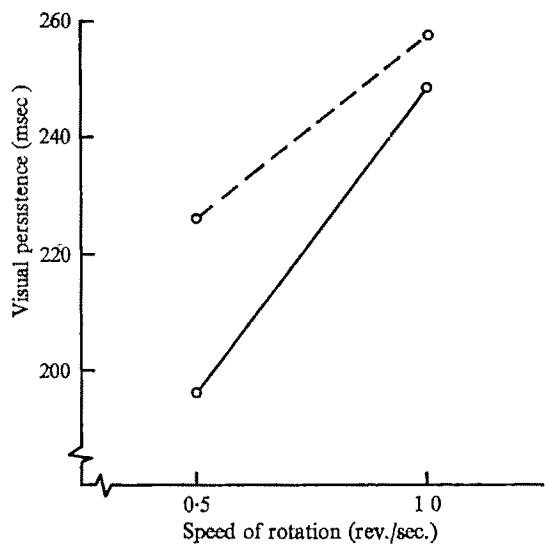

Fig. 3. The effect of the random disruption of the trace, and the interaction with speed of rotation of the CRO. ○—○, Pattern absent; ○--○, pattern present.

Discussion

Increased viewing distance (and hence a smaller retinal separation between successive sweeps) did not result in a decrease in VP as predicted on the basis of supposed lateral inhibition between lines. In fact there was a small, though non-significant increase. Unfortunately, this result is inconclusive since Barlow (1958) has indicated that temporal summation is more efficient at the fovea than at the periphery.

On the other hand, the finding that disruption of the successive sweeps with the random pattern does increase VP lends support to the notion of lateral inhibition. It was predicted that such facilitation of VP would occur through a reduction of inhibition as a result of the disruption. Another prediction that was borne out was the significant interaction between speed of rotation and the disruptive pattern. Lateral inhibition decreases with distance. It is thus predictable that if the effect of the disruption is a consequence of a reduction of lateral inhibition, it would have a smaller

effect at higher speeds of rotation where there is already a diminution of inhibition due to distance.

The above results suggest that lateral inhibition can attenuate VP. However, the role of spatial separation is still ambiguous, and so Expt. III was conducted. The object was to vary the spatial separation of successive stimuli independently of viewing distance and speed of rotation.

Experiment III

The purpose of this experiment was to test the hypothesis that increasing spatial separation between stimuli would result in progressively greater values of VP.

Method

Apparatus. Because of a change in procedure, it was more convenient to use a stroboscope as the source of illumination. The apparatus was similar to that used by Allport (1970), and is not described in detail here. Briefly, an opaque disc with a radial slot was rotated in front of a diffuse source of stroboscopic illumination. Only the slot, and a small central fixation spot were illuminated. The slot was 1·7 cm from the centre of the disc, and was 2·5 cm long and 0·1 cm wide. The angle of the slot to the radius of the disc could be continuously varied, and in this experiment angles of 0°, 30° and 60° were used (at 0° the slot coincided with the radius). The disc was rotated at a constant speed of 1·0 rev./sec. The brightness of the diffuse source, measured with a SEI photometer at 50 flashes/sec., was 0·1 ft-lm. and the ambient room illumination was 0·015 ft.-lm.

Procedure. The spatial separation of successive flashes was varied as follows: with the slot at 0° and rotating at 1·0 rev./sec., the mean spatial separation of flashes was typically about 1° 30′ (viewed from 50 cm, at ISI about 70 msec.). As the angle of the slot to the radius was increased, so the mean spatial separation of flashes decreased with the cosine of the angle. Thus, when the slot was inclined at 30°, the mean spatial separation of flashes was reduced by about 13 per cent, and when the slot was inclined at 60°, the spatial separation of flashes was reduced by about 50 per cent.

The procedure was similar to that of Expt. I, but ascending and descending trials were used, and only the threshold for seeing three lines was obtained. There were three experimental conditions (the three inclinations of the slot), and each subject was tested on each of these in a different order. Instructions to the subjects were that they would report, on an ascending trial when they could just see three lines, and on a descending trial when they could just not see three lines. There were three ascending and three descending trials, in alternating order, in each condition. The subjects were dark adapted for 5 min. before trials began.

Subjects. Three postgraduate subjects, with normal vision, were paid for their participation.

Results

VP was computed as follows: since the threshold for *just seeing* three lines was measured, there were just *two* ISIs separating the flashes. Hence persistence was taken to be $(N-1) \times$ ISI. The results are shown in Table 4. The decrease of VP with an increase of the angle of inclination of the slot (i.e. decreasing spatial separation) is significant ($P < 0{\cdot}05$, F test).

Table 4. *The mean duration of visual persistence (in msec.) as a function of the spatial separation of successive flashes (approximate angular separation when viewed from 50 cm)*

Angle of slot to radius	0°	30°	60°
Corresponding angular separation of flashes	1° 30′	1° 15′	0° 40′
Visual persistence	143·7	132·8	121·6

Discussion

The results of this experiment show that VP is indeed an increasing function of the spatial separation of successive stimuli, at least within the limits considered here (about 40′ to 1° 30′). The implications of this are examined more fully in the General Discussion.

GENERAL DISCUSSION

VP has been measured directly using a rotating display of phenomenally simultaneous lines. While it is necessary to rotate the display in order to estimate VP with this method, it is clear that the speed of rotation is itself a strong determinant of the obtained value of VP. Clearly, we should not attach too much importance to the absolute values obtained using a method of this sort. The same considerations presumably apply to the value of M, the span of phenomenal simultaneity, found by Allport (1970), which may have been a function of the particular speed of rotation that he used. On the other hand, the *changes* in VP found using this method may have implications for an understanding of the nature and function of VP.

The basic adaptive value of a mechanism of VP seems to be to prolong short exposures so that the organism is assured of a minimum effective duration of perception of at least 130–250 msec., regardless of the physical duration of the stimulus (Efron, 1970; Haber & Standing, 1970). At the same time there must exist a mechanism that can terminate persistence, and so prevent the persistence of an earlier image interfering with a later image. That VP can indeed be sharply terminated has been clearly shown in experiments where earlier images are masked by following visual noise (e.g. Kinsbourne & Warrington, 1962; Sperling, 1963; Haber & Standing, 1970). However, it is also clear that persistence can vary in a less all-or-nothing fashion than this, and the question is, in what conditions does it do so, and through what mechanism?

A general conclusion based on previous studies, and the present one, is that conditions that are visually unfavourable result in greater VP. Persistence is greater in conditions of low illumination, either of the stimulus or the surround, and is greater for physically short exposures. Conversely, persistence is reduced for conditions of brighter illumination and for longer exposures. In this context the adaptive value of persistence seems obvious.

How does this attenuation of persistence come about? The results of the present study are interpreted as supporting the notion that lateral inhibition is important in this regard. Lateral inhibition typically refers to a reduced sensitivity of the visual system as a consequence of other stimulation. It is explicitly suggested here that this diminished sensitivity is due to a reduction (as a result of lateral inhibition) in the hypothetical process underlying persistence.

In Expt. I the variable that resulted in the greatest change in VP was the speed of rotation of the CRO. One of the effects of this increased speed of rotation was an increase in the spatial separation of successive sweeps. Since lateral inhibition decreases with an increase in the spatial separation of stimuli (e.g. Leibowitz *et al.*, 1953; Ratliff, 1961), it seemed possible that the increase of VP with speed of rotation was due to a reduction of lateral inhibition. This was tested further in Expt. III, where

VP was found to be an increasing function of the spatial separation of successive flashes. In Expt. II the lateral inhibition hypothesis was tested indirectly by disrupting successive sweeps in a random fashion, thereby reducing the point-to-point correspondence between lines. This procedure can be expected to reduce lateral inhibition, and indeed VP was found to increase.

These conclusions about the role of lateral inhibition receive support from neurophysiological studies by Barlow *et al.* (1957) who investigated changes in retinal receptive field organization during dark adaptation in the cat. They found that lateral inhibition was virtually absent in the dark adapted retina, that it increased with increasing background illumination, and with increasing exposure duration. The correspondence between these results and those of the present, and other experiments on VP is striking: persistence is greatest when *S* is dark adapted (Haber & Standing, 1969, 1970); persistence decreases with increasing surround illumination (Allport, 1970; Haber & Standing, 1969, 1970); persistence decreases with increasing exposure duration (Efron, 1970; Haber & Standing, 1970). In addition, the results of the present experiments show that spatial conditions that are likely to reduce lateral inhibition result in an increase in VP.

A final point must be considered: although it was shown that spatial separation might account for the change in VP as a function of the speed of rotation of the CRO, it is by no means certain that the effects of speed of rotation reduce entirely to the effects of spatial separation. Temporal factors are likely to be important as well, as may be the perception of movement *per se*. In cases of apparent movement both temporal and spatial factors are important (Korte's Laws). Relevant here is Kahneman's (1967, 1968) conclusion that metacontrast suppression and apparent motion are closely linked phenomena. In both cases the first stimulus is either completely or partly suppressed by the second. The present results indicate that this suppression may be due to the attenuation or termination of the persistence of the first stimulus. In fact, this may well be a necessary condition for the perception of apparent motion. Something very similar seems to occur in some cases of real movement in, or of, the visual field. Thresholds of test flashes of light are raised both during, and slightly before voluntary saccades (e.g. Latour, 1962; Zuber & Stark, 1966; Volkmann *et al.*, 1968). Even more significantly, MacKay (1970) found that if the visual field moves suddenly (in the absence of eye movement), the same sort of suppression of test flashes occurs. On the basis of the present results it seems reasonable to speculate that this suppression is not just an elevation of thresholds, but is due to a reduction of the actual *duration* of the visual image. If this is true, the effect would be most adaptive since it would reduce the amount of motion blurring of the visual image, and hence improve its resolution.

Conclusion

The adaptive value of VP (which is, in effect, the 'duration of the present') is that it ensures a minimum duration of perception. While it can be abruptly terminated, more usually it varies continuously in response to situational demands. The aim of the present investigation has been to determine the conditions and the mechanism that result in changes in the duration of VP. As a generalization, it seems that VP is greater in conditions that are visually unfavourable (dim illumination, short

exposures), and is reduced in favourable conditions. It is suggested that the duration of VP is attenuated by a mechanism of lateral inhibition since the conditions of illumination, exposure duration and spatial proximity that are known to enhance lateral inhibition are the very factors that reduce persistence.

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(*Manuscript received* 21 *April* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 255–256
Printed in Great Britain

A NOTE ON THE TAXONOMY OF WITKIN'S FIELD-INDEPENDENCE MEASURES

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Vernon (1969, chap. 9) has pointed to the need for studies which map out a clearer taxonomy of Witkin's concept of field independence. As a by-product of a research project not specifically concerned with this issue, the opportunity arose to examine scores on two of Witkin's perceptual tests in groups of men and women homogeneous for age, education and social class.

The subjects were 80 men (mean age 21·28 years, S.D. 3·06) and 67 women (mean age 20·60 years; S.D. 2·77), all college of education students; 84 per cent of the men and 79 per cent of the women were from social classes 2 and 3. The subjects consisted of all those students who could be contacted by a member of the teaching staff over a 6-week period, only one person refusing.

Subsequent to the presentation of a battery of tests which included the shortened version of Witkin's Embedded Figures Test (EFT; Jackson, 1956) and the figure-drawing test (FDT; Witkin *et al.*, 1962), it was appreciated that a minority of the

Table 1. *Mean group scores on Witkin's Embedded Figures and figure-drawing tests*

		EFT		FDT	
	n	Mean	S.D.	Mean	S.D.
Non-art specialists					
Men	71	5·93	2·23	4·48	1·91
Women	57	4·84	2·19	4·25	2·10
Art specialists					
Men	9	7·44	2·28	6·11	2·24
Women	10	7·00	1·00	6·60	1·28

Table 2. *Analysis of variance of Embedded Figures Test scores*

Source	M.S.	D.F.	*F*	*P*
Sex (S)	9·83	1	2·02	n.s.
Art specialization (AS)	56·18	1	11·56	< 0·001
S × AS	1·67	1	0·34	n.s.
Within cell	4·86	143	—	—

Table 3. *Analysis of variance of figure-drawing test scores*

Source	M.S.	D.F.	*F*	*P*
Sex (S)	0·33	1	0·08	n.s.
Art specialization (AS)	66·01	1	16·42	< 0·001
S × AS	2·17	1	0·54	n.s.
Within cell	4·02	143	—	—

students were specializing in art. As Smith (1964) has pointed out, ability in art can reasonably be associated with high spatial ability. Since Sherman (1967) and Vernon (1969) have suggested an association between Witkin's field independence and high spatial ability, it was predicted that on both the EFT and FDT tests those specializing in art should score as more field independent than those not. This variable might well interact with the customary sex difference where men usually score as the more field independent (possibly also due to the operation of the spatial factor).

The mean group scores and analyses of variance (with unequal cell frequencies; Winer, 1962) are presented in Tables 1–3.

The results clearly indicate that high scores on two of Witkin's tests (indicating field independence) are associated with ability in art and thus with spatial ability. Unlike previous studies, the scores for men were not significantly higher than those for women. The reason for this is not clear, but it could be that women student-teachers tend to have relatively high spatial ability compared with most women.

Vernon (1969) has also drawn attention to a possible link between Witkin's measures and introversion–extraversion. Since the Eysenck Personality Inventory (Form A) had also been given to the students, this suggestion was investigated with no evidence of any relation emerging. The product-moment correlations between extraversion and EFT scores were: for men 0·05 and for women −0·05; between extraversion and FDT scores, for men 0·10 and for women 0·13. None of these correlations is significant and no association between introversion and field independence was established.

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(*Manuscript received* 17 *May* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 257–264
Printed in Great Britain

ADAPTATION TO SPEED DISTORTIONS UNDER WATER

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A diver's face-mask normally makes close objects in clear water appear both enlarged and too near. Objects travelling across the line of sight therefore appear to move faster than in air, and objects moving along the line of sight slower. Subjects estimated the speed of a moving rod in air and water, before and after a 10 min. period spent swimming and playing a pegboard game under water. Twenty subjects were tested with the rod moving across, and 20 with it moving along, the line of sight. In both conditions subjects showed the expected directions of speed distortion in water, with significant adaptation and after-effects after 10 mm. submersion. The results are consistent with the assumption that the type of speed perception measured here is largely dependent on size and distance perception, and that speed adaptation follows automatically from the correction of the spatial metric.

A diver's face-mask normally produces distortions of both size and distance under water. Due to refraction at the air/glass/water interface, objects are optically located at about three-quarters of their physical distance, and their angular size is enlarged by about 4/3. (For a more precise discussion of the optical effects, see Southall, 1933; Ross, 1970.) If divers perceive objects to be at their optical distance, and if size-constancy operates as in air, objects should not appear enlarged under water. In fact divers normally see objects as beyond their optical distance, and may see them as beyond their physical distance, particularly at far distances or in murky water (Luria *et al.*, 1967; Ross, 1965, 1967, 1968; Kinney *et al.*, 1969). This is probably due mainly to the increased 'aerial perspective' under water, the brightness and colour contrast of objects decreasing more rapidly with distance than they do in air. It may also be due to some immediate compensation for the optical foreshortening. The overestimation of the optical distance seems to account for apparent magnification under water, since apparent size grows with the overestimation of distance approximately (though not precisely) as might be predicted by the size–distance invariance hypothesis (Ross, 1967).

What effect might these changes in apparent size and distance be expected to have upon apparent speed? Velocity equals distance travelled over time, and it seems reasonable to suppose that apparent speed is related to apparent distance travelled over apparent time. Other factors are doubtless of some importance (Brown, 1931; Wallach, 1939), but Rock *et al.* (1968) and Evans (1970*b*) argue convincingly that a relationship of the above type can account for much of the data on speed estimation. This being so, the effect of underwater distortions on apparent speed can be predicted. The effect will vary, depending on whether the object is travelling *across* the line of sight or *along* the line of sight. Since distances across the line of sight normally appear enlarged (i.e. size distortion), it is easy to predict that objects will appear to travel too fast (further in the same time). The distortion along the line of sight is much more complicated, since near distances appear foreshortened (Ono *et al.*, 1970) but far distances appear extended (Luria *et al.*, 1967; Ross, 1967). The change from underestimation to overestimation of physical distance is almost certainly due to the

increasing effect of aerial perspective at far distances, which more than compensates for the optical foreshortening. At close distances then, objects should appear to travel too slowly; but they should appear to speed up in the distance, or in murky water. For simplicity, the present experiment was restricted to near distances in the clear water of a swimming pool: we can therefore predict that apparent speed will be too slow.

A further factor which might complicate speed estimation under water is a change in apparent time. Baddeley (1966) has shown that when divers become cold they tend to underestimate the passage of time (i.e. the time seems shorter than it is). However, this effect was small (about 6 sec. in 1 min.). In so far as it operates, it would tend to make all speeds apparently faster as the diver becomes colder.

It is now well established that divers adapt to some extent to most aspects of the optical distortion during a short period under water. This has been shown for size (Ross *et al.*, 1970), curvature (Ross, 1970), distance (Ono & O'Reilly, 1971), and various combinations of these (Luria & Kinney, 1970; Franklin *et al.*, 1970). On the assumption that apparent speed is a function of the apparent distance travelled, distortions of apparent speed should be reduced along with distortions of size and distance. There is marked individual variation in the extent to which divers perceive size or distance distortion, and in the extent to which they adapt to each aspect – they tend to trade off size and distance distortions against each other, and to adapt mainly to one aspect (Franklin *et al.*, 1970). Since we cannot measure size and distance adaptation at the same time as movement adaptation across and along the line of sight, we cannot say whether these different aspects of spatial perception are related to each other in a precise mathematical manner. We can, however, predict that with a large group of subjects apparent speed *across* the line of sight will initially *increase* under water, followed by some adaptation in water, with an after-effect of *reduced speed* in air. The effects at near distances along the line of sight will be the opposite in all cases.

METHOD

Subjects

The subjects were 40 male volunteers from local schools, swimming clubs and a diving club. Their ages ranged from 13 to 20 years. Most were novices in the use of a face-mask and snorkel. Twenty subjects were tested with movement across the line of sight, and 20 with movement along the line of sight. The subjects were randomly assigned to each condition.

Apparatus

The movement apparatus (Fig. 1) consisted of three basic items: a running track, a moving 'car', and a control box. The running track was a wooden trough 254 cm long, with rail grooves, and a central slit. The moving car was a black box on wheels constructed to fit the track rails. The car was driven by a 24 V. DC motor, and was capable of forward and backward motion. Two metal rods 0·635 cm thick and 91·5 cm long projected vertically from the car, one upwards and one downwards. The upper rod carried the wire leads to the control box, while the lower rod projected downwards through the track's central slit. (Subjects were required to watch this lower rod during the experiment.) The control box provided the power for the apparatus and contained six 9 V. batteries. A dial on the box enabled the experimenter to control the speed of the car.

Five speeds were chosen to cover the range of which the apparatus was capable. These were 21·6 cm/sec., 29·5 cm/sec., 35·1 cm/sec., 41·4 cm/sec. and 50·8 cm/sec. (The unequal intervals result from choosing the marked settings on the control box dial, to enable swift selection.) The

apparatus was checked regularly to ensure that these speeds remained constant (± 3 per cent) throughout the experimental sessions.

The running track was placed diagonally across one corner of a swimming pool (depth about 137 cm), with the lower rod attached to the car half submerged in the water. For the 'across' viewing condition, the subject stood or knelt in the pool, 180 cm from the centre of the track. For the 'along' condition the subject stood or knelt directly under one end of the track, looking towards the other end.

Procedure

The same general procedure was adopted for both the 'across' and 'along' viewing conditions. Four tests were conducted, in the order Air 1, Water 1, Water 2 and Air 2, each test lasting approximately 5 min. There was a 10 min. underwater adaptation period between the Water 1 and Water 2 tests, while the subjects sat on a stool playing Solitaire (a pegboard game, giving practice at eye-hand coordination) and swam around (giving additional movement information, and enabling the subjects to keep warm). Subjects were instructed not to open their eyes above water during this period. The whole experimental session lasted about 30 min. The subjects wore a face-mask during the entire experiment, whether in air or water, and breathed through a snorkel when under water. For the air tests, subjects stood in the pool with their heads above water, and watched the part of the lower rod which was not submerged. For the underwater tests, the subjects knelt or stooped so that their face-masks were submerged, and watched the submerged part of the lower rod. The subjects could, of course, see the car and associated apparatus in the air condition but not the water condition.

Before the first air test, subjects were shown a standard speed in air (35·1 cm/sec.) for six complete runs backwards and forwards of the rod. They were told that this standard speed represented 100 units on an arbitrary scale, and that they were to base all their subsequent speed judgements on their memory of this speed. For each of the four test conditions they were shown the five different speeds twice (once in each direction), the order being randomized within blocks of five. After each complete run of the rod across the track (about 230 cm), the subject was asked to call out a number which he thought represented the speed of the rod, in relation to the standard of 100. The experimenter wrote down the subject's estimates, and gave him no knowledge of results. Though the standard was used as one of the test speeds, it was never again identified to the subject as the standard.

Results

The mean total speed judgements for the four tests for both the 'across' and 'along' viewing conditions are shown in Fig. 2. The results show the predicted trends. On the first water test speeds were judged faster than in air in the 'across' condition, and slower in the 'along' condition. The differences between the first and second water tests went in the opposite direction. On the second air test the expected after-effects appeared, speeds being judged slower than on the first air test in the 'across' condition and faster in the 'along' condition.

The data for both viewing conditions were analysed separately by factorial analyses of variance. Both viewing conditions showed significant main effects ($P < 0{\cdot}001$) for the four test conditions. Further t tests between pairs of means within each viewing condition showed that they all differed significantly from each other at the 0·001 level, except for the Air 1/Air 2 difference in the 'along' viewing condition which was significant at the 0·01 level.

The mean judgements for the five different speeds and four test conditions are shown for the 'across' condition in Fig. 3 and for the 'along' condition in Fig. 4. The speed estimates appear to be approximately linear with true speed over the range of speeds tested. The analyses of variance showed the effect of the different speeds to be signifi-

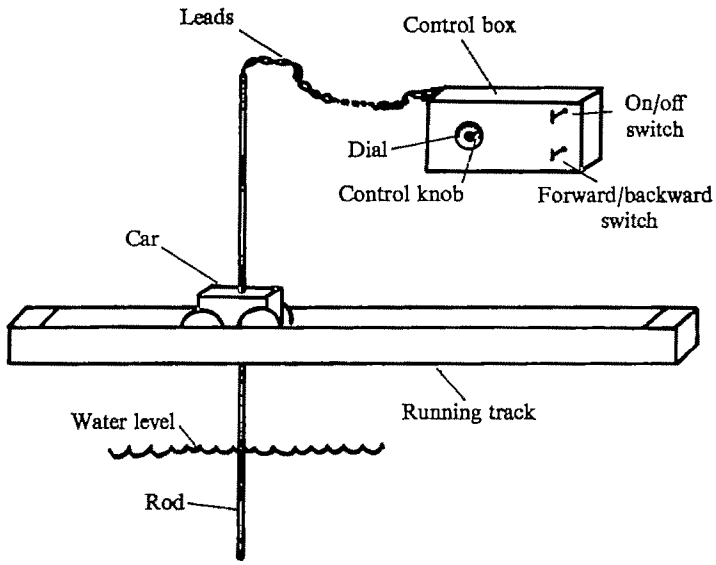

Fig. 1. Movement apparatus (not to scale).

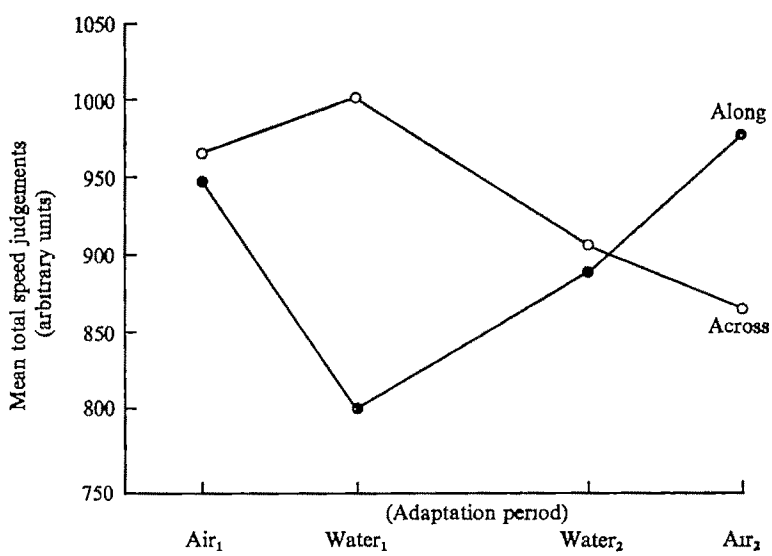

Fig. 2. Mean total speed judgements for movement along and across the line of sight in air and water before and after adaptation.

cant at the 0·001 level for both viewing conditions. In neither case was the interaction between speed and test condition significant.

In some experiments adaptation has been found to occur rapidly within the first few trials in water (e.g. Ross *et al.*, 1970), and similarly the after-effect may fade rapidly (Rock, 1966; Ross *et al.*, 1970). The present data was examined for such trends by comparing the two successive blocks of five trials, but no significant differences were revealed.

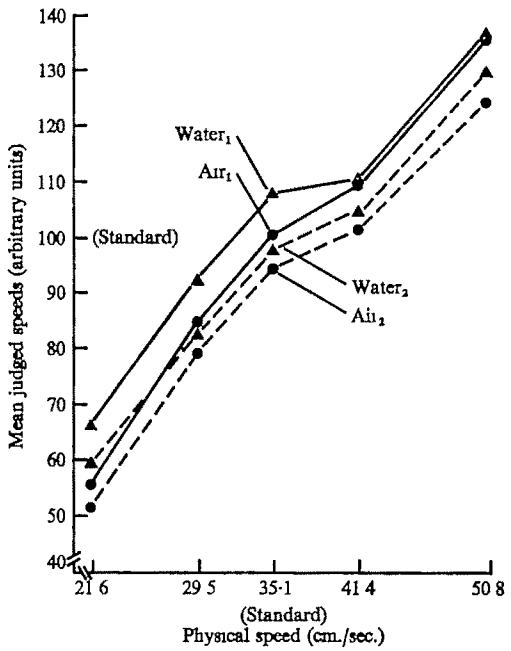

Fig. 3

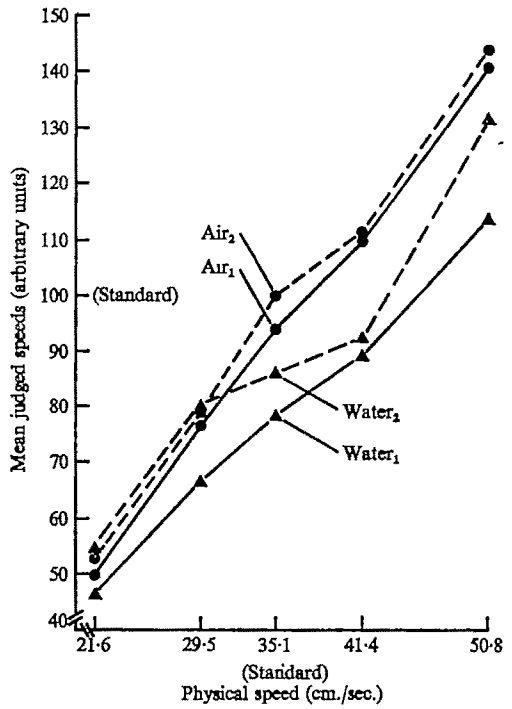

Fig 4

Fig. 3. Movement across the line of sight: mean speed judgements for each air and water condition as a function of the five speeds.

Fig. 4. Movement along the line of sight: mean speed judgements for each air and water condition as a function of the five speeds.

Discussion

The results clearly show the predicted directions of underwater speed distortion and adaptation for the two different viewing conditions. However, the subjects in the 'across' condition showed much less initial underwater distortion than those in the 'along' condition. (The Air 1/Water 1 differences on the total mean judgements in Fig. 2 were 32·5 and 147·6 respectively.) There are no grounds for assuming that the extent of the distortion should be identical. This should occur only if subjects perceive both size and distance to be magnified or diminished by the same factor, which is not usually the case (Ross, 1967). There is considerable variability in the way subjects trade off size distortions against distance distortions, and to which of these distortions they predominantly adapt (Franklin *et al.*, 1970). The present results suggest that most subjects were experiencing a large distance distortion ('along') and very little size distortion ('across'). The after-effects (Air 1/Air 2 differences) suggest relatively little distance adaptation (27·8), but large size adaptation (104·6). The adaptive shifts in water however (Water 1/Water 2), are similar for distance (87·5) and size measurements (96·9). Adaptive shifts and after effects are not necessarily equal in size (e.g. Ross *et al.*, 1970), particularly if there is rapid adaptation during the first water tests, or rapid fading of the after effects. In this case there was no such trend. Our current

research suggests that the relation between the size of the initial distortion in water, the adaptive shift in water and the after-effect in air, is not at all simple. The discrepancies noted here are often found in those adaptation studies which measure adaptation whilst wearing a distorting device, in addition to after-effects. The problem is not unique to movement adaptation or to this particular study.

A further problem in the results for the 'across' condition (Fig. 2) is that the size of the adaptive shift in water (96·9) and the after-effect in air (104·6) greatly exceed the size of the initial distortion in water (32·5). There is thus *overcompensation*, or adaptation of about 300 per cent. This is not impossible to understand, in view of the 'trading' of size and distance distortions. Large adaptation in one dimension is normally accompanied by regression or 'counteradaptation' on the other dimension (Franklin *et al.*, 1970). The subjects in the 'along' condition showed only 19 per cent adaptation on the after-effect, and 60 per cent on the water shift.

A factor which might affect apparent speed under water is the visual background. Brown (1931) showed that increasing the homogeneity of the surrounding field decreased the apparent speed of the object. The sides of the swimming pool under water probably represent a more homogeneous background than the area above water. This would tend to subtract from the expected increase of apparent speed when viewing horizontally under water. It might also enhance the effect of reduced speed when viewing along the line of sight, though background effects are probably less important in this condition. Similarly, it is hard to see how the experimental results could have been affected by the fact that subjects could see more of the apparatus in the air condition than the water condition.

A further factor which might be of some importance is a change in time perception. As mentioned in the introduction, cold would tend to make swimmers underestimate time and consequently overestimate speed. This would have the opposite effect from a homogeneous background, and the effect would tend to increase over the adaptation period as the swimmer becomes colder. However, neither of these factors can account for the adaptive shifts, which operate in opposite directions for the two different viewing conditions. Similarly, any tendency for speed judgements to shift over time cannot explain two different directions of adaptation, and it is logically unnecessary to include a control experiment for time trends.

The method of magnitude estimation used in the present experiment might be criticized on the grounds that it is less objective than an adjustment technique, and raises problems about the subjects' use of numbers. Adjustment techniques are slow to operate (especially with novices under water), and raise additional equipment problems in water. It is important in adaptation studies that judgements should be as quick as possible, and preferably be controlled by the experimenter. The method of magnitude estimation therefore seemed the best solution. This method of speed estimation has been used by several investigators in laboratory experiments, and generally gives power functions with exponents ranging from 0·77 to 1·8 (Ekman & Dahlback, 1956; Eisler & Ottander, 1963; Mashour, 1964; Rachlin, 1966). For practised subjects, as in the case of car drivers estimating the speed at which they are travelling, the exponent may be very near unity (Evans, 1970*a*). In the present experiment the curves appear approximately linear over the range studied, though they would doubtless be better fitted by power functions with exponents less than

unity over a wider range of readings. From the point of view of adaptation studies, the shape of the curve is not strictly relevant, provided it does not change significantly during adaptation.

Speed estimation is a complex operation. In the case of horizontal movement it depends on the rate of movement of the retinal image, the rate of movement of the eyes, head and body of the observer, and the apparent size and distance of the object. In the case of movement along the line of sight, it depends on changes in convergence, changes in the retinal size of the object, changes in its apparent size and distance, and any forward or backward movement of the observer. In both cases the appearance of the background and relative movement of surrounding objects will be important. Movement adaptation and movement after-effects can of course be produced by fixating moving or apparently moving objects (e.g. Anstis *et al.*, 1961; Anstis & Moulden, 1970), but this is not the type of adaptation with which we are concerned here. (Subjects experienced movement in many directions during the adaptation period.) We seem to be dealing with a non-specific type of movement perception, which integrates all types of spatial information, and depends in part on the accurate perception of size and distance. The subjects must have been able to obtain information about the speed distortions under water by the discrepancy between visual and proprioceptive speed when playing the pegboard game or swimming. At the same time, of course, they may receive information about size, shape and distance distortions from discrepancies in eye–hand coordination, and from visual deformations during head and eye movements. All aspects of the optical distortion are closely interrelated, and so are the possible sources of information for detecting the discrepancies. It seems likely that the brain deals with all possible sources of information, whilst trying out various ways of restructuring the visual metric to minimize the different types of errors which arise. It is just possible that adaptation to speed distortion across the line of sight can occur independently from size adaptation; and to speed distortion along the line of sight independently from distance adaptation. (Adaptation might occur in the time sense rather than the spatial sense.) However, it seems more likely that speed and space perception are closely linked, and that the same cues will cause adaptation to both. Without great refinement of experimental technique we cannot say whether any cue or any aspect of the distortion is of primary significance.

The speed distortions noted here are not unique to underwater vision. They also occur with image magnification or minification in driving and flying. Roscoe *et al.* (1966) have shown that pilots can learn to cope quite well with the equivalent distortions in landing and takeoff.

We should like to thank Mr G. Reid and the pupils of the Queen Victoria School, Dunblane; Mr N. Murphy and the pupils of St Modan's High School, Stirling; and the members of the Forth Valley Sub-Aqua Club who took part in the experiment. We should also like to thank Mr R. Macdonald for statistical and computing assistance. This work was supported by a grant from the Medical Research Council.

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(*Manuscript received* 18 *January* 1971;
revised manuscript received 26 *May* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 265–270
Printed in Great Britain

VERTICAL DISPARITIES

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It has been suggested that stereopsis could be explained on a physiological and neuro-anatomical basis. However, evidence on depth perception tends to rely on transversely disparate inputs. It was thought that if the explanation lies in the structures of the eye, vertical disparities might result in a depth perception also. The evidence obtained is in favour of this hypothesis but appears to contradict the assumption of different depth organizations depending on crossed or uncrossed visual pathways.

The literature regarding stereopsis seems to suggest that depth, as the apparent difference in 'nearness' and 'farness', can be explained by physiological structures which are largely innately determined. For instance, Ogle (1963) states that 'stereopsis appears to be a unique experiential response, a sensation in its own right, and not subservient to some other visual process. Its basis seems to be innate and stable'. If one concurs with this assumption one would expect that all retinal elements within the areas of fusion would have, potentially at least, equal capacity to contribute to a perception of depth. That is, although transversely disparate retinal elements would on the basis of everyday experience have an enhanced capacity to generate a depth perception, nevertheless vertically disparate elements should be able to induce the same sensation. That this was the case was shown by Ogle (1955), who used two light points, one above the other, and found that both his subjects reported stereoscopic depth. Ogle (1963) also discusses Hering's theory of local depth signs in which it is proposed that depth values, that is perception of 'farness' or 'nearness', is determined by the retinal areas involved, nasal parts of each retina having 'far' values and temporal parts having 'near' values.

It was thought that Ogle's (1955) data on stereoscopic depths on the basis of vertical disparities should be replicable on a larger number of subjects by using Julesz's (1964) method of random dot patterns and that Hering's notion of local depth signs should be testable in the process.

Method

A series of computer (ICT 1909) generated random dot patterns was prepared. The printed output of this computer was on 60×60 lines. The print density was 0·45, i.e. 45 per cent of the page consisted of stimulus points and 55 per cent of the page were the blank spaces in between. When the basic pattern was obtained subsequent patterns contained the following modification. The dots in the middle rectangle of 20×20 lines were displaced by two lines as well as by 5°, 10°, 15° and so on in steps of 5° up to 90° geometrically from the horizontal plane. In this way a series of outputs was generated which, as compared with the basic random dot patterns, contained, apart from horizontal disparity, also a vertical disparity factor as a consequence of the rotation.

Photographic negatives were made of the computer outputs such that the visual angle subtended by the disparate area in the display was approximately 9° horizontally on each side of the centre of the fovea and vertically approximately 8° above and below the centre of the fovea.

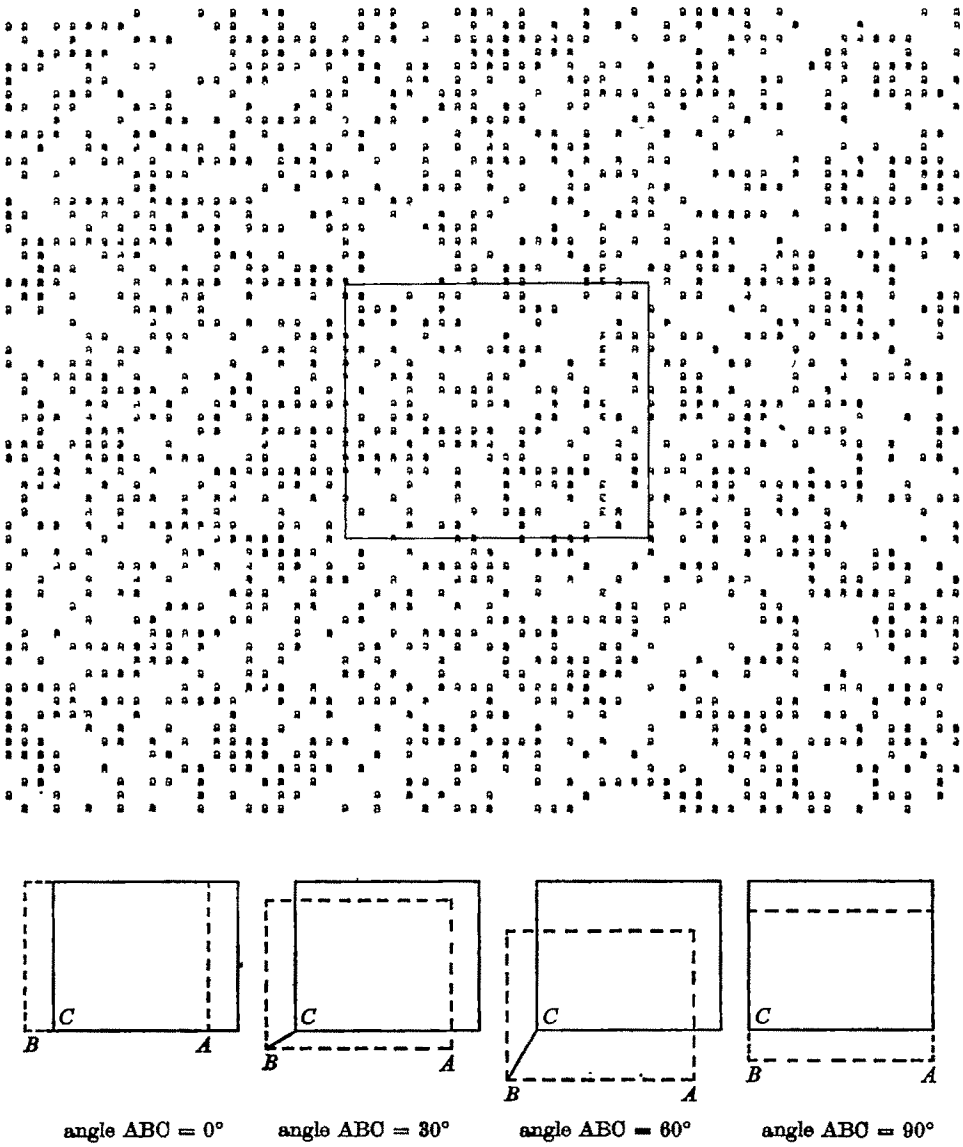

Fig. 1. Illustration of the basic stimulus, the middle portion of which was displaced over two lines of elements as well as rotated as shown in the lower diagrams where the broken-line rectangles represent the basic stimulus and the solid-line rectangles the displaced stimulus. Every picture element within the middle portion was displaced as indicated by these stimulus outlines, the distance *BC* being two lines of elements.

The visual angle subtended by each pair of disparate points was approximately 25°. A synoptophore, an elaborate type of stereoscope (Clement Clark 2053), was used for the presentation. A green filter for one eye and a red one for the other eye were placed before the stimuli in the synoptophore since this reduced considerably the time required for fixation until fusion occurred, possibly through brightness disparity.

Procedure

Subjects were requested to fixate a group of dots in the middle of the display and to report changes in the display if and when these occurred.

The slides were presented as follows: the basic random pattern slide was presented to the same eye for each subject (eyes randomized over the subjects) and the rotated dot patterns to the other eye – in ascending order of rotation from 0° to 90°. Those subjects who reported depth at 90° were then given the same set in descending order of rotation but the slides were reversed for this series, i.e. they were given to the same eye as before but left–right reversed. This procedure resulted in a temporal disparity for the ascending series and a nasal disparity for the descending series. After the descending series had been completed, the set with nasal disparity was again presented after a 5 min. rest, commencing from 0° rotation ascending to increasing degrees of rotation. Twelve students and staff from the School of Social Sciences at Bradford University were used as subjects.

Results

The results are given in Tables 1, 2 and 3.

Discussion

The results obtained confirm those of Ogle (1955) in that all subjects report depth when some vertical disparity is present, whilst half the subjects continue to report depth up to disparities of 90° from the horizontal plane. This depth sensation was reported as quite clear and distinct, the middle portion of the total field standing well in front of the remainder. The shape of this middle portion varied from a rectangle to a shape approaching that of a diamond and back again to a rectangle. This change in shape would seem a predictable consequence of the fusion of two inputs when the defining angles of each separate input do not coincide with those of the other input. That is, the boundary for the fused input would presumably be determined by the

Table 1. *Temporal disparities*

	S1	S2	S3	S4	S5	S6	S7	S8	S9	S10	S11	S12
0°	+	+	+	+	+	+	+	+	+	+	+	+
5°	+	+	+	+	+	+	+	+	+	+	+	+
10°	+	+	+	+	+	+	+	+	+	+	+	+
15°	+	×	+	+	+	+	+	+	+	+	+	+
20°	+	×	+	+	U.S.	+	+	+	+	U.S.	+	+
25°	U.S.	×	+	+	U.S.	+	+	+	U.S.	×	+	+
30°	U.S.	.	+	+	U.S.	U.S.	+	+	U.S.	×	+	+
35°	×	.	+	+	U.S.	U.S.	+	+	U.S.	×	+	+
40°	×	.	+	+	U.S.	U.S.	+	+	U.S.	.	+	+
45°	×	.	+	+	×	×	+	+	×	.	+	+
50°	.	.	+	+	×	×	+	+	×	.	+	+
55°	.	.	+	+	×	×	+	+	×	.	+	+
60°	.	.	+	+	.	.	+	+	.	.	+	+
65°	.	.	+	+	.	.	+	+	.	.	+	+
70°	.	.	+	+	.	.	+	+	.	.	+	+
75°	.	.	+	+	.	.	+	+	.	.	+	+
80°	.	.	+	+	.	.	+	+	.	.	+	+
85°	.	.	+	+	.	.	+	+	.	.	+	+
90°	.	.	+	+	.	.	+	+	.	.	+	+

+ refers to a depth perception being reported.

× refers to no depth, i.e. a flat image, being reported.

U.S. means an unstable image was obtained in which the middle square, although different to the surrounding area, had no unitary depth quality.

six angles of the two superimposed rectangles as the illustration shows for the cases where angle ABC is 30° and 60°. This diamond shape tended to have a rather 'ragged' boundary as distinct from the rectangular shape which had clear-cut boundaries. Although half the subjects did not report depth up to 90° of rotation of one of the inputs which may have been due to optical differences between their two eyes or to eye preference, this does not invalidate the finding that the other half of the subjects experienced stereopsis up to complete vertical disparity. That the subjects were not reporting binocular rivalry of the central portions of the input as compared with the binocularly fused background, is evident from the report of the changing shape of the

Table 2. *Nasal disparities*

	S1	S3	S4	S7	S8	S11	S12
90°	+	+	+	+	+	+	+
85°	+	+	+	+	+	+	+
80°	+	+	+	+	+	+	+
75°	+	+	+	+	+	+	+
70°	+	+	+	+	+	+	+
65°	+	+	+	+	+	+	+
60°	+	+	+	+	+	+	+
55°	+	+	+	+	+	+	+
50°	+	+	+	+	+	+	+
45°	+	+	+	+	+	+	+
40°	+	+	+	+	+	+	+
35°	×	+	+	+	×	+	+
30°	×	+	+	+	×	+	+
25°	×	+	+	+	×	+	+
20°	×	+	+	+	×	+	+
15°	×	+	+	+	×	+	+
10°	+	+	+	+	×	+	+
5°	+	+	+	+	×	+	+
0°	+	+	+	+	×	+	+

S3, 4, 7, 11 and 12 reported the middle shape in *front* of the background. × for S1 and S8 indicates that although the middle of the display appeared to be different, the actual plane of depth effect was ambiguous. S1 reported at 10° a switch from the ambiguous middle of the display to a clear depth effect with the middle behind the remainder of the display.

Table 3. *Nasal disparities, commencing from* 0° *rotation* (*after a* 5 *min. rest following the preceding series*)

	S1	S3	S4	S7	S8	S11	S12
0°	+	+	+	+	+	+	+
5°	+	+	+	+	+	+	+
10°	+	+	+	+	+	+	+
15°	×	×	×	×	+	×	+
20°	×	×	×	×	+	×	+
25°	×	×	×	×	+	×	+
30°	×	×	×	×	+	×	+
35°	+	+	×	+	+	×	+
40°	+	+	+	+	.	+	.
45°	+	+	+	+	.	+	.
50°	+	+	+	+	.	+	.

S8 and S12 reported the middle shape in front of the main display for all pairs of slides. The other subjects reported the first few shapes as distinctly behind the main display. Then followed a number of reports like 'middle is blurred, messy or ambiguous' or 'there appear two planes but it is mushy with some dots in front and some behind main plane'. Round about 35–40° of rotation these subjects again reported the middle shape well in front of the main plane.

centre of the input. Had binocular rivalry for that part of the stimulus field occurred then the shape of this central portion would have been rectangular in shape throughout the experiment which was definitely not the case. Ogle (1955) also found differences between his two subjects, one reporting doubling of images early in the experiment and the other only reporting this when the fusional limits of the vertical disparities were approached.

A more surprising finding was that a reversal to nasal disparities did, for the majority of subjects, not result in a shift from 'figure in front' to 'figure behind'. Only two of the subjects (S 1 and S 8 in Table 2) reported ambiguity as to the plane of the middle of the display in this series and one of these reported the actual shift to 'figure behind' for the last few trials. This could possibly be explained by an attentional factor similarly as in Julesz's (1964) experiment where ambiguous stereo pairs tended to be perceived similarly as the previously presented unambiguous stimulus pair. Julesz suggested that his findings implied that the first stimulus served as a 'depth marker' and determined which of the possible depth organizations should be attended to. He did, however, suggest that an attention time of about 50 msec. was critical for this dependence of the perception of the second stimulus on the apparent plane of the first stimulus, such that if the second stimulus was delayed for a longer interval than the attention time, the plane of perceived depth of the second stimulus was no longer determined by that of the first stimulus. Such a short interval between stimuli did not apply in the present experiment where the slides required manual changing. In this experiment, when the different stimuli were given in a series in which the object had been perceived in front from the beginning an explanation in terms of attentional set seems less likely if the time interval between stimuli is critical, which it need not be. Expectancy or some form of learning on the part of the subjects could also have been instrumental in the same depth plane being reported. However, one would have expected a change to 'object behind' after the subjects had had a rest away from the task and the stimuli were presented with 0° rotation and nasal disparity. This in fact occurred for five of the seven subjects for the first three presentations (i.e. 0, 5 and 10° rotation). For further presentation with increasing rotation these five subjects reported a shape in the middle of the field which was ambiguous as to its plane. This ambiguity disappeared for these subjects when the rotation was increased and all these subjects reported 'figure in front' when the rotation was increased to 40° and over. Two subjects reported that the shape in the middle appeared in front from the beginning of the series.

It would appear that nasal disparities do not necessarily result in depth being perceived in a different plane. Therefore Julesz's notion of 'depth marker' may not have an explicit neurophysiological correlate but rather be determined by learned aspects of depth perception. In any case, a pair of stimuli which contain a nasal disparity do so in a relative way only. That is, in the case of these stereoscopically presented random dot patterns the direction of the disparity depends on which one of the pair is taken as the reference.

It seems then that the portion of the retina which is served by crossed fibres is not functionally different from the portion served by uncrossed fibres, although Ogle (1963) was quite explicit about the existence of such a functional difference. If the present contention is correct then further doubt is thereby thrown on the validity of

Hering's theory of local depth signs. Ogle (1963), in presenting this theory, points out that Hering's theory has not been without its critics. To these critics can be added the evidence of Julesz (1964), who also considers that his evidence discredits Hering's theory, as well as the present data which contradict the assumption of retinally determined depth values.

The present data would seem to fit in with Hubel & Wiesel's (1970) findings in the macaque monkey which seem to indicate the existence of binocular depth cells in area 18 with varying orientations, oblique fields also containing a vertical component to the disparity.

In conclusion it can be said that vertical disparities can give rise to stereopsis but that the actual value of the depth perceived does not necessarily depend on the specific retinal area involved. The latter may possibly be better explained by factors relating to everyday experience rather than neuro-anatomical structures such as crossed and uncrossed nerve fibres.

My thanks are due to Miss M. Holdaway, of the Computer Department of Bradford University, for producing the computer programs for the random dot patterns.

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(*Manuscript received* 30 *October* 1970; *revised manuscript received* 23 *April* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 271–282
Printed in Great Britain

THE DEVELOPMENT OF VISUAL AND KINAESTHETIC JUDGEMENTS OF DISTANCE

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Three groups, each of 36 children, aged 4, 6 and 8 years, performed visual and kinaesthetic intra- and cross-modal matching tasks in three experimental conditions of simultaneous or successive visual inputs in an age × conditions × modality design. Stimuli were lengths with specific relations to a total distance.

No effect of simultaneous versus successive experimental conditions was found. Age, modality and the interaction between age and modality were significant effects. For 8- and 6-year-olds highest errors were on the kinaesthetic–visual (K–V) task; for 4-year-olds visual–kinaesthetic (V–K) and intra-kinaesthetic (K–K) errors were significantly higher than all others. To test if differences in error patterns could be explained by poor movement control by 4-year-olds, data for subjects selected on intra-kinaesthetic performance were subjected to a second analysis.

An age × modality analysis on subjects selected on K–K showed that age, modality and the interaction between age and modality were again significant effects. K–K errors for the 4-year group were now significantly lower than V–K errors, and not different from K–V. V–K errors had not been affected by the selection on K–K and were significantly higher than V–V, and than K–V errors. The cross-modal asymmetry (V–K > K–V errors) was thus not due to kinaesthetic response errors. K–V was only slightly and not significantly higher than V–V at the 4-year level. This error pattern contrasted with those found for 6- and 8-year-olds where K–V, not V–K errors, were highest and significantly different from V–V errors. The implications of the results for theories of cross-modal performance are discussed, and an explanation is suggested.

Recent literature on cross-modal performance shows an interesting discrepancy in the error patterns found for visual and kinaesthetic judgements of length, and visual and haptic (active touch) shape matching, respectively. Studies of visual and kinaesthetic length judgements, under balanced intra- and cross-modal conditions, have most frequently found intra-modal judgements superior to cross-modal, and larger error scores for visual–kinaesthetic than for kinaesthetic–visual judgements. This pattern was found for children between 5 and 11 years of age (Connolly & Jones, 1970) and for adults (Legge, 1965; Kress & Cross, 1969; Connolly & Jones, 1970).

On the other hand, investigations using similar balanced designs to compare visual and haptic matching of nonsense shapes (without prior training) have generally found one of two results: no difference between the two cross-modal tasks or between these and the intra-haptic task (Milner & Bryant, 1970; Millar, 1971, for children; Garvill & Molander, 1969, for adults); or, in so far as differences have been found, visual–haptic rather than haptic–visual performance was superior (Rudel & Teuber, 1964; Milner & Bryant, 1970, at 0 sec. delay, for children; Cashdan, 1968; Garvill & Molander, 1969, for adults). The discrepancy in the findings for distance judgements and shape matching, especially the apparent reversal of cross-modal asymmetry, is puzzling as kinaesthetic cues are presumably involved in haptic tasks. Task and stimulus differences, and the differences in cognitive processing these involve, may account for the discrepancy. But so far there is little experimental evidence to show whether or how these operate to change cross-modal performance. The present study

asks the question whether, and under what conditions, the pattern of errors found previously for visual and kinaesthetic judgements of lengths may be altered, and if so, how this relates to performance at different age levels.

Two conditions are considered. First, visual input has the advantage over haptic and kinaesthetic input of being simultaneously present, and having anchoring points in the visual surround, while haptic and kinaesthetic information is given successively. Krauthamer (1968) found tactual to visual shape matching by adults superior if immediately preceded by tactual training, but not when visual information was given by successive light-points rather than by viewing the figure as a whole. The question is therefore whether integration of successive and simultaneous information, rather than integration of cues from different modalities, is implicated in the cross-modal asymmetry of distance judgements. Simultaneous versus successive visual conditions were therefore to be tested.

Secondly, differences in cross-modal performance may be due to differences in the extent to which higher order processing (in Gibson's sense), or coding strategies can be used in different tasks. Gibson (1969) has argued that intersensory errors occur in so far as stimulus features are processed differently in the two modalities, but that accurate intersensory matching depends upon the pick-up of higher order relational information common to several modalities. This is more likely to be provided by multidimensional than by unidimensional stimuli. Studies of length judgements have generally used a number of equidistant lengths along a continuum without an end or midpoint to which a given length could be related. Adequate coding of the input may, therefore, have been much less possible than in matching multidimensional shapes. If so, then increasing the possibility of coding lengths in absolute terms, or providing information to which the input can be related, should make cross-modal performance more accurate. The question is whether this affects visual–kinaesthetic and kinaesthetic–visual errors equally. It is assumed that young children will be less likely than older children to make use of additional relational information; for instance, information which allows them to code a given length in relation to a total distance as 'one quarter of the total distance'. A change of error pattern with differences in information pick-up should therefore show up with age. The lengths to be used were, consequently, to have specific, although not too easily labelled, relations to a total distance.

A reasonably wide age range was to be tested. A pilot study on 2-year-olds had shown them to respond with impulsive ballistic movements regardless of the length of the standard. Two-year-olds were therefore not included in the study. The subjects were chosen from nursery, infant and junior classes.

METHOD

Subjects were tested at three age levels, allocated randomly to one of three experimental (simultaneous or successive visual input) conditions as follows: (1) successive presentation; (2) successive presentation with guide lines; (3) simultaneous presentation. Age and experimental conditions were between subject-factors.

Each subject was tested under four modality conditions as follows: (1) visual standard – visual judgement (V–V); (2) kinaesthetic standard – visual judgement (K–V); (3) visual standard – kinaesthetic judgement (V–K); (4) kinaesthetic standard – kinaesthetic judgement (K–K). The order of modality conditions was varied randomly for each subject. Modality was a

within-subject factor at three levels (V–V, K–V, V–K), with K–K as a within-subject control condition for a $3 \times 3 \times 3$ analysis of variance (analysis I).

The data were subjected to a second analysis using subjects selected in intra-kinaesthetic performance (analysis II).

Material

Lengths of 2, 4 and 6 in. and a total distance of 7 in. were chosen. Each of the three lengths bears a definable proportionate relation to 7 in. With a midpoint of $3\frac{1}{2}$ in., the 4 in. length is larger than this by half the distance that the 6 in. length is from the end point. The 2 in. length is twice that distance from the starting point. The lengths could be differently 'tagged' therefore, but easy verbal labels ('small', 'middle', 'large') would be insufficiently precise for accurate reproduction.

Each of the three lengths was presented twice in each of the modality conditions, the order for each subject being determined by random numbers.

Apparatus

The apparatus consists of two joysticks, connected at a distance of $18\frac{1}{2}$ in. by two horizontal bars, so that any movement by one is also made by the other. Visual input to the subject is from a $\frac{3}{8}$ in. diameter bulb fitted to the top of the subject's stick (total height $6\frac{1}{4}$ in.). For kinaesthetic input the subject grasps the $\frac{3}{8}$ in. diameter joystick between thumb and fore- or middle finger below the bulb. A $\frac{2}{10}$ in. circular plate, fitted at $5\frac{1}{2}$ in. on the joystick, prevents the subject from grasping the stick at any point lower than this.

Two circular potentiometers, fitted to the experimenter's joystick, drive the X and Y inputs of a Hewlett Packard 7035 B X–Y recorder through zeroing and scaling circuits. The direction and extent of the movements of the joysticks are directly transcribed on to replaceable graph paper on the X–Y recorder. (The servo switch on the recorder can be turned off to prevent recording and noise from the apparatus during presentation.)

The apparatus is housed in a box $28 \times 15 \times 15$ in. The sloping front panel has a viewing aperture ($8\frac{7}{8} \times 2$ in.), fitted with a black Perspex shutter, operated from the experimenter's side. The light on the joystick, itself clearly visible in all positions, does not illuminate any part of the box. A blackout curtain divides the subject's from the experimenter's part of the apparatus. At 1 in. below the viewing aperture, the open 9×13 in. panel is fitted with a loosely gathered sleeve which fits over the subject's wrist with soft elastic and allows free movement but prevents viewing. During visual conditions the subject's hand rests on the baseboard out of contact with any part of the joystick.

A ledge, fitted at a height of 7 in. inside the experimenter's end of the apparatus, bears a metal plate which restricts movements to 7 in. in the horizontal plane.

Subjects

Subjects were 108 children from three local authority nursery schools, and from the two local authority infant/junior schools to which these nursery school children normally graduate. The schools are in predominantly working class (skilled and semi-skilled) areas. The proportion of children of foreign-born parents in the sample was close to the official estimate, and similar for the three age levels and conditions.

There were 36 children at each of three age levels: mean age of 8·0 (range 90–105 months); mean age of 6·0 (range 64–78 months) and mean age of 4 years 1 month (range 39–55 months). An equal number of boys and girls from each class-list were allocated randomly to the three experimental conditions. A further 19 nursery school children were tested under the same conditions. These were drawn upon for a second analysis, based only on subjects with relatively low intra-kinaesthetic error scores (see below).

Procedure

Subjects were tested singly in an empty room of their school. The experiment was explained as a game of playing pilots with the apparatus as the pretend 'plane. The subject was seated on a chair in front of the apparatus, when necessary elevated by a cushion to ensure that his forearm was approximately at right angles to his body when grasping the stick. Children were encouraged

to use their right hand, but were allowed to use the left if they preferred. Under these conditions only 13 children in all used the left hand. It is unlikely that such a small percentage could seriously distort the results. All movements were from left to right. Trial runs with 7, 3½ and 1 in. lengths followed. This procedure was repeated before each change in modality condition. Testing was completed in one session. All the children greatly enjoyed the game.

Presentation of the standard was at the rate of 1 in./sec. followed by a delay of 3 sec. during which the joystick was returned to starting point, and the subject grasped or released the joystick as appropriate. The viewing window was open in all V, and shut in all K conditions.

Commands 'watch' ('feel it move') signalled the start of presentation. The three successive or simultaneous visual input conditions (experimental conditions) differed only in the presentation of the visual stimuli in visual standard and visual response conditions. Kinaesthetic standard and response conditions were the same for all three conditions as follows:

Condition I: successive. V–V: V – standard: Input from lighted bulb on joystick moving to required distance. V – response: Input from moving lighted bulb on joystick. The subject stops it by saying 'stop'. K–K: K – standard: The subject grasps joystick and executes movement to right. The experimenter controls speed and stop of the movement via the experimenter's joystick. K – response: The subject moves joystick to required length. V–K: V – standard: Input as for V–V standard. K – response: As for K–K response. K–V: K – standard: As for K–K standard. V – response: As for V–V response.

Condition II: successive with guide-lines. V–V, K–K, V–K and K–V conditions exactly as in condition I, except that in all V standard and response conditions the apparatus is lit by a houselight and a white cardboard prop bearing a 7 in. black, horizontal, bisected line is visible at a height of 6 in. behind the joystick. This cannot be touched (or of course seen) in K–K conditions.

Condition III: simultaneous. (The X–Y recorder, screened from the subject in all other conditions, is placed level with the apparatus, slightly to the left of the subject's midline in good viewing position, adjusted for each child.) V–V: Visual standard: Lengths, drawn in green ink on white paper pasted on to 8 × 10 in. cardboard. Presentation is above the apparatus in an upright position for 4 sec. Visual response: The pen of the X–Y recorder traces a line on a plain white (8 × 10 in.) sheet until the subject says 'stop'. K–K: As for condition I. V–K: V – standard: As for V–V standard (III). K – response: As for K–K response (I). K–V: K – standard: As for K–K standard (I). V – response: As for V–V response (III).

It should be noted that in K–K conditions, the speed and stop of K standard movements are guided by the experimenter to make the K standard like the V standard in which speed and stop are necessarily controlled by the experimenter also. However, the experimenter only controls speed and stop. The subject moves the joystick himself, subject only to this guidance. The standard K–K movement is therefore unlike Paillard & Brouchon's (1968) passive levering of a weighted limb reproduced by the other arm.

RESULTS

Analysis I

The results were subjected to a 3 × 3 × 3 analysis of variance with age (OS, MS, YS), experimental conditions (I, II, III) and modality (V–V, V–K, K–V) as variables. Modality was a within-subject factor. Calculations were based upon absolute errors, measured in 0·1 in. units: age ($F = 38{\cdot}5$; d.f. = 2, 99; $P < 0{\cdot}001$), modality ($F = 19{\cdot}4$; d.f. = 2, 198; $P < 0{\cdot}001$), and the interaction between age and modality ($F = 6$; d.f. = 4, 198; $P < 0{\cdot}001$) were significant effects. With reduced degrees of freedom on the conservative criterion for repeated measures (Winer, 1962), modality was significant at $P < 0{\cdot}001$, and the age × modality interaction was significant at $P < 0{\cdot}005$. There was no effect of experimental conditions, nor was there any significant interaction between either age and experimental conditions, or modality and experimental conditions, respectively. The reasons for this lack of effect are not entirely clear. There may have been confounding variables in the experimental set-up,

or successive versus simultaneous input conditions are not effective factors for length judgements, or for the age levels tested.

Since there was no evidence of any difference between the three experimental conditions, these were collapsed and compared with K–K. Mean errors are shown in Table 1.

Modality effects were significant at the $P < 0{\cdot}05$ level for the 8-year as well as for the 6-year groups. Modality effects were also significant within each age group on F tests which included intra-kinaesthetic tasks. It is worth noting that for both the 6-year and the 8-year group it was K–V rather than V–K which showed the highest error scores. It was significantly different from V–V at the $P < 0{\cdot}05$ level on Newman–Keuls (Winer, 1962, p. 309) test for each group. V–K and K–V were not significantly different from each other, or from K–K. For both the older groups, therefore, the asymmetry between V–K and K–V, previously reported, was here abolished. The effect was, indeed, to some extent reversed, since it was K–V rather than V–K which showed the highest errors and was significantly different from V–V.

For the youngest group the significant modality effect ($P < 0{\cdot}001$) showed a different pattern. On Newman–Keuls test V–K had significantly more errors than V–V ($P < 0{\cdot}01$) and K–V ($P < 0{\cdot}01$). The cross-modal asymmetry reported in previous work was here replicated. It should be noted, however, that V–K was not significantly different from K–K, while the latter differed from V–V ($P < 0{\cdot}01$) and from K–V ($P < 0{\cdot}01$) in the same way as did V–K; V–K was thus similar to K–K in all respects.

The divergence of effect was not, as might be expected, between intra-modal and cross-modal performances, but between tasks requiring visual judgements and those requiring kinaesthetic judgements.

A possible explanation was that some subjects in the youngest group responded by making impulsive ballistic movements like the 2-year-olds in the pilot study. This would affect K–K and V–K. The results might, consequently, merely reflect better movement control at older age levels. To test this, a second analysis was performed on data excluding subjects with poor or erratic K–K performance.

Analysis II

All subjects whose K–K records showed evidence of impulsive or erratic responding were dropped from the analysis. The criterion was: little or no difference between responses to different standard lengths. (In practice, these subjects had the highest

Table 1. *Mean absolute errors measured in* 0·1 *in. units and standard deviations for analysis I*

Mean age		Modality conditions: Visual–visual	Kinaesthetic–visual	Visual–kinaesthetic	Kinaesthetic–kinaesthetic
4·1	Mean	11·3	14·5	22·4	23·2
	S.D.	5·1	6·2	9·6	9·6
6·0	Mean	8·6	12·7	11·9	11·0
	S.D.	4·9	10·5	6·4	4·6
8·0	Mean	6·5	9·8	9·3	8·3
	S.D.	4·4	5·5	3·6	4·5

error scores on K–K. No account was, of course, taken of responses on any other modality task.) At the 8-year and at the 6-year level four subjects, respectively, fell into this category. At the 4-year level, 11 subjects had to be excluded. Of the 19 subjects additionally tested at this age level (see Subjects) 10 had acceptable intra-kinaesthetic records. These subjects were included in the second analysis: a total of 32 subjects at each of the three age levels. Mean ages and age ranges for the 8-year and 6-year groups remained as before. The newly constituted 4-year group had a mean age of 4 years 2 months (range 41–56 months). No evidence of an effect of experimental conditions had been found for this group either. Error scores were therefore collapsed over conditions.

As expected, the selection reduced the mean error on K–K at the 4-year level. The means for the three age groups on this task were now much more in proportion (see Table 2). V–V errors were not significantly affected by the selection at any of the three age levels.

An age × modality analysis of variance (3 × 4), with repeated measures on the last factor, showed age ($F = 25{\cdot}9$; d.f. = 2, 93; $P < 0{\cdot}001$), modality ($F = 9{\cdot}0$; d.f. = 3, 279; $P < 0{\cdot}001$; conservative criterion: d.f. = 1, 93; $P < 0{\cdot}01$) and the age × modality interaction ($F = 3{\cdot}6$; d.f. = 6, 279; $P < 0{\cdot}01$; conservative criterion: d.f. = 2, 93; $P < 0{\cdot}05$) to be significant effects.

The simple effect of age was significant at the 0·001 level for each of the four modality conditions.

The simple effects of modality were significant at the 0·05 level for the 8-year group, and between the 0·05 and 0·025 levels for the 6-year group. The patterns of scores for the two older groups remain as in the original analysis with K–V errors significantly higher than V–V errors for the 8-year ($P < 0{\cdot}05$) and 6-year group ($P < 0{\cdot}05$) on Newman–Keuls tests.

The simple modality effect for the 4-year group was significant at the 0·001 level. Newman–Keuls tests showed K–K errors to be significantly higher than V–V errors but at a reduced level ($P < 0{\cdot}05$), and no longer significantly different from K–V. The difference between K–V and V–V failed to reach the 0·05 level of significance. But despite selection on K–K, V–K errors remained significantly higher than K–V ($P < 0{\cdot}01$) and V–V errors ($P < 0{\cdot}01$), but were also now significantly higher than K–K scores ($P < 0{\cdot}01$). The cross-modal asymmetry (V–K > K–V) on this analysis cannot, therefore, be attributed solely to poor movement control or kinaesthetic

Table 2. *Mean absolute errors measured in* 0·1 *in. units and standard deviations for subjects selected on K–K (analysis II)*

Mean age		Modality conditions: Visual–visual	Kinaesthetic–visual	Visual–kinaesthetic	Kinaesthetic–kinaesthetic
4·3	Mean	12·1	13·6	19·1	15·3
	S.D.	4·9	5·5	8·7	6·5
6·0	Mean	8·5	12·6	10·2	9·9
	S.D.	5·1	11·1	3·8	3·3
8·0	Mean	6·2	9·9	8·9	7·1
	S.D.	4·3	5·5	3·6	2·3

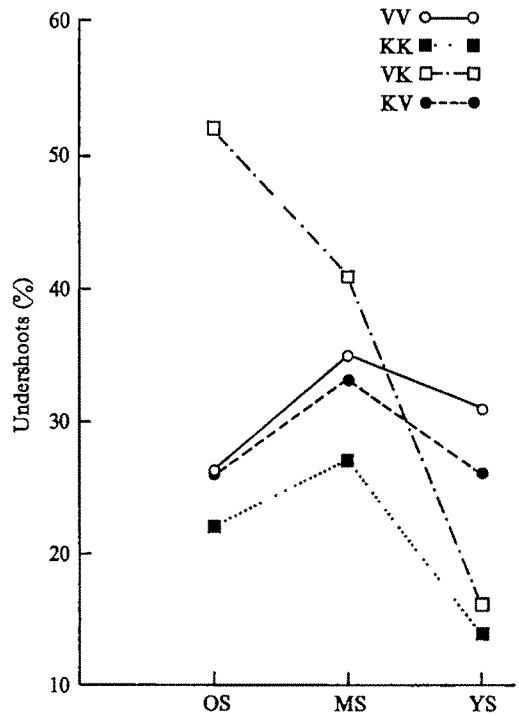

Fig. 1. Percentage of undershooting scores, regardless of error size, for visual–visual (VV), kinaesthetic–visual (KV), visual–kinaesthetic (VK), kinaesthetic–kinaesthetic (KK) conditions at age levels 4 years 1 month (YS), 6 years 0 months (MS), 8 years 0 months (OS) (analysis I).

response errors as such. Reduction of V–K errors clearly does not necessarily follow upon reduction of K–K errors.

The age × modality interaction for subjects selected on K–K thus represents a change in cross-modal performance which cannot be attributed solely to disproportionate errors on K–K at the youngest age level. The main contribution came from V–K which had significantly higher errors than any other task at the 4-year level, but was not significantly different from either K–K, K–V or even V–V for the 8-year and 6-year groups. K–V, which differed only slightly from V–V at the 4-year level, but was significantly different from V–V and showed the highest errors at both the 8-year and the 6-year levels, also contributed.

Proportion of undershoots

The manner in which patterns of errors are achieved is of interest. 'Response sets' of undershooting or overshooting give some indication of this (Pepper & Herman, 1970). For a meaningful comparison of the tendency to over- or undershoot between different age groups and across different lengths, the size of the error is a confounding factor. The proportion of undershoots to overshoots, regardless of error size, was, consequently, computed for each age and condition. This method obviates the difficulty that 'room' for undershooting is less for the 2 in. than for the 6 in. lengths, since only the positive or negative signs of an error, not its size, are taken into account.

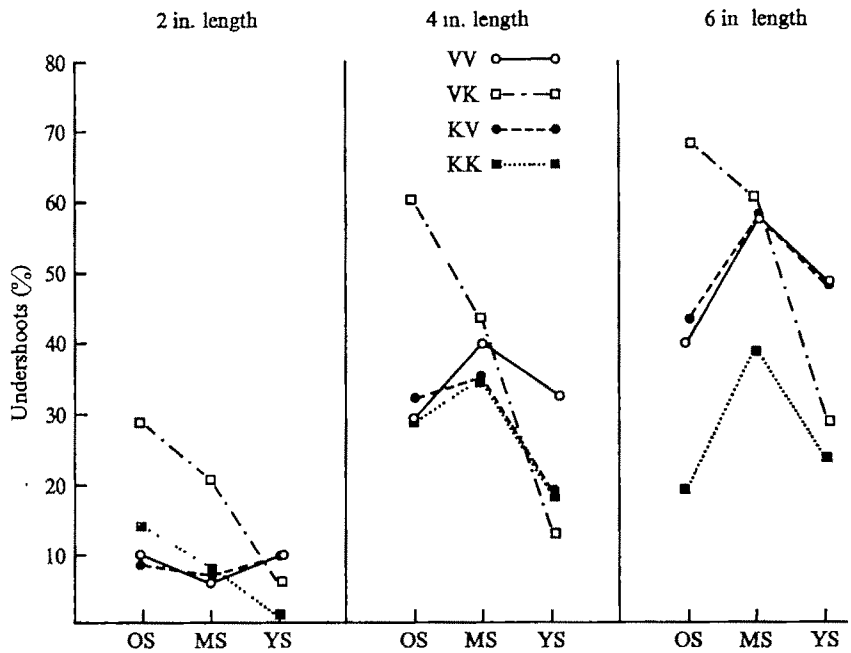

Fig. 2. Percentage of undershooting scores, regardless of error size, for 2, 4 and 6 in. standard lengths for visual–visual (VV), kinaesthetic–visual (KV), visual–kinaesthetic (VK), kinaesthetic–kinaesthetic (KK) conditions at 4 years 1 month (YS), 6 years 0 months (MS), 8 years 0 months (OS) age levels (analysis I).

The proportion of negatively to positively signed errors is thus not an artifact of length size. The results for subjects in analysis I are shown in Fig. 1.

At the 4-year level the proportion of undershoots on both K–K and V–K was small (14 and 16 per cent respectively). However, with one exception, the percentages of undershoots at all ages and for all conditions were below 50 per cent. The exception was a surprising and quite unexpected peak of undershoots on V–K at the 8-year level and to a lesser extent at the 6-year level. That this was not due to any particular standard length is shown in Fig. 2 which gives the proportion of undershoots for the 2 in., 4 and 6 in. standards separately. As in work with adults the proportion of undershoots varied with the size of the standard (Pepper & Herman, 1970). It was highest for the 6 in. length (45 per cent), less for the 4 in. length (32 per cent) and smallest for the 2 in. length (11 per cent). Nevertheless, the peaks on V–K for the 8-year group, and to a lesser extent for the 6-year group, are replicated in the patterns for all three lengths. The characteristic profiles of the proportion of undershoots for different modality conditions did not seem to depend upon the particular length employed.

Similar results were found for subjects in analysis II (see Fig. 3). The percentages of undershoots for both K–K and V–K were slightly higher for the selected than for the unselected 4-year group: but while there was little or no difference between this age group and the 8-year-olds on K–K (25:21 per cent), on V–K the 8-year-olds showed more than twice the percentage of undershoots than did the 4-year-olds (60:25 per cent). For the 6-year group the difference was less (45:25 per cent).

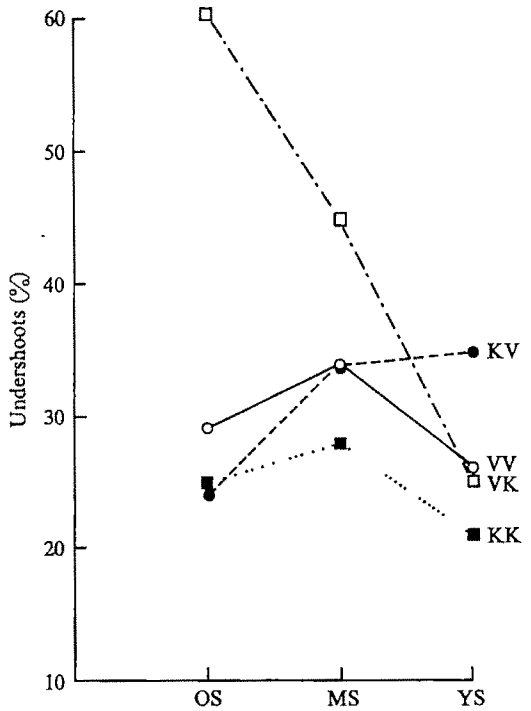

Fig. 3. Percentage of undershooting scores, regardless of error size, for visual–visual (VV), kinaesthetic–visual (KV), visual–kinaesthetic (VK), kinaesthetic–kinaesthetic (KK) conditions at 4 years 2 months (YS), 6 years 0 months (MS), 8 years 0 months (OS) age levels (analysis II, subjects selected on K–K).

The relatively high proportion of undershoots by the 8-year group on V–K was not found for their K–K performance (52:22 per cent on the first analysis; 60:25 per cent on the second analysis), despite the fact that V–K and K–K did not differ on absolute error at this age level. Similar scores were here evidently achieved by different means. Shrinkage of memory trace (Pepper & Herman, 1970) cannot explain this even if V–K information is held in a kinaesthetic short-term store as assumed by Connolly & Jones (1970). This should have affected K–K also. It is more plausible to suppose that the undershoots on V–K resulted from over-correction of the tendency to overshoot (shown by low undershooting which characterized K–K performance despite equal absolute error scores).

Discussion

Tasks and stimuli were the same for 4-year-olds as for the 6- and 8-year-olds. The fact that the order of errors changed significantly between these groups was therefore due to differences in utilizing the stimuli. This is consistent with the assumption that older, but not younger, subjects were able to use the additional information provided in the task. More importantly, the change with age was not due to all modality conditions being equally easy for the older children. Modality was a significant effect for both the 6- and the 8-year groups, and was accounted for by kinaesthetic–visual errors in each case. At the 4-year level visual–kinaesthetic and to a lesser extent intra-

kinaesthetic, but not kinaesthetic–visual, errors accounted for the effect. By whatever processing or coding strategies the 6- and 8-year-olds differed from the 4-year-olds, the effects were different for the two cross-modal tasks.

These results have a number of implications for theories of cross-modal performance and development. They do not support the view that cross-modal errors were some weighted average of intra-modal errors. Equally, they make it doubtful that one specific integrating process is implicated, unless additional assumptions are made to account for asymmetries in performance when the modalities of the standard and the response inputs are reversed (as in V–K > K–V errors), and also for changes in such error patterns. Ability to utilize additional information seems to be importantly involved in this. The 4-year-olds judging lengths with proportionate relations to a total distance showed the V–K > K–V errors found previously for 5-, 8- and 11-year-olds judging lengths within an unspecified distance. The 6- and 8-year-olds judging lengths which could be coded in relation to a total distance showed different cross-modal errors. It is not surprising that subjects who are not as yet able to utilize additional information should perform similarly to older subjects for whom such information is not available. But it suggests that the significant change in error patterns with 'age' was not a change in intersensory integration, but resulted from lack of information; whether the subject is too young to utilize it or the task does not contain it. This is consistent to some extent with Gibson's view that intersensory errors change with the possibilities of amodal information pick-up. But her account does not explain why intersensory errors should ever be asymmetrical, or why they should change to a different kind of asymmetry with more information. Connolly & Jones's model (1970) does explain V–K > K–V errors. It assumes that information from the standard is translated into the response modality via an integrated long-term store prior to being held in modality specific short-term stores with different decay properties. The model does not, however, specify conditions which would easily account for a change to the pattern found here at the older age levels.

Most difficulties in explaining differences in cross-modal performance are removed if it is assumed that the amount and kind of information available to the subject determines what strategies of coding are possible, and whether these will be in the response or in the standard modality. If amodal information can be used as a measure against which discrepancies in the inputs are assessed, the response can be adjusted more accurately to the standard. Thus if a length can be coded as 'midpoint plus X', assessment of the deviation of 'X' from the midpoint is likely to be more accurate than if input over the total length had to be assessed without such a 'measure'. The better the pick-up of such relational information from the standard modality, the more accurate will be the response adjustments. Visual processing and/or short-term memory tends to be better than kinaesthetic (Posner, 1967). K–V rather than V–K would be more inaccurate in this situation. This could also explain the relatively high percentage of undershoots on V–K by the older children. Kinaesthetic responses were characterized by overshoots; adjustment to the visual standard compensates by undershooting.

On the other hand, if little amodal information is available to the subject this strategy may not be possible. Input may then be coded, as it is in perceptual conflicts, in terms of the dominant (visual) modality (Rock & Harris, 1967). When the response

is visual, therefore, kinaesthetic input from the standard will be recoded in some visual form or code. When the standard is visual, kinaesthetic response stimuli are likely to be neglected in the coding of the visual standard input. Adjustment to the visual standard of the kinaesthetic input is likely to be highly inaccurate in this case. This account has the advantage of explaining asymmetries and changes in error patterns with 'age' or 'stimulus' conditions by differences in coding strategies, such as those described above, which depend upon available information or the utilization of such information.

It is interesting to note the high proportion of subjects in the unselected group of 4-year-olds who responded with undifferentiated movements while showing quite adequate visual responses. A similar discrepancy has repeatedly been shown between visual and haptic performance by young subjects (Pick *et al.*, 1967; Millar, 1971), and has often been attributed to the later development of systematic exploration by touch than vision. Here the kinaesthetic response did not involve manual exploration. The extent to which kinaesthetic errors were due to lack of experience with blind positioning movement cannot be assessed from these results, and is a point worth pursuing.

The explanation of the results given above makes a number of assumptions which need to be tested further. The experimental results, however, are quite clear and require explanation. Intra-kinaesthetic errors were disproportionately larger than intra-visual at younger than at older age levels. But this alone did not account for the significant age × modality interaction of analysis II. The main contribution was from visual–kinaesthetic errors and, to a smaller extent, from kinaesthetic–visual errors. Simultaneous versus successive presentation of the visual standard did not have a significant effect.

The author is in receipt of a grant from the Social Science Research Council which is gratefully acknowledged.

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(*Manuscript received* 17 *March* 1971)

Br. J. Psychol. (1972), 63, 2, *pp.* 283–296
Printed in Great Britain

REPRODUCTION OF ORIENTATION OF KOHS-TYPE FIGURES: A CROSS-CULTURAL STUDY

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Adults and children from Zambia as well as children from Iceland were used to evaluate hypotheses that rotation of reproduced patterns in a Kohs-like task is not random but is such as to increase the 'stability' of the model and its symmetry about the median plane, and that the symmetry is of secondary importance to stability. The data obtained confirmed the difficulties which some African populations encounter when dealing with this type of task and verified the hypotheses stated above. The Icelandic sample made so few erroneous responses that their data could not be used in evaluating the hypotheses.

The first recorded observation of difficulties in orientation of objects experienced by subjects drawn from an African sample is probably that of Laws (1886), who noted that Malawi girls engaged as housemaids found it hard to learn to place cutlery and to set a table, paying due regard to orientations conventional in Europe. Laws attributed these difficulties to the roundness of houses and household goods in the culture from which the girls came.

Subsequently similar difficulties were noted under more rigorous experimental conditions. Nissen *et al.* (1935) found that Sousou children, who were 'probably as isolated and as little modified by cultures alien to their own as any group examined with performance test material in current use', experienced difficulties with reproduction (by drawing) of abstract designs and in several cases the reproduced designs were rotated relative to the models. But these difficulties might have been caused by the subjects' inexperience in drawing; this indeed is hinted at by the experimenters' observation that the highest score achieved on one of the more difficult designs was gained by a 'big, good-natured fellow, the only one who seems at home with a pencil'.

McFie also reported orientation difficulties in both Ugandan student nurses (1954) and in Ugandan technical school entrants (1961), when he tested them by Wechsler's Block Design test. Similar difficulties were also observed in the responses to Terman–Merrill Memory-for-Designs test which was used only with the latter of the two groups of subjects. McFie suggests that the essence of the difficulties lies in the cultural background of the subjects – a suggestion which has also been put forward by Jahoda (1956) – and attempts to refine their nature further, by hypothesizing that it is a difficulty in correct comparison. His 1961 study shows that the difficulty may become less if the subjects are given general technical training. A time lapse of about two years, during which the subjects underwent such training, is reported to have caused a statistically significant improvement in performance of the subjects on the two tests in question and on no other tests of the battery with the exception of the Picture Description test. Using these data McFie argues: 'Had the increases been the consequence of maturation associated with increase of age, there is no reason why the verbal scores should not have shown a similar increase'; and hence the increase is likely to be due to training received by the subjects. However, such an argument

assumes positive correlation between the changes in scores of verbal and non-verbal items of the battery when such a change is due to maturation and no evidence of such a relationship is presented. Nor was a control group of matched subjects, who spent an equal amount of time on some other activity than training, used.

Similar difficulties with spatial relations have been reported by Jahoda (1956), who studied a sample of Ghanaian children and noted that on the Goldstein–Scheerer cube test the subjects 'paid very scant attention to the spatial arrangement of the blocks on the table; designs were usually left at whatever angle they happened to be completed, so that there was nearly aways some rotation'. Furthermore, Jahoda observed that the spatial position 'errors' could easily be remedied by 'specific and emphatic instructions'; on the other hand, the accuracy of pattern reproduction was 'not so easily subject to improvement'.

Shapiro's (1960) study provides yet more data on this issue. The sample used consisted of 14 (13 Cewa and one Ngoni) men employed as sweepers in a Rhodesian town, and of 17 educated Africans (12 of these were Shona and the remaining five each represented a different tribe), all of whom were on the staff of elementary schools. Of these subjects 11 were men and six women. The subjects were requested to copy Kohs-type designs on a piece of paper, and the difference between the orientation of the scorable drawings thus obtained and the models was measured. The angles of rotation thus obtained for the two groups were compared and found to differ significantly, the illiterate group having higher scores. Comparisons of the illiterate group with various English control groups (normals, brain damaged and mental defectives) showed a similar difference. Shapiro discusses the responses of the illiterate group at some length, suggesting that the rotations produced were lawful and were influenced by two factors: (i) the orientation of the axis of symmetry of the design; whether 'vertical' (items *b* and *d*, Fig. 1) or 'inclined' (items 5*a*, 9*a* and 3*c*, Fig. 1), and (ii) the orientation of the pattern as a whole; whether the square was presented with one of its diagonals vertical or at 45° (items *a* and *b* and items *c* and *d* respectively; Fig. 1).

There appears, therefore, a divergence of opinion as to the very nature of the observed rotations, some of the researchers maintaining that the rotations are probably unsystematic, a result of lack of attention to orientation (Nissen *et al.*, 1935; Biesheuvel, 1952; Jahoda, 1956; McFie, 1961), whilst others, notably Shapiro (1960), and Maistriaux (undated), who studied this and allied problems in the Congo, suggest that the rotations are lawful. This suggested a further examination of Shapiro's data by postulating the following *post facto* hypotheses.

Hypothesis 1. Subjects rotated the designs so that squares appeared in their 'stable' orientations, i.e. with their sides parallel to the edges of the table at which the subjects sat.

Hypothesis 2. Subjects rotated the designs so that the axis of symmetry of the patterns became 'vertical', i.e. so that it lay in the subjects' median plane.

The corollary of these hypotheses is: Both the stability of a design and its symmetry about the vertical axis tend to decrease the tendency to rotate.

The hypotheses can be verified using Shapiro's own data. Table 1 presents them in such a manner that all rotations falling within response zones 30° wide, and having 0° and 180°, and 45° and 135° as central values, are grouped together. In the 0° zone

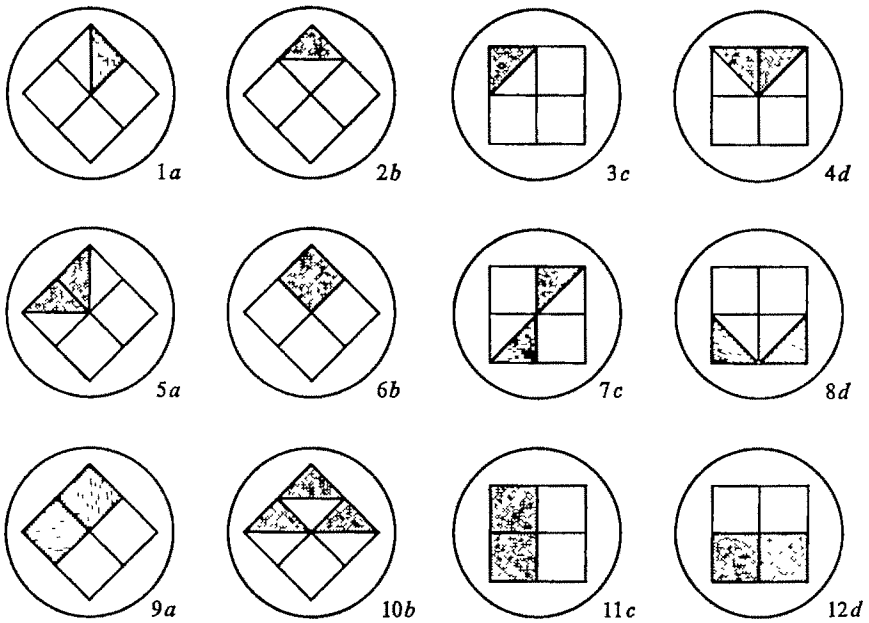

Fig. 1

fall all the responses defined as showing no rotation; in the 180° zone are all responses which are inversions of the models and which therefore do not differ from their models either in symmetry about the vertical axes or in stability. The other two zones (45° and 135°) contain rotations compatible, in the case of some of the figures, with the postulated hypotheses. The 'other rotations' column contains frequencies of responses outside the four zones described above and covers a total of 180° of arc. Hence the probability of a response falling by chance within this category is 0·5, whereas the corresponding probability for each of the possible 30° zones is 0·166. This

Table 1. *Reanalysis of Shapiro's* (1960) *results*

Hypothesized reason for rotation	Shapiro's fig. no.	Frequencies of rotations: 0° 0°–15° 166°–180°	45° 31°–60° 121°–150°	Other	χ^2	$P <$
Stability and symmetry*	2	0	14	0	14·00	0·001
	3	0	14	0	10·29	0·010
	6	0	13	1	10·29	0·010
Stability only	1	2	11	1	4·59	0·200
	4	0	13	1	10·29	0·010
	8	0	13	1	10·29	0·010
Symmetry only	7	11	0	2	13·00	0·010
None	5	13	0	1	10·29	0·010
Chance probability		0·16	0·33	0·50		

* Since Shapiro's data do not take account of the direction of rotation some of the responses may have been such as to create symmetry about the horizontal axis.

consideration leads to the total chance probabilities for each category of response as given at the foot of the table. These values were used in calculating chi-square values using the reported frequencies and grouping either the 0° category or the 45° category together with the 'other' category in such a way as to increase the probability of rejection of the postulated hypotheses.

The data support the first of the hypotheses but not, at first sight, the second. When the sole postulated reason for rotation was symmetry, and the stimulus was in a 'stable' position, a highly significant result *against* the hypothesis was obtained. If one, however, compares the group of stimuli where both hypotheses called for rotation with the results obtained with the stimuli where only the stability hypothesis held, the trend of results is in the expected direction, albeit statistically non-significant. (Yet this argument may be invalid because the data used do not permit one to distinguish between rotations leading to symmetry about the vertical axis and those leading to symmetry about the horizontal axis.) Furthermore, the high frequency (five out of 13) of reported reversals of the stable and symmetrical figure which preserved both stability *and* symmetry and the correspondingly low frequency of rotation through 90° (one out of 13), which retains stability but destroys symmetry about the vertical axis, also suggest that symmetry is not entirely ignored but plays a secondary role.

In view of these observations it was decided to modify the original hypotheses, for the purpose of a further experiment, to read:

Hypothesis 1. Stability of the figure is of primary concern to the subjects and reproduced figures will reflect a tendency to stability even when it leads to asymmetry of the pattern about the vertical axis.

Hypothesis 2. Subjects are influenced by the symmetry of figures and tend to orient figures so that they are symmetrical about the vertical axis. This tendency, however, is much weaker than the tendency to stability described in Hypothesis 1.

These hypotheses led to the experiment reported below.

METHOD

Materials

Because, as Shapiro's data clearly show, interpretation of drawings obtained in response to Kohs-type stimuli presents considerable difficulties in the case of illiterate subjects (of 20 subjects of this category tested by Shapiro 14 produced a full, or nearly full, number of recognizable drawings), it was decided to use a simple construction task similar to Kohs' blocks. However, blocks are occasionally arranged by subjects from certain cultures in a manner which suggests that the response, although erroneous by the established scoring criteria, is not in fact a random response. Thus, in reproducing a simple pattern consisting of two white squares above two red squares, a subject may arrange the two cubes nearest to him so that the two *vertical* faces nearest to him are red and the two top faces of the same cubes are white, while the top faces of the two cubes further away from the subject show an irrelevant pattern of colours. To obviate this difficulty it was decided to use flat tiles similar to those used by Ord (1970) in his pattern construction test.

It was further decided to use two sets of stimuli: one of traditional square type, the other circular. The latter was introduced because it eliminated the effect of stability, a circle being equally stable in all orientations, and hence allowed observations of effects of uncontaminated symmetry.

Two sets of 12 stimuli – one of traditional kind (Fig. 1), the other depicting circular patterns (Fig. 2) – were made. Each stimulus was drawn in Indian ink on a plain white disc about 90 mm

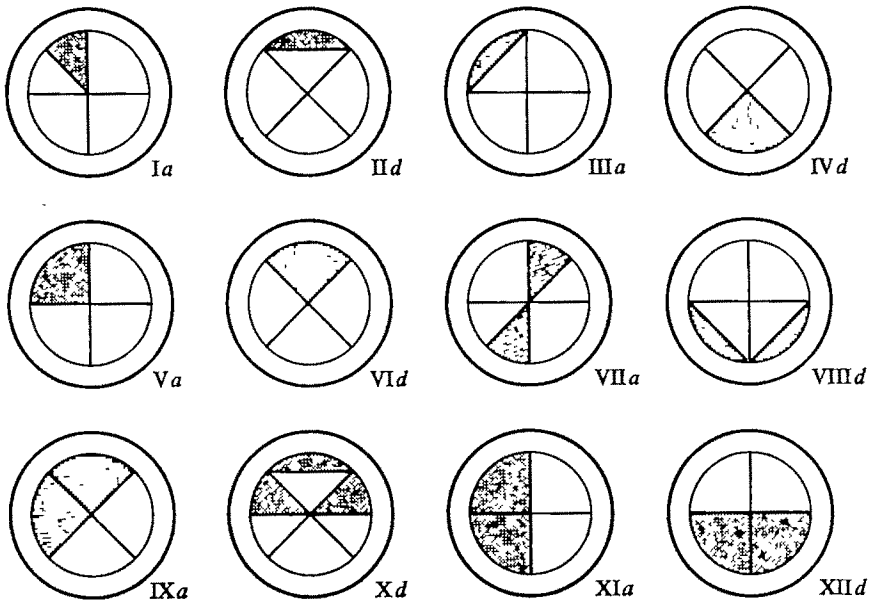

Fig. 2

in diameter; the areas shown white in the figures were left white; the dotted areas were coloured red. In the case of the square stimuli (which were 56 × 56 mm) each of four possible combinations of stability and symmetry was represented by three figures. In Fig. 1 these are coded as follows: *a*, unstable and asymmetrical; *b*, unstable and symmetrical; *c*, stable and asymmetrical; and *d*, stable and symmetrical. It will be noted that whereas instability can always be removed by rotation through 45° or an odd multiple of 45°, this is not so in the case of asymmetry. To achieve symmetry in stimuli 5*a*, 9*a* and 3*c*, a rotation of 45° or 135° is required; stimulus 11*c* requires a rotation of 90° and in stimuli 1*a* and 7*c* no symmetry can be achieved by rotation.

The patterns of the circular stimuli (64 mm diameter) were modelled on the square stimuli. Since the circle is by its nature in a neutral equilibrium, the stability factor is therefore absent and the stimuli fall into asymmetrical (group *a*, Fig. 2) and symmetrical (group *d*, Fig. 2) categories. In the case of these stimuli symmetry can always be achieved by rotation.

Special response tiles were made. Those to be used with the square stimuli were square, 28 × 28 mm, and 4 mm thick, made of wood backed by cardboard. (They were in fact pieces of Lowenfeld Mosaic test mounted on cardboard.) There were three types of tile: plain white, plain red and white-and-red, the colours meeting along a diagonal.

The tiles for use with the circular stimuli consisted of 90° segments of 64 mm diameter circle and were made of cardboard about 4 mm thick. There were five types: (*a*) plain red; (*b*) plain white; (*c*) white-and-red, the colours meeting along the bisector of the 90° angle, as shown in the upper left-hand quadrant of stimulus 1*a* (Fig. 2); (*d*) similar to (*c*) but with the colours reversed; (*e*) white and red with the red sector adjacent to the curved edge as shown in stimulus 2*d* (Fig. 2).

Subjects

Three groups of subjects were used; two Zambian (school children and labourers) and one Icelandic (school children). The Zambians were chosen as subjects because they came from a cultural and ethnic background similar to that of Shapiro's illiterate sample. The Icelandic subjects were intended to provide a Western Caucasian control group.

The two Zambian samples were: (*a*) *School children* – 32 school children from grade 3 of a suburban non-feepaying school in Lusaka. There were 16 boys and 16 girls. The mean and the

S.D. of their ages, either as stated or as estimated by the experimenters (when stated age appeared improbable), were 9·4 and 1·5 years respectively. (*b*) *Labourers* – 16 University of Zambia gardeners and cleaners, adult men of little formal schooling. The sample characteristics derived from personal data given by the subjects were: schooling, mean = 4·7 years (S.D. = 2·7): age, mean = 34 years (S.D. = 11·5).

The Zambian children were fetched from school where they were randomly selected from their classrooms. The labourers were approached at their places of work in the university and invited to participate in an experiment. In both cases a local female research assistant was responsible for bringing the subjects to the laboratory.

The Icelandic sample characteristics were: 12 children (six of each sex); mean age, 10·2 years (S.D. = 0·6). All the subjects had just completed three years of formal schooling. This sample was drawn from a small port and rural district centre (population 2200). The experiment was carried out during the summer vacation and the sample was obtained with the help of the local education official by approaching parents of children and asking them for permission for their children to participate in the experiment at their school.

Procedure

Two experimenters participated in the experiment, one of them was a local research assistant (either a Zambian or an Icelander) who instructed the subjects in their native language. The other (the author) merely handled the stimuli.

Subjects were tested individually and were allocated at random to be presented with either the square or the circular set of stimuli. The subject was sat at a table and told that he would be given a picture and some 'pieces' (these were shown) and with these would have to build a thing like a picture and tell the experimenter when he had finished building.

The second experimenter put the pattern to be reproduced as well as four tiles necessary for its reproduction on the table in front of the subject. The pattern was carefully aligned with its centre within subject's median plane. If the subject did not begin his reproduction immediately he was told: 'You can begin now'. After the subject had indicated completion of the task, the completed figure was removed and a new stimulus and the corresponding tiles placed in front of him. This procedure was continued throughout the series of 12 patterns, the patterns being presented in a random sequence. Photographs of subjects' responses were taken. It is noteworthy that unlike the traditional Kohs testing method this procedure did not require the subjects to choose appropriate response elements.

There were slight differences between the procedures used in Zambia and Iceland. In the former case subjects sat at a low glass-covered table, in the university psychological laboratories. Immediately underneath the glass top lay a large sheet of graph paper providing additional directional cues; in the latter case a school table in one of the classrooms of the subjects' school was used. A reward was offered to each Zambian subject at the end of his experimental session. It consisted of a box of ten cigarettes in the case of adults, and of a sweet in the case of children. No reward was given to the Icelandic children. None of these differences were thought likely to produce significantly larger rotations in the Zambian sample. Indeed the additional background cues and the rewards could be argued to enhance the probability of correct responses.

RESULTS

The responses were divided into four groups: (1) wholly correct responses having both correct shape (i.e. a square or a circle as appropriate) and showing correct pattern in any orientation; (2) correct shape responses having correct shape but showing incorrect pattern; (3) wrong responses having incorrect shape, and therefore of necessity showing an incorrect pattern; (4) a number of responses could not be reliably measured owing to a photographic fault, which was independent of the subjects' performance – these are given below as 'faulty photographs'. The frequencies with which these categories occurred within the three groups of subjects used were as follows.

Table 2. *Distribution 'wholly correct' responses to square stimuli*

Stimulus no. (Fig. 1)	Expected rotation (in degrees) due to: Instability*	Expected rotation (in degrees) due to: Asymmetry	Zambian men (n = 8) Type of rotation: Nil	Zambian men: Towards stability	Zambian men: Towards symmetry	Zambian men: Other	Zambian children (n = 16) Type of rotation: Nil	Zambian children: Towards stability	Zambian children: Towards symmetry	Zambian children: Other	Icelandic children (n = 6) Type of rotation: Nil	Icelandic children: Towards stability	Icelandic children: Towards symmetry	Icelandic children: Other
1	45	—	2	4	—	0	0	2	—	2	5	1	—	0
2	45	0 or 180	2	4	0	1	1	6	0	4	5	0	0	0
3	0	45 or −135	6	0	0	1	10	0	0	0	5	0	1	0
4	0	0 or 180	4	1	0	1	4	0	0	1	5	0	0	0
5	45	45 or −135	1	2†	2†	1	1	7†	7†	0	5	0	0	1
6	45	0 or 180	2	3	0	2	2	11	0	0	6	0	0	0
7	0	—	1	0	—	2	1	0	—	0	5	0	—	1
8	0	0 or 180	4	0	0	0	4	0	0	2	6	0	0	0
9	45	45 or −135	1	5†	4†	2	2	8†	8†	6	4	0	0	2
10	45	0 or 180	2	0	0	0	1	2	0	0	6	0	0	0
11	0	90	5	1	0	0	15	0	1	0	6	0	0	0
12	0	0 or 180	7	0	0	0	13	2†	2†	1	6	0	0	0
		Total	37	20 (incl. 6†)	6 (incl. 6†)	10	54	38 (incl. 17†)	18 (incl. 17†)	16	64	1	1	4

* 45 stands for 45°, 135°, 225° and 315° rotation.
0 stands for 0°, 90°, 180° and 270° rotation.
† These groups within each stimulus and subject include the same responses.

Square stimuli. Zambian adults: wholly correct, 67; shape correct, 20; wrong, 4; faulty photographs, 5; total, 96. Zambian school children: wholly correct, 109; shape correct, 60; wrong, 16; faulty photographs, 7; total, 192. Icelandic school children: wholly correct, 70; shape correct, 2; wrong, 0; faulty photographs, 0; total, 72.

Circular stimuli. Since in six of the circular stimuli (nos. I–VI, Fig. 2) a 'shape correct' response implies a 'wholly correct' response – a point dealt with more fully in the analysis section – it was decided to group 'correct shape' and 'wrong' categories for the six stimuli in which the former exists under a common heading of 'other responses'. The frequencies of the responses were as follows. Zambian adults: wholly correct, 87; other responses, 7; faulty photographs, 2; total, 96. Zambian school children: wholly correct, 154; other responses, 37; faulty photographs, 1; total, 192. Icelandic school children: wholly correct, 72; other responses, 0; faulty photographs, 0; total, 72.

In the case of entirely correct responses both to square and to round stimuli, the angles made by the corresponding pattern details were measured and recorded as falling into one of 16 equispaced zones $22\frac{1}{2}°$ wide into which the entire arc of 360° was divided. These zones are denoted by their central values. Thus the 45° zone lies between $33\frac{3}{4}°$ and $56\frac{1}{4}°$ measured clockwise from the orientation of the model, and the $-135°$ zone between $123\frac{3}{4}°$ and $146\frac{1}{4}°$ measured anti-clockwise.

The hypothesized orientations of responses to each stimulus are listed in Table 2, as are the observed responses. The latter are subdivided into four groups; (i) those falling into the 0° zone, and therefore regarded as perfect reproductions both in shape and orientation of the stimulus pattern; (ii) those supporting the stability hypothesis (which applies only to the square stimuli); (iii) those supporting the symmetry hypothesis; and (iv) other responses which do not belong to any of the above categories.

Table 3. *Distribution of 'wholly correct' responses to circular stimuli*

Stimulus no. (Fig. 2)	Expected rotation (degrees)	Zambian men ($n = 8$) Type of rotation			Zambian children ($n = 16$) Type of rotation			Icelandic children ($n = 6$) Type of rotation		
		Nil	Towards symmetry	Other	Nil	Towards symmetry	Other	Nil	Towards symmetry	Other
I	$22\frac{1}{2}$ or $202\frac{1}{2}$	1	3	4	5	5	4	5	1	0
II	0 or 180	3	—	5	8	—	6	5	—	1
III	45 or 225	1	2	5	1	3	9	6	0	0
IV	0 or 180	3	—	5	6	—	8	5	—	1
V	45 or 225	3	0	5	4	3	6	4	1	1
VI	0 or 180	0	—	7	4	—	9	5	—	1
VII	$22\frac{1}{2}$ or $157\frac{1}{2}$	0	3	2	1	6	3	3	3	0
VIII	0 or 180	4	—	4	9	—	5	5	—	1
IX	45 or 225	2	3	3	2	4	8	4	0	2
X	0 or 180	1	—	4	4	—	3	4	—	2
XI	90 or 270	1	0	7	8	3	3	6	0	0
XII	0 or 180	4	—	2	9	—	5	5	—	1
	Total	23	11	53	61	24	69	57	5	10

Analysis of results

Preliminary analysis

A comparison of the frequencies with which responses fell into 'wholly correct', 'shape correct' and 'wrong' categories for the three samples of subjects was carried out for the data obtained with the square stimuli. For the purpose of this comparison it was assumed that those Zambian responses photographs of which were indecipherable were in fact correct, since such an assumption reduced inter-group differences. A 3×3 chi-square table thus obtained was partitioned (Maxwell, 1961) and yielded the following comparisons.

(1) Comparison of the two sets of Zambian responses within 'wholly correct' and 'shape correct' categories. This was not statistically significant ($\chi^2 = 1{\cdot}74$; d.f. $= 1$; $P > 0{\cdot}10$). (2) Comparison of the two sets of Zambian responses within the joint 'wholly and shape correct' category with those in the 'wrong' category. This was also non-significant ($\chi^2 = 2{\cdot}02$; d.f. $= 1$; $P > 0{\cdot}05$). (3) Comparison of the combined 'wholly correct' and combined 'shape correct' responses of the two Zambian samples with the corresponding responses of the Icelandic sample. This was highly significant ($\chi^2 = 20{\cdot}57$; d.f. $= 1$; $P < 0{\cdot}001$). (4) Comparison of the combined 'wholly correct and shape correct' responses of both Zambian groups and of the 'wrong responses' of these groups with the corresponding responses of the Icelandic group. This comparison was statistically significant ($\chi^2 = 5{\cdot}04$; d.f. $= 1$; $P < 0{\cdot}025$). The Icelandic subjects were more often correct.

No analogous analysis of the responses to the circular stimuli was carried out because in half of these assembling of four tiles into a circle ensures a 'wholly correct' response. The 'wholly correct' and 'shape correct' responses are therefore confounded. (This confounding arises but once in the case of the square stimuli; in responses to stimulus 6*b*. In the analysis reported all these ambiguous responses were classified as 'wholly correct' since such a classification reduces the probability of obtaining statistically significant results.)

Analysis of the 'wholly correct' responses

Square stimuli. For all three groups of subjects, wholly correct responses were divided into those falling within the 0° zone, and those falling within other zones and hence regarded as rotated. They were further subdivided into four groups according to the nature of the stimuli: (i) unstable and asymmetrical figures (Fig. 1, col. *a*), (ii) stable unsymmetrical figures (Fig. 1, col. *b*), (iii) stable unsymmetrical figures (Fig. 1, col. *c*) and (iv) stable symmetrical figures (Fig. 1, col. *d*). A 2×4 table thus obtained, in which stimulus categories were presented in the above order, was used for non-parametric linear regression analysis (Maxwell, 1961).

For Zambian adults the overall value of chi-squares obtained was 20·0 (d.f. $= 3$) of which 19·3 (d.f. $= 1$; $P < 0{\cdot}001$) is accounted for by the linear component. For Zambian children 43·7 (d.f. $= 1$; $P < 0{\cdot}0001$) of the overall chi-squares is accounted for by the linear component whilst the remainder 18·4 (d.f. $= 2$; $P < 0{\cdot}0001$) accounts for departure from the regression line.

No corresponding calculation can be carried out for the Icelandic sample because of the very low frequency of rotations (eight out of 72 responses).

In order to evaluate the hypothesis that the orientation 'errors' made are not

random, the 'wholly correct' responses made to each group of stimuli having the same stability and symmetry characteristics, were examined. In each case a binomial test was used to compare the frequencies of the hypothesized errors with those of other erroneous orientations. Correct (zero rotation) responses were not considered. Only the Zambian data could be so analysed, for here again the low frequencies of error scores of the Icelandic group made analysis impossible.

In stimuli 5 and 9 (Fig. 1), both instability and asymmetry are postulated to influence orientation. Under these circumstances a response has a probability of 0·125 of falling by chance into one of two zones concordant with the hypotheses. The observed frequencies (Table 2) were in the case of both Zambian groups significantly higher ($P < 0{\cdot}0001$).

In stimuli shown in column *b* (Fig. 1) only instability should influence the orientation of the response according to the hypotheses. The corresponding chance probability is therefore 0·25 (since there are now four zones which yield the expected result). Comparison of the expected and observed frequencies shows them to differ significantly for both groups (Zambian men, $P < 0{\cdot}001$; Zambian school children, $P < 0{\cdot}005$).

No rotations supporting the symmetry hypothesis were obtained in the case of stimulus 3*c* (Fig. 1), whilst only one such response (made by a child) was observed in the case of stimulus 11*c* (Fig. 1). Few rotation responses of any kind were obtained to the stable and symmetrical stimuli given in column *d* (Fig. 1), which yielded the following absolute ratios of stable and symmetrical responses to other responses: for Zambian adults, 16:1; for Zambian children, 23:4.

Of stimuli 1 and 7 (Fig. 1), which were introduced to observe the effect of pattern skewness, no. 7 evoked, in the Zambian groups, very few 'wholly correct' responses (three in adults and one in children), while responses to no. 1 did not differ significantly from those for other figures of column *a*.

Circular stimuli. The circular stimuli were subdivided into two groups, symmetrical and asymmetrical, consisting respectively of the stimuli portrayed in the even and odd columns in Fig. 2. A chi-square test was used to compare the frequencies of 0° and non-0° rotations of the 'wholly correct' responses, made by the three groups of subjects to both types of stimuli. Neither Zambian adults ($\chi^2 = 2{\cdot}54$; d.f. = 1; $P > 0{\cdot}10$) nor Icelandic children ($\chi^2 = 0.00$; d.f. = 1) showed a significant difference between two sets of figures. Such difference was, however, observed in the case of Zambian children ($\chi^2 = 9{\cdot}59$; d.f. = 1; $P < 0{\cdot}01$) – rotations were more frequent in the case of asymmetrical figures. Comparisons of the Zambian and Icelandic children on the symmetrical and asymmetrical stimuli showed that the Icelanders rotate significantly less frequently than do the Zambians. (For symmetrical stimuli $\chi^2 = 6{\cdot}9$; d.f. = 1; $P < 0{\cdot}01$; for asymmetrical stimuli, $\chi^2 = 25{\cdot}9$; d.f. = 1; $P < 0{\cdot}001$.)

The hypothesis that the rotations are not random but are such that the reproduced figures become more symmetrical was tested by considering those responses made to asymmetrical stimuli which showed rotation errors. The hypothesis was confirmed in the case of both Zambian samples (for men, $\chi^2 = 10{\cdot}04$, d.f. = 1; $P < 0{\cdot}01$; for children, $\chi^2 = 45{\cdot}7$; d.f. = 1; $P < 0{\cdot}001$). Similarly in the case of Icelandic children those rotations which render the reproductions symmetrical occur significantly more often than other rotations (binomial test; $P < 0{\cdot}005$).

Discussion

As in the case of earlier studies in which Kohs' blocks were used (Jahoda, 1956; McFie, 1961) so here African subjects were found to have difficulties with reconstruction of patterns. The extent of these difficulties, as measured by the number of erroneous responses, was not found to differ between the two Zambian samples used, but both those groups differed significantly from the Icelandic group, which made fewer errors.

The difficulties encountered by African subjects are not unique in their nature; similar difficulties have been observed in other populations. Goldstein & Scheerer (1941) and Ord (1970) have encountered similar problems in American and New Guinean samples. Some of these problems appeared to stem from subjects', already discussed, inability to reproduce a two-dimensional pattern with three-dimensional blocks. However, as Shapiro (1960) points out, the similarity of responses does not here indicate similarity of causes. The importance of the present finding lies in the fact that, with the dimensionality issue largely removed, the African subjects were less often correct than the Icelandic subjects in reproducing the stimulus patterns.

The Zambian data support both hypotheses about the nature of rotation errors. The subjects in both Zambian groups who made orientation errors tended to reproduce patterns in stable orientations, and to set them so that their axes of symmetry lay in the subjects' median planes. The symmetry about the median plane was, as postulated, found to be the weaker of the two determinants of orientation and therefore likely to be overridden by the stability considerations.

No confirmation of the hypotheses was obtained from the Icelandic group, which made so few errors as to render statistical comparison of frequencies of various types of rotation impossible. It was also observed in the course of the experiment that this group was much more neat in construction of the responses and that the range of rotations of the responses which were for the purposes of this experiment considered unrotated was much smaller in the case of this group.

It appears, therefore, that Shapiro (1960) and Maistriaux (undated) were right to suggest that the responses of the African subjects to this type of task are not random as far as orientation is concerned and that their orientation errors are influenced both by the stability and the symmetry of the stimuli which they are required to reconstruct. It is impossible at this point to decide whether the subjects attempt a conscious aesthetic correction of the orientation of the pattern such as was investigated by Bakay & Schiller (1948) or whether both stability and symmetry about the vertical axis are used to facilitate coding of the stimuli when making responses, as suggested by Attneave's (1955) work. As far as the present writer is aware the stability effect has not been systematically investigated in this context on populations comparable to those sampled here. Symmetry on the other hand has been subject to such an inquiry and forms the basis of the Patco test (Hector, 1958, 1959), in which subjects are required to complete a pattern so as to make it symmetrical (but not necessarily symmetrical about their median plane). Tekane's (1961) analysis of errors on this test made by Bantu industrial workers suggests that the most frequent errors are due to imprecise placement of the test-pieces rather than to an inability to reproduce a symmetrical figure. The same point is made by Fridjohn (1961) who, in

discussing the performance of Bushmen, states that they were 'more concerned with approximate accuracy and would often tend to put the fourth oblong in only more or less the right position', an observation concordant with our observation of greater neatness and lesser variance of Icelandic responses when compared with Zambian responses. This evidence therefore suggests that symmetry is normally attended to.

In contrast with the above studies, Cole *et al.* (1968) report that Kpelle took less advantage than American subjects did of the patterning of arrays of dots, when estimating the numbers of dots present. Unfortunately their data do not permit one to assert how many of the patterns involved symmetry. Another pertinent study is that of Deręgowski (1961) which showed that Zambian schoolboys derived from the same population as used here were less affected by an orientation of a simple geometrical figure than were their Scottish controls, and tended to interpret the diagram which was symmetrical about their median plane, as depicting a three-dimensional model with a frequency not differing from that derived from interpretation of the same diagram rotated through 45° and no longer symmetrical about this plane. This result could *prima facie* be interpreted as implying that symmetry about the median plane is of no consequence to the Zambian subjects, thus contradicting the present data.

This contradiction is, however, only apparent; the two sets of results are complementary. For in the present experiment subjects are shown to rotate their responses so as to increase the stability and the symmetry of the array. It could be said that they respond as if the stimuli were more stable and symmetrical than they really are. However, as Hochberg & Brooks (1960) have shown and as Deręgowski's data obtained from the Scottish sample confirm, symmetry about the median plane reduces the likelihood of three-dimensional perception, and hence one would expect subjects who respond to asymmetrical stimuli as if these were symmetrical about this plane to respond two-dimensionally more often. Furthermore, since in the case of the figures used by Deręgowski stability is linked with symmetry, one would expect this effect to be relatively strong – which is the case: Zambian subjects fail to distinguish, in terms of dimensionality of their responses, between symmetrical (and stable) and asymmetrical (and unstable) presentations of the stimuli.

It is noteworthy that the frequency of erroneous responses varies with the stimuli. This variation is especially apparent in the case of the square stimuli. The two stimuli which were found to be especially difficult are stimuli 7 and 10 (Fig. 1). The former is a diagonal pattern in which symmetry of pattern cannot be achieved. This result could therefore be considered a further argument for the importance of symmetry in perception in the Zambian subjects. Stimulus 10, on the other hand, is a symmetrical pattern which is presented symmetrically; it is, however, the only pattern showing three red and white tiles. These, unlike the plain white or plain red tiles, have only a unique orientation in which they reproduce the pattern correctly, whereas single-colour tiles would do so in any of four possible orientations. An element of correct reproduction by chance is therefore much reduced and the pattern complexity probably adds to the difficulties. This problem of the difference in difficulty of patterns could perhaps be best studied by observing the sequence of manipulations which subjects perform when reconstructing a pattern, in a manner

employed by Serpell (1969), bearing in mind Vurpillot & Moal's (1970) finding that young children tend to regard as identical pictures containing the same elements but in a different array. If such an identity is also perceived by our Zambian subjects it would of necessity lead them to make a number of responses which were here classified as 'wrong', as well as a number of pseudorotations. However, since there is no reason to suppose that these variants would be non-random, the present finding of systematic rotation of responses is not affected by these considerations.

Two more points ought perhaps to be considered: the relation of the present findings to those of physiological psychology and to those of cross-cultural testing and selection. A connexion with the former becomes clear if one accepts that the terms 'internal pattern' and 'contour contrast' as used by Salzen (private communication) can be used to describe symmetry and stability as defined in the present study. As far as cross-cultural testing is concerned this study brings forth the divergence between the methods and the aims of a psychologist *qua* tester and a psychologist *qua* experimentalist. A response variable, such as the 'orientation error' considered here, may be seen by the former as a nuisance and his apparatus and method so modified as to preclude its occurrence (Ord, 1970) and to ensure that other desirable characteristics of the test are retained. A cross-cultural variable thus suppressed does not however cease to interest the experimentalist. It may be of practical importance (e.g. when instruction of subjects or tasks involving such a variable is considered) and it always remains, of course, of theoretical interest.

In carrying out this research I have received much assistance from the Ministry of Overseas Development (which helped to establish and still supports the Human Development Research Unit of the University of Zambia) and from the University of Aberdeen Carnegie Fund. I have also been greatly helped by and enjoyed the hospitality of Dr R. Serpell in Zambia and of Björg Friðriksdottir and Ingvar Þorarinsson in Iceland.

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(*Manuscript received* 12 *July* 1971)

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Printed in Great Britain

PAPERS TO APPEAR IN FORTHCOMING ISSUES

(Not previously listed)

ANNETT, M., Department of Psychology, University of Hull. The distribution of manual asymmetry.

BADDELEY, A., Laboratory of Experimental Psychology, University of Sussex. Selective attention and performance in dangerous environments.

BYTH, W., Department of Psychology, Queen's University, Belfast. Extraversion and the visual figural after-effect with tachistoscopic presentation.

DILLOW, P. V., MYERSON, J., SLAUGHTER, L. & HURWITZ, H. M. B., Department of Psychology, Arizona State University, Tempe, Ariz., U.S.A. Safety signals and the acquisition and extinction of lever-press discriminated avoidance in rats.

DIMOND, S. & BEAUMONT, G., Department of Psychology, University College, Cardiff. Processing in perceptual integration between and within the cerebral hemispheres.

FREEMAN, N. H., Department of Psychology, University of Durham. The influence of short-term novelty on affective assertions in young children.

FRIED, P. A., Department of Psychology, Carleton University, Ottawa, Canada. The interaction of intertrial intervals, timing of surgery, and differential hippocampal lesions.

GALE, A., HASLUM, M. & LUCAS, B., School of Social Studies, University College, Swansea. Arousal value of the stimulus and EEG abundance in an auditory vigilance task.

GALE, A., MORRIS, P. E., LUCAS, B. & RICHARDSON, A., Department of Psychology, University of Exeter. Types of imagery and imagery types: an EEG study.

GARDNER, H., Department of Social Relations, Harvard University, Cambridge, Mass., U.S.A. The development of sensitivity to figural and stylistic aspects of painting.

HAMILTON, V., Department of Psychology, University of Reading. Continuities and individual differences in conservation.

HARGREAVES, D. J. & BOLTON, N., Department of Psychology, University of Durham. Selecting creativity tests for use in research.

JOHNSON-LAIRD, P. N., LEGRENZI, P. & LEGRENZI, M. S., Institute for Advanced Study, Princeton, N.J., U.S.A. Reasoning and a sense of reality.

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THOULESS, R. H., 2 Leys Road, Cambridge. Perceptual constancy or perceptual compromise: a note.

WILLIAMS, D. I., Department of Psychology, University of Hull. Discrimination learning in the pigeon with two relevant cues: one hard and one easy.

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Br. J. Psychol. (1972), 63, 2, *pp.* 299–321
Printed in Great Britain

BOOK REVIEWS

An Introduction to the Study of Man. By J. Z. Young. Oxford: Clarendon Press. 1971. Pp. xxv + 719. £6.00.

This book grew out of a series of lectures given to medical and dental students at the beginning of their studies. It covers what is broadly known as human biology, viz. the biology of cells and of organisms, biological control and adaptation, elementary human genetics, growth and regeneration, ageing and senescence and some problems of population and its control. Topics also covered which will be of particular interest to psychologists include consciousness, brain maturation and the study of thinking, the measurement of intelligence, the development of the child as seen by Piaget, the evolution of the brain in relation to speech, the springs of human action and co-operation and aggression in man.

By and large, these psychological chapters are appropriate and well written. In the chapter on consciousness, in particular, Professor Young showed himself to be well acquainted with the semantic pitfalls (most of which he avoids fairly successfully), while at the same time discussing in a way likely to interest biological and medical students such topical problems as whether computers can think or consciousness in the split-brain patient is divided. His chapter on speech and the brain, too, brings out well the very special problems of anatomy, physiology and linguistics involved, though it does not perhaps quite face up to the problem of why language appears to be so exclusively human an accomplishment. Further, while hand specialization is discussed at some length, handedness is not. None the less, these chapters form a good introduction to a very complex and controversial field of study.

The chapter on intelligence measurement is sensible and the author is clearly aware of the empirical nature of intelligence tests and the influence of social factors upon their construction and use. Until we have more basic knowledge, Professor Young contends, we should try to avoid depending upon tests wherever we possibly can, while at the same time greatly increasing our efforts to understand the higher cerebral capacities. Yet he is by no means implacably opposed to intelligence tests and points out that their use may, under certain circumstances, make a creative contribution to education.

The chapter on 'The Springs of Human Action' takes in virtually everything from arousal to E.S.P. There is much interesting material here on homeostatic regulations, attack and defence reactions and computer models of the brain. Yet the treatment is inevitably so condensed as to appear piecemeal and superficial. It is a pity that the material could not have been expanded to cover at least a couple of chapters, even though this might have meant some condensation elsewhere. 'Human Cooperation and Human Aggression' is the only chapter to deal explicitly with social psychology and even this takes its departure from territoriality in animals. A little more about social organization in primate societies would have been helpful and perhaps a suggestion about where social science might help to clarify biological issues.

Every reader of this book will have his criticisms of its scope and content. The present reviewer, for instance, would have liked a chapter on stress and its implications for biology and medicine and rather more attention given to the special senses in relation to the integration of the central nervous system. Yet the breadth of coverage and close acquaintance with some half a dozen biological disciplines are truly remarkable. Professor Young combines outstanding erudition with an almost boyish enthusiasm which will ensure a most favourable reception for this book. Some people may think it tries to cover too much and in too facile a way. But education today needs books of this kind, even though traditional scholars may find such a state of affairs faintly distasteful. And if we are to have such books, we are surely very fortunate to have such a good one.

O. L. ZANGWILL

Subliminal Perception: the Nature of a Controversy. By N. F. Dixon. London: McGraw-Hill. 1971. Pp. xiv + 363. £4.00.

In his foreword to this book Professor Westby describes it as 'one of the most substantial works of British psychology of recent years which should stand as a classic of its type'. I am in complete agreement with him. Dr Dixon has written the first thorough account of subliminal perception and the strange controversies that have arisen in connexion with it.

Some may be wondering how it has been possible for Dr Dixon to have written such a substantial book on a hypothesis of such questionable status. But, as the bibliography of about 500 items demonstrates, an immense amount of time and energy has been devoted to the investigation of subthreshold stimulation.

References to the possibility of subliminal perception can be traced back to Democritus, Plato, Aristotle and more clearly to Leibniz. These early speculations were followed by a steep rise in the credibility of the hypothesis from the mid-19th century until 1960, when a steep decline occurred. The reasons for the doubts that then arose included fears of commercial exploitation by means of subliminal advertising and the demonstration that the classical methods of determining thresholds were not as accurate as had hitherto been believed. Interestingly enough, since the early 1960s confidence in the hypothesis began to grow and, Dr Dixon claims, has reached an all-time high at the present time, in spite of a few isolated and very verbal pockets of resistance.

A most useful chapter is devoted to the confusion that has arisen over such terms as 'consciousness', 'awareness' and 'attention'. Confusion does not, of course, necessarily bar the use of a particular concept, but semantic controversies have so bedevilled this particular field that Dr Dixon rightly takes great pains with his definitions. He concerns himself primarily with cases where the subject is *unable* to be aware of the stimulus. Thus he excludes situations where the attention of the perceiver is directed to other aspects of the stimulus field. The most important criterion, which if it is satisfied would justify the concept of subliminal perception, is the occurrence of contingent responses, without reported awareness of the stimulus, that differ qualitatively from those elicited by the same stimulus when it is above the threshold of awareness. This criterion would not only ensure that the subject was really unaware of the stimulus but would also entail that perception at the subliminal level is not just a weakened version of that occurring supraliminally. Of course, very few experiments have ever been performed that satisfy this stringent criterion. In order to assess the vast majority of experiments it is necessary to ask more loosely whether the result is more likely to have been due to subliminal perception than to some other principle.

The author works though the various contexts in which the subliminal hypothesis has been tested. He concludes that it has been shown to affect dreams, memory, adaptation level, conscious perception, verbal behaviour, emotional responses, drive-related behaviour and perceptual thresholds. It follows that subliminal stimulation constitutes a useful tool for further exploration of these processes. Furthermore, it would seem that the results of all this work force us to the conclusion that 'phenomenal registration is...just one of the many processes which brains can initiate'. As the energy of a stimulus is reduced the related subjective phenomena drop out although sufficient information may well remain to activate the response system.

Dixon argues that when brains evolved with the potential for conscious experience there had also to evolve control mechanisms to enable this limited-capacity system to function optimally. The maximal advantage was to be derived not from a gating of peripheral sensory input but from a restriction of that input into consciousness. Thus only vestigial mechanisms for sensory gating are to be found in man.

He concludes his argument by stressing the emotional content of much of the criticism directed at the idea of subliminal perception. He does not in any way detract from the value of the many methodological criticisms which have indeed helped to establish its reality status. But he cites examples from the literature which seem to show that there is a real fear of an entry of unscientific mystical concepts into hard perceptual theory. When the apparent threat to personal liberty posed by some subthreshold work is also taken into account, it is not surprising that there should have been so many critical attempts to demolish this intriguing concept.

D. W. FORREST

Behavior Modification in Child Treatment: an Experimental and Clinical Approach. By R. M. BROWNING and D. O. STOVER. Chicago: Aldine. 1971. Pp. vii + 422. $11.95.

This book should be compulsory reading for those involved in the care of severely disturbed children and adolescents, as it may help to refute the common belief that behaviour therapists work only with simple problem cases. The book describes in exhaustive detail the development of a 'total' treatment environment in a residential centre for very severely disturbed children. The authors and the members of their multidisciplinary team created a carefully scheduled treatment programme for each patient; the activities of each day were planned either to bring about a reduction in deviant behaviour by modifying its consequences or to facilitate the development of new response patterns. It is a monument to the patience and care of the staff that they were able to create such a controlled environment, and the book describes how they achieved it. The basis of all treatment was exclusively Skinnerian.

In addition to writing a book that will be of interest to anyone who follows the literature on behaviour therapy, the authors have also managed to make a significant contribution to the $N = 1$ literature. Theirs is a strictly idiographic approach; the major theme of the book is that the experimental-clinical model *can* provide a viable strategy for the management of highly complex treatment programmes. As they point out, experimental-clinical method (represented in this country by M. B. Shapiro) promotes a constantly changing and searching approach to treatment problems. It makes possible a truly scientific approach to treatment, whereby the management of each patient can be under constant review, with hypotheses generated and tested from within a consistent model. Accordingly, the treatment of all cases was conducted within predetermined same-subject experimental designs; the authors describe how they were able to manage treatment on a team basis (itself no mean achievement), whilst at the same time monitoring the effects of treatment programmes.

There is a useful chapter on single-case research designs, with plenty of illustrative clinical material and the authors provide a careful discussion of the relative merits of the different designs for a wide range of clinical problems. Another chapter describes how behaviour was monitored, using observation methods. The chapter on the principles and design of treatment may well provide a useful basis for training staff in techniques of behaviour therapy (at least where these are operant), and the authors outline some of the practical problems that arise in the management of any such programme.

Results are presented in the form of single case studies, and in this respect the book should be judged as a preliminary report, with (we hope) further publications to follow. The authors make a number of interesting points in their conclusions. For example, they note how naive it is to assume that reinforcement therapy is as simple as it is often portrayed. The effectiveness of even a primary reinforcer may be very significantly determined by who is the therapist. Secondly, a very detailed behavioural analysis is necessary before the differential effectiveness of reinforcers can be identified for each individual patient. There is the additional problem that in some children there may be a reversal of the values of social reinforcers. The authors demonstrate that in a few children positive social reinforcers had an opposite effect from that expected (i.e. they led to a decrease in the behaviour under study), while negative reinforcers accelerated behaviour. These carefully documented observations deserve further investigation in view of the significance that they have for the theory and practice of behaviour therapy.

In summary, the book can be recommended for its useful description of behaviour modification programmes conducted with disturbed children and for its contribution to the literature on the single case and experimental method. It is unfortunate that the authors have written so narrow a book, in so far as they very rarely stray outside an operant analysis. Also, the book leaves a lot to be desired in its style. It is overlong, and while the exhaustive detail can be justified, there seems little excuse for the frequent repetition.

R. K. TURNER

Learning Foundations of Behaviour Therapy. By F. H. KANFER and J. S. PHILLIPS. London: Wiley 1970. Pp. xi + 642. £4.85.

This book presents the reviewer with a problem. It adds yet another volume to a growing number of reviews of behaviour therapy. However, as the authors point out, it has been written not as a manual for clinicians, but rather to present the research and theory underlying the applications of learning principles to therapy. The authors hope that by presenting a comprehensive account they may reduce tendencies towards premature orthodoxy within behaviour therapy.

The book then lays more emphasis upon the theoretical roots of behaviour therapy than some others published recently. It is divided into five parts and covers the relationship between clinical psychology and behaviour models. The second part examines basic paradigms for behaviour therapy. In this, reviews are presented of behavioural modification related to classical conditioning procedures, the complexities associated with conditioned emotional responses, the use of covert and overt verbal responses as controlling stimuli for behaviour and finally the relationship between both social learning and behavioural rehearsal and clinical behaviour therapy. The third part reviews the use of operant procedures in the control of behaviour. In the fourth part there is a review of work on verbal behaviour, interviews and self-regulation. The final part contains a review of assessment issues and problems of ethics, training and theoretical foundations for behaviour therapy.

Any work which attempts to mix descriptions of practical procedures and theoretical background is open to criticism as to the selection of the theoretical material. In the main, however, the authors have established a good compromise between the amount of theoretical detail and description of practical procedures. Some of the material has been presented before, in particular, the substance of the chapter on self-control procedures is to be found in other writings by Kanfer. The success of the book is to collect together in one volume many useful references and valuable reviews. It can be recommended as a source book for those interested in behaviour therapy and required reading for those who have a substantial commitment to the clinical application of behavioural principles.

J. T. QUINN

An Introduction to Mental Retardation. By ROBERT M. SMITH. New York: McGraw-Hill. 1971. Pp. 272. £3.80.

The addition of 'in the United States' to the title of this book would not have been inappropriate. It is difficult to find a reference from a European source, and much of the advice as to the management and education of the mentally retarded is given with reference to the services available in the author's own country. This bias is more pronounced than one might expect as the text is written in the form of question and answer. The questions were obtained from a survey of teachers of the retarded, parents of the retarded and a randomly drawn sample of people from rural and urban communities. After each answer a list of recommended reading is presented, which makes it extremely easy to seek out books that will give further information on a particular subject. The format of question and answer is very readable, and should lead the student gradually into the subject-matter.

The book is organized into three sections. The first section, and chapter, gives an overview of the field, answering such questions as 'What is mental retardation?' and 'What can be done about mental retardation?'. It serves its purpose well and prepares one for the second section which forms the major part of the book. In the second section five developmental periods are taken and discussed in turn. The characteristics of the retarded individual are given for each stage, and techniques of management are discussed from the point of view of both teachers and parents. The most comprehensive chapters are those entitled 'The Pre-School Years' and 'The Elementary School Years'. They are preceded by a chapter on 'The Time Before and Soon After Birth', and followed by chapters on the adolescent and adult periods. The theme that stands out in this second section is that of the continual need for individual diagnosis and assessment before a programme of therapy and education can be administered. The author concludes the book with appendixes on 'Some Issues Pertaining to Research in Mental Retardation' and a 'Summary of Recommendations for Planning, Developing, and Coordinating Programs for the Mentally Retarded'.

Although much of the text is written with reference to the services available in the United States, there is still very much that is universally applicable to the field of mental retardation. It must be pointed out that 'mental retardation' refers to the bottom 16 per cent of any one age group, as measured by a test of general intellectual functioning, and not only to those persons with an IQ below 50, as is usual in this country. Consequently, one criticism that might be made by British students of the mentally retarded is that the book says far too little about the severely and profoundly retarded, concentrating as it does on the mildly and moderately retarded. The text as a whole places great emphasis on educational techniques, both in the home and at school, and tends to neglect the medical and psychological aspects of mental retardation. This bias reflects the 'spirit of the age' and gives the book a sense of dynamism. One hopes that the book will be read sufficiently widely by workers in the fields of mental retardation and special education for some of this dynamism to be reflected in the services available in Britain. N. A. BEASLEY

Genetics of Psychopathology. By DAVID ROSENTHAL. London: McGraw-Hill. 1971. Pp. ix + 182. £2.85; paper, £1.90.

David Rosenthal has produced a most valuable book. It is obviously a much pruned and edited version of his earlier book *Genetic Theory and Abnormal Behavior*, from which a great deal of non-essential information has been excised. In this form it will serve undergraduate students taking courses in psychogenetics and abnormal psychology, and could also be of considerable help to postgraduate students of abnormal psychology and more generally to clinical psychologists interested in biological determinants of psychopathology.

After a historical introduction, three chapters give basic information on evolutionary perspective, genetics and research methodology. These are followed by the longest and most important chapter on the genetics of schizophrenia. Three further chapters summarize information on the inheritance of manic-depressive psychosis, psychopathy, criminality, psychoneurosis, homosexuality and alcoholism, and a final chapter outlines some weaknesses and problems in the field under review. In reading the introductory chapter some British readers may suffer mild irritation at Rosenthal's North American humour. He speaks of 'the dear dead days beyond recall' rather than historical perspective, or puns horribly with 'the method in the madness studies'. Fortunately, the levity does not extend beyond the first chapter, which is in fact a brief but effective summary of the history of thought on madness from ancient to modern times. It considers the antithesis in views of madness as either psychologically or biologically determined, and the more specific variant, environmental versus genetic determination, forms the basic problem to which the rest of the book is devoted.

My open recommendation does not imply that the book is flawless. The chapter on basic genetics, for example, details a number of important principles to which no later reference is made. Also, in discussing meiosis, a needless and confusing distinction is made between spermatogenesis and oögenesis. In the methodology chapter a number of basic research techniques are outlined including pedigree, consanguinity, adoption and twin studies. Although Rosenthal pinpoints many of the assumptions underlying these methods he does not elaborate on the degree to which he considers them to be reasonable. The reader is left to guess at the relative economies of the different methods. To be fair, Rosenthal does question the assumptions underlying all the methods, but this is done piecemeal as the need arises in other parts of the book. This lack of systematic presentation is perhaps the most important criticism to be made of the chapter on schizophrenia. In many ways this chapter could stand alone, forming as it does the essential backbone of the book. It is a much more than adequate summary of present knowledge (one is tempted to say ignorance) of the inheritance of schizophrenia. Since the book is aimed at non-specialists, I find it difficult to justify Rosenthal's reluctance to 'attempt to tie all the material... into a neat package with a cogent conclusion'. The reader is left to do this for himself, a task which an intelligent and informed person could well undertake, though it seems unfair to demand this from undergraduate students.

A final criticism relates to the balance of the book. Some of the material on formal genetics, not at present well integrated into the book, could have been left out and the space filled by discussion of some methodological problems such as sampling and diagnosis, topics covered rather inadequately in the final chapter. Alternatively, because these foregoing problems are generally well covered in textbooks of abnormal psychology, Rosenthal might have expanded the genetics

to give an inkling of more recent developments, particularly in biometrical genetics. This would have given him an opportunity to talk about probable future developments. As it stands, the book is a lucid, though matter-of-fact record of what *has* been done. On reading this book, few prospective research students are likely to think, what those of us already involved believe, that psychogenetics provides an exciting frontier of research. If only David Rosenthal had been more in evidence in his writing, this could have been a great little book.

JOHN WILCOCK

Anxiety and Neurotic Disorders. By BARCLAY MARTIN. London: Wiley. 1971. Pp. xii + 160. £3.25; paper, £1.55.

This is one of a series entitled 'Approaches to Behaviour Pathology' edited by Brendan Maher 'to meet the need for depth of coverage on the central topics of abnormal psychology'.

Each volume is designed to be read either in conjunction with a standard text or (since the total series aims to provide a comprehensive coverage of the whole field of abnormal psychology) together with the others in the series, i.e. as a single text, if preferred. The aim is for each section 'to be scientificially authoritative, clear, and direct enough for the introductory reader, yet sophisticated and timely enough for the advanced student and fellow specialist to find of value'. It is an ambition which can only arouse interest in view of the dearth of such texts and particularly, as is explained in the introduction, in the light of the difficulties in understanding the nature and development of neurotic disorder and of investigating it 'scientifically'.

The author of this volume shows his hand at the outset by dismissing the belief that 'explaining-by-naming' has merit either for science or for understanding and treating such disorder. He nevertheless goes on to summarize the common symptom patterns of neurotic dysfunction, according to the traditional categories: 'anxiety reaction', 'phobic reaction', 'depression', etc. Is this a little surprising, disappointing even, after what we have been led to anticipate? Perhaps, but only momentarily. Martin himself justifies the step by claiming that it is still a basis for systematic description of common neurotic symptomatology. He leaves unspoken (does he perhaps think it not worth saying?) that it is also a link with the psychiatric literature, if this is to be utilized and integrated. At any rate it would be hard to fault the succinctness and precison with which he outlines and illustrates what he feels rightfully to fall under the headings he utilizes. We can certainly lose any sense of adverse criticism when he begins to show his plan more fully in the subsequent chapters. The first of these is on 'Anxiety and other Emotions', in which he describes the physiological aspects of anxiety, fear, anger and grief reactions, together with the processes of habituation and homeostatic control in these states. He draws on a wide range of contemporary research for his illustrations and is careful to emphasize in every case the actual procedures and measures from which we infer the presence of emotion. Having developed the theme that almost all biological systems (anatomical, biochemical or neurological) are characterized by wide variations from individual to individual and that many of these individual differences are influenced by heredity, he illustrates how this has been investigated in the field of the neurotic disorders already classified.

Each subsequent chapter unfolds the story further in a manner as simply, inevitably and pleasingly as the process by which a Japanese water-flower expands on immersion. For instance, having discussed the influence of these hereditary factors, he relates them to learning experiences with special reference to processes involving emotional expression and social interaction.

This brings him to the implication for the development of neurotic disorder of such research-based concepts and terms as conditioned stimuli and discriminative reinforcement which normally get very detached treatment but which are here given an unusual degree of psychological importance. In this connexion he is very careful to emphasize that the importance of this objectively oriented terminology is that it is susceptible to experimental handling: and, although he points out the danger of using terms even such as those mentioned above in an explaining-by-naming way, he himself avoids the pitfall masterfully. His discussion (for example of the problems representing mediating stimuli and the 'real-life' interacting systems at the end of this chapter) is characteristic of the honesty and clarity of approach that must earn the respect and gratitude of teacher and student alike.

In the following chapter on 'Learned Avoidance Strategies', these basic concepts are clothed with psychological detail. Experimental work over a wide range is brought in to illustrate his theme of the development of neurotic patterns of behaviour. These include mechanisms like

displacement, intellectualization and denial, some of which, as he points out, have been studied systematically and precisely, some less so.

In the last chapter but one he examines testable hypotheses about originating circumstances and current sustaining factors in the different varieties of neurotic disorder; then, without recourse to any existing general theory such as Freud's or Eysenck's, he attempts (in the last chapter of all) to marshall a wide range of findings in relation to the categories of neurotic illness specified at the outset.

I found this a thoughtful and satisfying (if unexpected) plan. The book is full of matter, expounded in such a cogent and deceptively simple style that I think it will go a long way to fulfil the ambitions expressed by the series editor at the outset.

From a more immediate standpoint I feel that it has virtue as an initial undergraduate text as long as the teaching that accompanies it can amplify it sufficiently sympathetically. Owing to the somewhat condensed style of writing this would be essential for beginners in the subject. However, even as it stands it must provide a helpful summary and framework for the more sophisticated reader.

SHEILAGH WHITAKER

Psychiatry and the Community. Edited by ISSY PILOWSKY and DAVID MADDISON. Sydney: Sydney University Press. (Australian edition 1969.) Pp. 155. £1.90.

This collection of eight papers by senior psychiatric authorities in Australia arose out of a conference held in 1967. The first three papers discuss epidemiological approaches to psychopathology; another three describe schemes of administrative regionalization, social work training, and child psychiatry that are employed or recommended (*qua* community-care) in Australia; and two papers offer empirical evidence as to the burden that early-discharge policies can place on the community (M. Harper), and as to the high incidence of psychopathology in an Aboriginal subpopulation recently driven from its home island by ecological disaster (J. E. Cawte).

The theoretical papers echo the political optimism of epidemiology's founding-fathers: the science, we are assured, has perfected its capacity to 'identify and cognate phenomena', and can tackle problems in 'a highly sophisticated manner'. The authors' tendency to mention studies without giving methods *or* results is disturbing in the context of such claims, as is their inability to properly review or conclude even such hoary debates as those on the 'downward drift' of schizophrenics or the importance of 'crises' in the aetiology of depression. Epidemiological approaches to psychopathology have encountered at least four major problems: the uncertainty of psychiatric diagnoses; the inability of correlations to reveal causes; the influence of the availability of treatment upon rates of referral; and the dampening evidence of the importance of genetic factors. When such problems receive their occasional mentions in these papers, the authors' apparent predilections – the claim for a genetic predisposition to smoking is attributed to 'psychiatric prejudice' – weaken their credibility as epidemiological enthusiasts. It is only in Cawte's paper that the possible causal contribution of illness to socio-cultural phenomena is seriously discussed.

Such problems apart, the evidence from field studies of untreated symptomatology clearly makes a case for increased involvement of psychiatrists in the community. D. C. Maddison raises what is a key question today for medicine generally: should attention be concentrated upon recoverable or upon almost certainly terminal illnesses? His suggestion that community psychiatrists might focus on 'crises' – like that of the 'redistribution of libidinal cathexes' involved in romantic love – does not inspire confidence that the psychotherapeutic appetite is satiable. If psychiatry is in search of new therapeutic successes, public education in mental health – via clinics, schools, clubs, women's magazines – is surely essential; but such measures receive little consideration here. Rather, the authors of the practical papers offer what is now rather conventional wisdom on the maintenance of contact between existing patients and their communities. No guarantees are offered for the effectiveness of mental health visitors selected for their low 'aggression' and high 'adaptability', nor for the value of treating absconding by continuous contact with staff on return. It is left to Harper, in a study of discharges from Powick Hospital, Worcester, U.K., to deal with the crucial question of Cunningham Dax's foreword: is 'the community' a place invented by the mental hospitals in which to put long-stay patients? In the light of his findings – half of the families at risk were thought to be under unwanted strain as a

result of the patient's discharge – we should perhaps expect the community only to multiply, not to add to, psychiatry's successes.

Despite the publisher's promises of this 'outstanding contribution', it contains little for the layman and only two wholly commendable papers for the expert; and the student will obtain better value for money from Ransom Arthur's recent Penguin.

CHRIS BRAND

Speech Communication Behavior: Perspectives and Principles. Edited by LARRY L. BARKER and ROBERT J. KIBLER. Englewood Cliffs, N.J.: Prentice-Hall. 1971. Pp. xv + 382. £5.00.

This book comprises eight chapters, each of which begins with an original review by 'an established scholar in the given area' and is followed by a selection of papers previously published elsewhere which are intended to deepen the content of the text. About 40 per cent of the pagination is original material, the rest being republished. A great deal of this material originally appeared in diverse sources which are not readily available in the U.K. and in this respect the book is of particular value to British readers. Each original section attempts to give a conceptual framework for the subsequent readings and concludes with a useful collection of references. In addition, the editors encourage active reading of the text by heading each chapter with a statement of objectives and terminating each chapter with a set of questions and points of discussion.

According to the editors the book was primarily designed as an introductory text for undergraduate courses in speech communication. Such a prescription falls short of its potential. With a surprising breadth of subject-matter, including discussions of the nature of theory and theorizing, an analysis of psychophysiology, sections on psycholinguistics, sociolinguistics, information theory, attitude scaling, attention, learning theory and language development, the book would make quite a useful – if unusual – introductory text in psychology. Indeed, after reading the first chapter, which presents an extensive description of what constitutes communication behaviour, one is left with the impression that all the time one thought one was studying psychology in the past one was really studying the communication process!

The breadth of coverage of the book has resulted, perhaps unavoidably, in considerable variation in the relative contribution made by different sections. With specific regard to the readings, for example, one might contrast the inclusion of the largely irrelevant discussion by Sternbach which attempts to distinguish psychophysiology from physiological psychology with the colourful, entertaining and informative sociolinguistic analysis of non-standard Negro English by Kochman. Similarly, with regard to the introductory review sections, one might contrast the rather uncritical account by Behnke of the application of psychophysiology to communication behaviour with the comprehensive, well-balanced and occasionally highly original review by Williams of the acquisition of communication behaviour.

However, in spite of these differences, the book succeeds in presenting a stimulating introduction to the subject-matter of communication. For the casual reader it is conveniently organized into more or less self-sufficient chapters and is certainly full of surprises. For the serious student of the communication process it provides both the necessary groundwork for and a taste of the promise inherent in this expanding field of scientific inquiry.

R. G. C. FULLER

Psychopharmacology and the Individual Patient. Edited by J. R. WITTENBORN, SOLOMON C. GOLDBERG and PHILIP R. A. MAY. London: Pitman. 1970. Pp. 256. £5.50.

The fourteen papers in this volume were given at a meeting of the Prediction Study Group of the American College of Neuropsychopharmacology held in December 1968.

The question discussed is the prediction of response in an individual patient to treatment with a psychoactive drug. There are two aspects to this question. One is how best to describe a patient's characteristics as a basis for prediction. The other is how to assess the patient's progress during drug, or placebo, treatment. Several papers are devoted to each of these problems: various techniques are proposed for the isolation of fundamental dimensions of individual differences and for the description of change. The important point is made that the amount of improvement of a

patient on a drug depends on the social environment in which the drug is given as well as on the pattern of pretreatment symptoms and social history. A paper of particular interest in this respect is one reporting an investigation of psychotropic drugs under domiciliary conditions carried out by the General Practitioner Research Group in England. There is also a valuable discussion of the specific problems posed by predictions of pharmacotherapeutic responses in geriatric patients whose sensitivity to drugs can vary quite markedly from that of younger age groups.

Altogether this is a useful volume which covers significant facets of the complex problem of finding the 'right drug for the right patient'.

DAPHNÉ JOYCE

Learning and Sleep: the Theory and Practice of Hypnopaedia. By F. RUBIN. Bristol: Wright. 1971. Pp. xi + 138. £1.75.

Sleep favours the retention of learned material but how this is brought about is not known. This book describes sleep learning, or hypnopaedia, and attempts to demonstrate its value as a reliable teaching technique. Too often the literature abounds with theoretical speculations that are based upon complicated and inadequately described techniques. This is not so for this book; the technical and applied aspects are dealt with in an explicit manner. It is true that the electroencephalic correlates of sleep would have benefited from a more detailed discussion (the source reference for this aspect is given in the bibliography), the Author (*sic*) has clearly been handicapped by lack of EEG facilities, but all other technical points are dealt with in great detail.

However, when we turn to the theoretical issues we find that they are not dealt with in a similar manner. The author appears to be largely unfamiliar with large areas of the psychological literature relating to learning experiments. One constantly obtruding fact is that the author's proselytizing zeal brooks few, if any, queries as to the substance of his claims. For example, a miniscule chapter two, just over one page in length, is headed 'Popular and Academic Misconceptions of Hypnopaedia' and it appears to be no more than a manifestation of the author's wish to scourge academics. Unfortunately, we are not told which misconceptions belong to which group and we are left to wonder if the academic misconceptions were the more, or less, substantial ones. This is important, because a later passage dismisses as naive, questions that related to the adequacy of the experimental design in some of the studies reported in this book. Surely such fundamental questions should not be dismissed in this sweeping condemnatory way. Great weight is placed upon successful hypnopaedic attempts to help Russian students learn English. These backward students are admitted to be required to learn the presented material in order to complete an educational requirement, and we might suspect that they were rather more than passive recipients of recorded information. Therefore, we could expect the use of a balanced experimental design with adequate controls to extract the relevant variables, rather than a series of consumer comments testifying as to the excellence of the product. In place of scientifically verified examples, we are confronted with generalities.

These are not mere carping criticisms for at many points the opportunity is missed to extend the author's position. For example: Do subjects undergoing hypnopaedia show increased REM or dreaming activity that might relate to 'neural learning processes'? Or could a differential effect be shown for material presented in the terminal rather than the early stages of sleep? The latter finding might relate to individual differences in diurnal activity cycles. But the opportunity to answer these, or more general, questions is ignored. The book also would have benefited from a rational discussion as to why hypnopaedia, has, apparently, never been considered as a particularly viable research project by the western scientific communities. This reviewer is not convinced that it is because of an active bias by western psychologists. Rather, perhaps, that whatever the admitted defects of our educational systems, the need to quickly raise the educational standards of countless numbers has never figured among them.

So, we are confronted with a small manual that is certainly of value at the introductory stage and containing detailed technical information for any aspiring hypnopaedist. But I suspect that such a person would soon encounter theoretical questions that this book, despite its subtitle, does not attempt to answer.

O. VAN-TOLLER

Perception through Experience. By M. D. VERNON. London: Methuen. 1970. Pp. x + 306. £2.75.

Readers of Professor Vernon's earlier books on the psychology of perception will not be disappointed, or greatly surprised, by *Perception through Experience.* The aim of the book, in the words of its author, is to provide for undergraduate students of psychology 'a general outline of a large number of experiments in visual perception, their findings and some of the conclusions which may be drawn from them'. In this modestly phrased aim the book unquestionably succeeds, although it would be of only limited use, in most areas, for the graduate student embarking on research.

Perception through Experience, as a title is used to indicate, is not the espousal of an empiricist theoretical position, but rather an interest in 'complex' rather than 'simple' perceptual processes – specifically, those processes of identification, classification and coding which can be assumed, or shown, to depend upon cognitive functions such as learning, memory, language. Thus, the chapters of the book deal with the perceptual development; attentional processes; perception of form, space, distance and movement; the constancies; effects of motivation and of personality; and – more unusual for a textbook of perception – the perception of people: human characteristics, emotions and actions. Given this wide scope and the emphasis placed upon empirical data rather than theoretical formulation, there are inevitably a few occasions on which a report of experimental procedure is so condensed or so cursory as to be virtually misleading. However, overall the treatment strikes an admirable balance between detail and breadth of coverage, between recent and well-established findings, and between sympathy and scepticism towards data and explanations which are rarely beyond dispute.

One has come to expect these topics and this quality of treatment from Professor Vernon's earlier books; what is a little different about the present book is its greater emphasis on perceptual development. There is an early, brief chapter on the origin and development of visual perception in infants, but in fact developmental trends are discussed in virtually every context throughout the book, appropriately enough since the importance of experience for a perceptual function is the basic rationale for its inclusion for discussion. The brief concluding chapter is principally a business-like summary of the reviewed data relating to innate perceptual abilities and their modification with growth in infancy and towards adulthood. *Perception through Experience* is not basically a textbook of perceptual development or perceptual learning, and it does not concern itself to any great extent with the explanation of age differences in terms of maturation, types of learning and types of training. Its achievement is to stress the developmental aspects of complex perceptual functions while not sacrificing the 'conventional' meticulous, up-to-date and usefully detailed account of those functions as they appear in adult perceivers.

ANN TAYLOR

Third Conference on the Fundamentals of Psychology: Various Approaches to the Study of Perception. Edited by ERNEST HARMS and MARGARET E. TRESSELT. New York: The New York Academy of Sciences. 1970. Pp. 597–738. $17.50.

This expensively slim volume is the consequence of a two-day conference which 'attempted the mammoth task of sampling problems in perception and discussing the role of perception in information processing, in the clinical setting in the abnormal mind and in the arts'. These topics are categorized under four headings: (1) perception of the outer world; (2) perception of the inner world; (3) perception in abnormal experience; and (4) perception of creativity.

Perception of the outer world is what most people have considered perception to be about, yet it is dealt with in just three quite disparate papers concerned with sequential blanking and displacement (Mayzner and Tresselt); complexity and preference for figures (Walker); and (nominally) the perception of gestalt – which is really an account by Helson of his adaptation-level theory. Each of these presents some novel data, but the areas cannot be considered a representative sample of work on external perception.

The four papers concerned with perception of the inner world deal with depth psychology; perceiving and thinking; and perception of the self and reality. Here we learn of the role of the ego in symbolism from Henderson; whilst Boernstein reminds us that 'Konrad Lorenz has most

emphatically stressed the fact that the complex laws that control man's mental life, including his social behaviour, are the same that control the life of all organisms'. In this strange paper are to be found terms such as 'specific sensori-tonic sense synergies', 'encephalization' and 'entoptic phenomena'. Spiegel, though, wanders even further – linking Freud's visit to the Acropolis with Kohler's experiments on adaptation to displaced vision in order to show that 'the identity of inanimate things and identity of self are necessarily bound to each other'. The reasoning behind this statement requires no comment – merely verbatim report: 'Since the Acropolin: – Freud's unconscious conflict appears to represent a projection of his father, and perhaps of his mother, we can assume that identity of animate objects and identity of self are also tied to each other.'

Bewilderment gives way to bemusement by the third section in which two papers seem completely out of place in a book on perception since they are about the classification and identification of psychotic and neurotic symptoms. The other paper in this section is by Weiner and is a reasonable review of the abnormalities of schizophrenic perception.

The final section contains but one paper by Pratt, which the title suggests concerns the perception of objects of art, but three quarters of which seems to be a potted history of perception. The remainder of the article discusses the relative roles of the artist and perceiver in determining the tertiary or empathetic aspects of art. The former's contribution emerges as the more important.

I hope that this brief account will give insight into the heterogeneity of topics discussed in this book. The aim of the editors was to help demolish the Tower of Babel that the field of perception has become. It might be said, however, that they have only contributed to the confusion. This is due to at least two reasons: (*a*) because the sampling of topics is not representative of the field, and (*b*) because of the variegated definitions of the term 'perception'. As the discussant of one paper put it, 'I just wonder whether you think that perception can justifiably be treated so broadly?' No one would deny Harms' initial contention that 'Perception is no longer merely sense perception', but to describe the topic, as does Henderson, in terms of 'an ongoing dialectic between conscious and unconscious modes of thought on behaviour', is to broaden it beyond the bounds of useful taxonomy.

In short, then, despite the merits of some individual papers, the symposium has not contributed to a better understanding of what the term 'perception' means. Instead, it has made it too amorphous.

H. D. ELLIS

Cognition: a Multiple View. Edited by PAUL L. GARVIN. London: Macmillan. 1971. Pp. xiv + 428. £10.00.

Readers of this *Journal*, particularly in the educational and training sectors, should be warned that this book is distinctly not what they might suppose from the title. For such readers the consumable psychological content is negligible since the volume is very much by, and for, theorists in cybernetics–computers–linguistics. Having sounded this warning, it is fitting to add that systems theories have profound relevance for psychology and that this volume contains interesting speculations for the initiated. The book derives from a 1969 Chicago symposium on 'Cognitive Studies and Artificial Intelligence Research'. It reports 15 contributions, two of which are by British authors, Patrick Meredith and Gordon Pask. Its four sections are entitled respectively: Cognition and the Organism; Cognition, Culture and Language; Cognition and Meaning; and Cognition and Automata. As with most symposium-derived volumes a reviewer cannot succinctly evaluate the whole, do justice to the parts, or characterize an appreciative readership. The most helpful advice this reviewer can offer is that specialists in artificial intelligence might look at the last section, and specialists in psycholinguistics at sections 2 and 3. Such specialists could then decide individually whether to purchase the volume.

IAN M. L. HUNTER

Probability and Statistics: Models for Research. By DANIEL E. BAILEY. London: Wiley. 1971. Pp. xvii + 686. £5.75.

Another large volume on statistics intended for advanced graduate and postgraduate students in the behavioural sciences. It has been, the author tells us, 'in preparation, theoretically, for sixteen years'. His description of the work as 'an interpretative presentation of the mathematical and logical basis of probability and statistics' is a fair one. It covers, largely from an abstract theoretical viewpoint, conventional ground up to higher order experimental designs, but much of the presentation is idiosyncratic with strong emphasis throughout on interpretation of the results of statistical tests. Herein lies the interest and value of the text and also what could be its weaknesses.

The first eight chapters contain a great deal of very elementary material in highly formalized form with a liberal use of symbols and few practical examples. 'When', we are told, 'an experiment is performed some specific result is observed. Thus results are the things which may be observed on performing an experiment' (p. 14). Some of the proofs offered contain inferences of similar profundity. On p. 149 Theorem 2 states: 'If a random variable X has only one member c in its value set, that is, $X = (c)$, then it is a constant random variable and $E(X) = E(c) = c$.' A proof is provided for this theorem! Admittedly the author recognizes this as 'a degenerate case in the sense that constants cannot really be random variables' but then why make a 'theorem' out of it? These are not the only examples of the labouring of simple points to be found in the preliminary section and it would be a pity if a reader lost the patience to continue further. It is, perhaps, a matter of opinion whether the manner of presentation used would be of help to the more statistically naive student. He might well be led to suspect and seek a profundity which is not really there.

The chapters on continuous random variables contain a useful introduction to the subject with a clear exposition of integration for those with no mathematics, and a novel exposition of the t distribution in some depth. But even as far on as p. 232, where Chebyshev's inequality is adequately expounded, we have a probability, $P \geqslant 1-(5/N)$ and are told: 'This inequality makes sense for probability only when $N > 5$', and we are enjoined to 'calculate it for some N less than 5 to check this'! However, the level of the text changes quite radically as it proceeds.

What, in general, is regarded as a Bayesian approach is strongly evident from p. 331 onwards, although the name of Thomas Bayes is not mentioned, and the preceding chapters on probability never reach this degree of complexity. The author suspects that a subtle form of cheating may occur in conventional statistical practice in psychology: 'The "cheating" is accomplished by making the rival, the unorthodox, the unlikely theory the test theory. Then when the test hypothesis is rejected, the unsavoury conclusion is that the championed, the orthodox, the conservative theory must be true.' (This is the strongest language used in the book!) The fallacy, we are told, is mitigated by a suitably small α (P of Type I error) but β (P of Type II error) must be at a reasonable level in the decision rule.

This seems a rather strange argument for a statistician who is emphasizing the importance of the neglected Type II error throughout. In the absence of utility considerations a fair decision rule is described as one where $\alpha = \beta$, and β plays an equal role with α throughout the book. For this, of course, specific alternative hypotheses are needed and some discussion is devoted to a 'rationale for formulating a specific alternative in the absence of a natural alternative'. The solution proposed is to add (or subtract) a quantity $\delta\,\sigma$ to the mean predicted by the test hypothesis, where σ is the standard deviation of the measured variable and δ is chosen by the experimenter. 'Once a reasonable value of δ is selected the alternative can be calculated.' Nothing more is said about criteria for δ, although what is called the '(α, β, δ) procedure' is used from p. 337 onwards to set criteria for optimal sample size. In the cases considered where σ^2 is known, the procedure would probably yield optimal results (with a 'reasonable' choice of δ!) but such cases are rare in psychology. The single sample case where variance is unknown prior to the experiment poses a problem to which two solutions are suggested: (1) 'Find $\hat{S}^2$ from a pilot study in which no attempt is made to test hypotheses regarding μ;' (2) 'Use the nature of the observations, available studies, and so on, to estimate σ^2' (p. 359).

Thus it is doubtful whether such a procedure would commend itself to the research psychologist. 'As tempting as it may seem, it is seldom a good idea to utilize data from previous experiments...' (p. 377).

There are good expositions of elementary analysis of variance in greater depth than is generally to be found in current introductory texts intended for the same readership and again the (α, β, δ) procedure plays a predominant role. This is the author's main theme, as indicated by the appearance of these symbols on the cover of the book. Numerous formulae are presented together with new tables in the appendix for the purpose of calculating optimal sample size although, as the author notes, these become unworkable in experimental designs of any complexity.

As will perhaps be apparent this book is very much a 'curate's egg'. Few would wish to recommend it to students in its entirety without serious reservations. There is, however, much of interest and potential value in it for those specifically oriented to statistics in psychology. The problem of weighing the arbitrariness of δ and the restrictions on experimental procedure proposed, against dissatisfaction with the null hypothesis expounded in chapter 15, is all part of current statistical controversy on which Dr Bailey has apparently made up his mind.

J. K. CLARKSON

Cybernetics, Simulation, and Conflict Resolution: the Third Annual Symposium of the American Society for Cybernetics. Edited by D. E. KNIGHT, H. W. CURTIS and L. G. FOGEL. New York: Spartan Books; London: Macmillan. 1971. Pp. 249 + xxi. £5.00.

It is perhaps symptomatic of this symposium volume that one of the speakers at the original conference, Harold Guetzkow, when sent a copy of his address to edit for publication, preferred to substitute a previously written paper which, in depth, better spoke to the title of his talk than did the talk itself. For depth is what the remaining contributions signally lack, despite the range of experience met together at the conference.

The aim of the meeting was to assemble experts on conflict – at international and campus levels – and on computer simulation of major social systems, to see if 'the simulation laboratory could offer a testing ground for developing possible alternatives to violence'. Five papers describe confrontation on campus: they range from Tanter's severely statistical comparisons of turmoil from 1964 to 1968 (how many involved; whether police brought in, etc.) to the largely impressionistic review by Gardner of six 'critical variables' which precipitate conflict in academe. Of these authors, only Sherif would appear to have both a theory and empirical data to back it; and he, in presenting a highly condensed version of the socio-psychological analysis of the rise of social movements, has for the sake of brevity to refer the reader to his group's other publications for this empirical evidence.

A second series of papers deal with the analysis of international conflict; these are more business-like than the first series, in that no political scientist confines his analysis to statements of impression as some of the contributors on campus conflict are content to do. Feierabend describes an ongoing cross-national comparative study of political violence and aggression; Platig welcomes the snowballing of problem-orientated research; the Guetzkow paper links more conventional international relations research with the potentials of simulation; and Voevodsky shows the Vietnam war to be an orderly, mathematically lawful behavioural event. Each paper describes one aspect of international relations which could be studied in the simulation laboratory: Feierabend, for instance, applying the frustration-aggression hypothesis to political violence, is able to predict political stability or its lack in terms of such 'resources' as the average diet and literacy rate of a nation. Voevodsky illustrates how battle strengths, casualties and deaths are the 'quantitative behavioural data' which best describe modern warfare, and how they can map the Vietnam war on to other wars of the past century to show a surprising degree of consistency.

The last section of the book consists of a series of prognostications about hard- and soft-ware in the 1970s and beyond, and their broader impact on society. These papers, only parlously linked to the main theme of the conference, not only fail to be of much value to the social and political problems raised earlier, but are also couched in such general terms as to lack much intrinsic interest. Too easily do the men from the big computer corporations slip into something very like a public relations routine; and even I. J. Good, associated with the development of computers and simulation programs from their earliest, suffers from the same amiability that besets the whole proceedings: his discussion of the Ultraintelligent-Machines on society only begins to air the philosophical, moral and technological problems that will arise from their coming.

Cybernetics and simulation exercises so clearly have heuristic value for the understanding of

social system dynamics that the failure of the present enterprise to progress beyond the first pleasantries of an interaction is saddening: more detailed examples of cross-disciplinary co-operation already in progress would not only have given the conference participants more to discuss, but would also have written up into a much more convincing volume. As to whether simulation techniques have more than heuristic value, it is significant that in the only paper to bring one 'inside' a simulation exercise, Martin Shubik so clearly lays emphasis on the value of the constructing of a model: it is this, he implies, which will enable us to see what questions to ask in our research; actual validation of a model is infinitely harder. And the 'possible alternatives to violence' promised us in the editors' preface seem as far away from this as from any other perspective.

O. P. SPENCER

The Evolution of Psychological Theory: 1650 to the Present. By R. LOWRY. Chicago: Aldine. 1971. Pp. 237 + xviii. $8.95.

Dr Lowry, now associate professor of psychology at Vassar College, has written, not another textbook on the history of modern psychology, but in effect a long essay which revolves around a single theme, the application of physical models to the explanation of psychological subject-matter. Dr Lowry regards this as constituting the mainstream of psychological theory over the last three centuries, and he hopes that by expanding on this theme he may make us more aware of what psychologists have been attempting to do, and more aware of the limitations of what they have achieved. For he holds that 'psychological theory has not yet caught up with psychological reality'.

The book is divided into three parts. Part I deals with the period 1650–1800, and deals with Descartes, Hobbes and the British associationists, and the physiologically based theories of la Mettrie and Hartley. The upshot was 'the great simplification' – the belief that 'human nature was simple, uncomplicated, harmonious and readily intelligible'.

Part II deals with the 19th century and discusses the new German physiological psychology, psychophysics, and (rather sketchily) Darwinian evolution.

Part III is entitled 'The Threshold of the Present', and is concerned with psychoanalysis, Watsonian and Hullian behaviourism, and gestalt psychology.

The emphasis throughout is on the influence of the physical sciences and physical models. For Descartes and Hobbes equally psychology was ultimately the study of matter in motion. Locke and his followers attempted to portray the mind as a kind of Newtonian cosmos in miniature. Herbart, also, grafted the Newtonian conception of attractive and repulsive forces on to psychology almost without alteration. Fechner saw the same basic principles as operating in both the physical and the psychological realms. Darwinian evolution was a blind, mechanical process. And when he comes to discuss Freud it is the early 'Project' with its neuronal models that constitute the core of Dr Lowry's account. 'It is fair to say', he writes, 'that Freud's "psychical apparatus" of 1900 and thereafter was structurally similar to his earlier "neuronal system"' and basically, therefore, inspired by physical conceptions. Similarly, in dealing with the Gestaltists it is Kohler's 'physischen Gestalten' that receive most attention.

Dr Lowry writes attractively. He has studied his historical sources carefully, and there is a freshness and perceptiveness about much of what he has to say. The main point he makes is worth making. The influence of physics on the thinking of psychologists has been important. But of course Dr Lowry's thesis represents, as he would probably himself admit, a considerable simplification. Psychology has not had just one main track. There has never been agreement among psychologists, and it is the confluence of many diverse streams that has constituted modern psychology. Dr Lowry excludes all but one stream, and many great names (Spinoza and Leibniz, for example) receive no mention from him. But in spite of this one-sidedness, in spite of the fact that 'to the present' in fact means up to about 1952 (so that the developments of the last 20 years are disregarded), and in spite of a weak and somewhat disappointing concluding epilogue, there is much that is stimulating and interesting in Dr Lowry's book.

L. S. HEARNSHAW

Theories of Psychology: a Handbook. By ANN NEEL. London: University of London Press. 1971. Pp. xii + 482. £4.75; paper, £2.25.

What is a theory in psychology? Does the present state of knowledge in psychology justify the use of such a term? In other disciplines, such as the physical and biological sciences, a theory is a collection of partially confirmed hypotheses or a hypothesis which has had enough confirmation to more than justify its likelihood of being correct. Many psychologists would feel that their own field has sufficiently advanced to merit the use of the term 'theory' in these senses but is it appropriate to apply it to the whole of Freud's ideas or Jung's or Hull's, for example? Perhaps we would do better in the case of these latter examples to define a theory, loosely, as a relatively well-organized collection of hunches, suppositions and observations which are held with varying degrees of flexibility or rigidity.

It is this general sense of 'theory' with which we are concerned in Neel's book. We have here essentially an introductory text which contains in summary form an account of many of the major writers and thinkers in psychology. After a short introduction to psychology considered as a science, we survey with breathtaking speed the development of philosophical associationism and go on to structuralism, William James and functionalism. The next part considers Thorndike and Pavlov and this is succeeded by a treatment of relatively more 'recent' workers: Watson, Hull, Guthrie, Tolman and Skinner.

Each and every theory is examined in terms of a common framework to make comparisons between concepts clearer and more systematic. This framework considers: (1) the basic units of the theory; (2) the field of study; (3) methods of investigation; (4) basic principles; (5) consciousness; (6) sensation, perception and discrimination; (7) thinking, cognition and memory; (8) emotion and motivation; (9) the nature of the learning process; (10) behaviour and personality. Finally, a short critique is made of each theory.

Using this method the book continues with 'analytic' theories and summarizes the work of Freud and the neo-Freudians, Jung, Adler, Horney, Sullivan, Fromm and Kardiner. Next follows a shorter section on gestalt psychology in general and Lewin's work in particular. This is followed by a section on the personality theories of Allport, Murray and Murphy, with a side glance at some highly selected phenomenological theories.

We now leave the 'tender-minded' theories and go on to some 'tough-minded' physiology with an examination of the basic concepts of cybernetics and a summary of the ideas of Hebb and Selye. The final part is entitled 'An Overview of Psychology at Mid-Century' and reviews developments in general using the ten framework headings described earlier. It is the title of this concluding section which gives a clue to the basic fault in this book. We are not at the mid-century any longer and the book is hopelessly out of date. From reading the book one has the overwhelming feeling that it was mostly written about ten years ago; finished around 1963 and touched up to a trifling degree about 1969. One does not know who is chiefly responsible for this and it would be unfair to throw all the blame on the author as the occasional publisher can sometimes share a degree of responsibility.

Some of the major developments in psychology and related areas are ignored. There is nothing whatsoever on the work of Harlow and Chomsky; not a breath about ethology and the mass of research in animal behaviour. Piaget is mentioned in passing in relation to Sullivan's work and is not referred to again. The physiological material is too thin and outdated to give even the remotest impression of work in the last decade.

The blurb makes a point of referring to the 'valuable' list of references, but here again it is so very outdated, the majority being around the 1950s and very few in the 1960s. Major texts are given, but no attempt is made to give the new editions. For example, the third edition of Hilgard has been available since 1966, but here we are referred to the second edition of 1956. So many references of the last decade which are of major importance find no place in the bibliographies here.

Finally, most chapters are headed with a set of dates to 'refer to year of earliest and of latest major publications relevant to that theory'. These dates are often wrong enough to disturb any student's chronological sense of the development of psychology. For example, the chapter on William James is headed with the dates 1890–1922. This is clearly nonsense, as James died in 1910. At the end of the chapter there is a James and Lange reference dated 1922, presumably the source of the confusion, but this is a reprint of articles written before 1890! BERNARD SINGER

Analyses of Theories and Methods of Physics and Psychology. Edited by MICHAEL RADNER and STEPHEN WINOKUR. Minneapolis: University of Minnesota Press. 1970. Pp. ix + 441. £6.00.

This book is volume IV in the well-known series, the Minnesota Studies in the Philosophy of Science. It is divided into two parts. In the first, the interest of the contributors centres on two related themes. The one is the 'standard' or 'orthodox' view of scientific theories, which one finds in N. R. Campbell and later in, for example, Carnap, Nagel, Hempel and Braithwaite. The other theme is whether, and in what sense, it is possible to have an observation language that is not infected by theory. The contributors to this part of the volume include Feigl, Feyerabend, Hanson, Hempel and Hesse; and this part also contains a long report of a conference discussion on these (and other) issues. The bulk of Part 2 of the volume is made up of three chapters by Meehl, and one by Gunderson; and these do bear quite closely on matters of psychological interest.

I regret to have to say that I do not think the volume as a whole is a happy one; and I am not sure that psychologists will find it, *qua* volume, of much value. Part 1 struck me as heavy going and inconclusive. I am inclined to think that the issues would have been thrashed out better if, for instance, Feigl's opening statement of the problem had been followed by a discussion in successive chapters between two discussants alone; for example, Hanson and Hempel. This is not to say that the hard working reader will not find a great deal of stimulus in Part 1. But the design of the part goes some way to defeat the aims of the distinguished contributors to it.

In Part 2 Gunderson examines some asymmetries between first-person statements and claims, and second- and third-person ones. (For example, when I say: 'I am going to the bank', I know what I mean, whereas you may have to 'disambiguate' my statement.) He examines these asymmetries in order to show that they do not constitute objections to a physicalist stance on body and mind.

The most substantial chapter in Part 2 is by Meehl on 'Psychological Determinism and Human Relativity'. In it he attempts to refute Popper, and to show that determinism is quite compatible with human rationality. He does this by arguing, essentially, that though, of course, 'reasons are not causes', 'the tokening of a reason' can be the cause of my behaviour; and I 'token a reason' when I 'think the relevant sentences'. He explores several aspects of this whole question – in particular, perhaps, the relation between a complete micro-causal account of rational decision making, and the 'molar' account asserting that reasons decisively influence my choices. He argues that the truth of the former account would be quite compatible with the truth of the latter. I think this whole issue is an important one. It is nice to see that Meehl has gone into it, and I think his contribution is a valuable one. It is limited, however, by the fact that he does not make sufficiently clear just what is 'the tokening' (or 'thinking of') a sentence. It may be possible to say much more here without first waiting for the micro-men to come along and tell us what is going on.

Meehl's second chapter is concerned with 'Nuisance Variables', and he argues that a large portion of current research in the behavioural sciences is methodologically unsound – specifically because the *ex post facto* design used in most work is radically defective. I am not in a position to comment on the cogency of this large argument. His third chapter presents some methodological reflexions on the difficulties of psychoanalytic research. He says that it might be good research strategy to concentrate on the verbal behaviour of the analytic session itself – a remark with which I concur. The whole chapter seems to me to contain a number of shrewd and penetrating observations about this complex and paradoxical field.

B. A. FARRELL

Child's Play. Edited by R. E. HERRON and BRIAN SUTTON-SMITH. London: Wiley. 1971. Pp. xxi + 386. £4.50.

For Sutton-Smith the last 20 years seem to have been all work and very little play. His industry has borne fruit in the present volume, however, which is one of the most authoritative contributions of the diffuse and disorganized literature on play. The editors of this collection of readings have, by their careful selection, pithy interstitial material and overall layout, pruned the untidy shrub to a recognizable form so that next year's growth is bound to be direct and healthy.

The readings have been chosen, on the basis of considerable experience of the subject, to give some insight into historical development whilst at the same time stressing the ongoing nature of the research. It includes many recent papers in the field from a wide background of projects. The treatment is to a large extent impartial, their own views offered amongst others but not forced. Where the editors do commit themselves, their guidance is helpful and not prejudiced. The papers are grouped under the following headings: Narrative, Ecological, Psychoanalytic, Comparative, Cognitive, Developmental and Theoretical approaches. The weakest section seems to be the one on Comparative work which, unlike the rest of the book, presents two fairly old (1940s) and familiar papers, those of Beach and Schlosberg, whilst at the same time only briefly mentioning more recent work of much greater impact. The important work of both Dolhinow and Van Lawick-Goodall with primates in this area is, for instance, completely overlooked.

The editorial commentary included in the section on Cognitive approaches provides the most thought-provoking section of the book which is worth reading for this alone. Material is produced which emphasizes that not only are competitive games not universal but, more surprisingly, that imaginative socio-dramatic play is not universal either. This is a surprising conclusion and one which is well worth serious consideration as so much material on the psychology of play has been based on middle-class samples where this type of play seems the rule. The necessity for studying the learning procedures and differential types of play structures that are present in different groups becomes clear. Smilansky, working in Israel with immigrant groups, finds the development of play proceeds from motor-play through to circumscribed and realistic imitative play and without the imaginative play era, finally to rule games. Sutton-Smith puts forward the proposition that this is probably the most general sequence – imaginative play being a newcomer. Drawing on rather contradictory work by Eiferman, also working in Israel, he considers the possibilities of 'critical interdependencies between the time at which imaginative activity is most abundant and its availability for the development of the abstract processes'. He goes on to suggest that the integration of imagery and action in solitary imaginative pre-school play 'leads to relatively neutralized capacities for internalization by school age, so that the "as if" attitude, rid in part of its object cathexes, is free for the imaginative enterprises of reading and number' and then goes on to discuss the consequences for cognitive development of aberrations of this sequence. These are challenging propositions for the educator and invigorating ideas for the research psychologist.

A unique contribution of this book is the interchange between Sutton-Smith and Piaget in the final chapter. The discussion centres on the importance of play as a form of thought in the maturing individual, Piaget taking the view that it is less necessary when the individual is able to maintain a more adequate balance between his assimilative and accommodative activities. Sutton-Smith contends that play continues throughout life and becomes increasingly internalized. Unlike Vigotsky, however, Sutton-Smith points out that it is *play* that is internalized – it does not disappear into 'directed internalized thought' but continues as games on the one hand and an internalized expression system (fantasy) on the other. Child's play has its adult counterpart in daydreams and ruminations.

The readings in this volume, as the editors point out, exemplify a tension between forms and causes. Play has so often been treated as a derivative response system for expression of various kinds and this, argue the editors, has been the greatest stumbling block to the task of serious systematic description.

The power of this work lies not only in the careful selection from a wealth of reading but also in the insights of the editors. This book is dedicated to 'those who maintain that there is more than one way to play hopscotch'. Few of us had any idea there are so many.

MARY CROXEN

Human Infants: Experience and Psychological Development. By BURTON L. WHITE. Englewood Cliffs, N.J.: Prentice-Hall. 1971. Pp. xv + 160. £2.50.

This book is not, as the title suggests, concerned merely with summarizing the scanty evidence available on the effects of early experience upon the psychological development of the human infant: it is also concerned with presenting the author's arguments for a 'radical reorientation' of research in developmental psychology.

White points out that we are 'appallingly ignorant' of many aspects of human development, particularly between six days and two and a half years post-partum. This ignorance, he suggests,

is due partly to the practical problems inherent in work in this area, partly to the existing emphasis of psychological research on theoretical analysis and what he describes as 'superficially scientific exercises in establishing isolated minutiae', and partly to the policies of agencies providing funds for research. In listing outstanding workers in child development, the author stops after Freud and Piaget. It is no coincidence, he claims, that both come from biological backgrounds, and the work of both is based upon enormous amounts of non-experimental observation in naturalistic or near-naturalistic conditions. White advocates the increased use of intensive, longitudinal, ethological studies of development and much of the book is given over to describing a series of such studies.

In the introductory chapter the special importance of the first two years of life and the problems of investigating this period are discussed. Chapters 2 and 3 summarize the present state of knowledge concerning the behaviour of human infants and the role of early experience. The author distinguishes direct evidence, information generated from investigations of human infants *per se*, and indirect evidence, information generated from the study of adult humans and other species. He favours the former and calls for a greater investment of time and funds in collecting such information. Chapter 4 describes a series of experiments by the author and his colleagues on the development of visually directed reaching and its precursors, visual exploration, hand regard, visual pursuit and visual convergence, in institutionalized children during the first six months of life. Studies of visual accommodation and the blink response to approaching targets are also included. Further experiments examining the effects of different patterns of early experience on visual-motor development are reported in chapter 5. The general conclusion drawn from these experiments is that the development of sensorimotor behaviour shows a certain degree of plasticity and can be accelerated by early environmental enrichment. In chapter 6 major issues in current research are discussed and many of the arguments introduced earlier are expanded. In this chapter White claims that the field of child development has been achieving far less than it should. He suggests that it is overloaded with theories and concepts, most of which have been brought in from the indirect approach, and he concludes that it might be better to 'invest most of our resources in sharpening our observational tools and collecting 20 years of solid natural history first'. The final chapter consists of suggestions to postgraduates beginning work on development and to agencies providing funds for research.

This is likely to be a controversial book; many will undoubtedly disagree with the author's view on the current status of developmental psychology and the direction it should take in the future. It is, however, a valuable addition to the undergraduate library and it is essential reading for anyone contemplating research in the area.

ROBIN PRIESTNALL

Intelligence, Creativity and Cognitive Style. By GEORGE SHOUKSMITH. London: Batsford. 1970. Pp. 240. £2.30.

This book is a useful contribution to discussion about the nature of cognitive processes with the aim of integrating findings from a variety of fields. Much of the book comprises an exhaustive review of the literature in the fields of intelligence theory and testing, problem solving and concept formation, and creativity. A good deal of this retraces fairly well-trodden ground except that the survey dealing with creativity covers territory less well charted, being particularly concerned with studies relating creativity to thinking in general. The sections on cognitive style owe a good deal to Bruner, Goodnow and Austin's formulations on cognitive strategies in concept attainment and Miller, Galanter and Pribram's views on plans and behaviour. Other topics covered are intelligence and creativity, personality and creativity, and response bias and cognitive style.

The critical review chapters are augmented by a report on studies by the author on the way in which various aspects of personality are implicated in different modes of thinking. He used a battery of 16 tests including tests of creativity, personality measures and tests of cognitive style with first-year university students. Factor analyses of the test correlation matrices are reported and provide the bases for inferences about the nature of the constructs under discussion. These findings are then related to those of other investigators, conclusions drawn and implications discussed.

The emphasis of the book is on the way in which 'cognitive processes direct behaviour' and how these processes are themselves influenced by environmental pressures and non-cognitive variables.

One outcome of this discussion is the argument that the term 'cognitive style' may be misleading since the style variables which affect the course of thinking are not solely cognitive. The concept of creativity is also seen as a composite concept related to style and other aspects of personality. In addition, arguing from the results of his investigations, the author suggests that there are two independent parts in the process of creative thinking: an early open approach to problems and a second perseverating stage after the essential elements in the problem have been isolated. The author argues from this position that to equate creativity with openness of approach is misleading. Openness in some circumstances could be dysfunctional. Educationists who equate creativity with openness of approach are taking too simple a view of the subject and may lead to children's adjustment being less adequate than it should be.

The book is intended to meet the needs of senior undergraduate and graduate students in psychology. This it seems to do very well but only a try-out with the target population can give a definitive pronouncement here. The earlier parts of the book tend to be somewhat tedious but they provide a valuable conflation of the key works in the various fields. The later parts are more interesting with some stimulating ideas and a very useful synthesis of current thinking. The index, unfortunately, is vestigial.

EDGAR STONES

T-Groups: a Survey of Research. Edited by C. L. COOPER and I. L. MANGHAM. London: Wiley. 1971. Pp. xvii + 283. £4.50.

Group Dynamics: the Psychology of Small Group Behavior. Edited by M. E. SHAW. New York: McGraw-Hill. 1971. Pp. xvii + 414. £5.25.

Cooper and Mangham have assembled in book form a dozen recent empirical studies of T-groups, all reprinted from easily accessible sources, notably the *Journal of Applied Behavioral Science* from which no fewer than half the papers were selected. The editors have added an introduction, six short link passages (which do not review *all* the empirical literature as they claim in the introduction) on 'The Effectiveness of T-groups in Producing On-the-Job Change', 'The T-group as a Vehicle of Organizational Change', 'The T-group Trainer' and other topics with an equally down-to-earth flavour, and a rather mysterious list of 14 'recent publications', half of which are *unpublished* manuscripts, including one by each of the authors. The undisguised bias of this book is towards applied psychology and away from basic research, and this occasionally verges on the anti-scientific, for example on p. xii of the editors' introduction: 'Basic research also can be seen as adopting a neutral, high-distance stance *vis-à-vis* the "subject" and this, too, runs counter to the values intrinsic to T-group training.'

The papers are supposed to represent the 'most methodologically sound models of research design in each of the mainline areas of T-group research', but most are in fact methodologically weak. This is no doubt partly due to the natural shortage of sound work in this comparatively young area of research, but the editors are not entirely blameless. Our paper on changes in self-perception following T-group experiences, by Gassner and her co-workers, is highly praised by the editors as 'the only well-controlled study' in this field, and was selected for inclusion as a model of methodological sophistication for future researchers. In fact, this paper describes three experiments, each of which contains at least one fundamental design flaw. In each experiment, for example, the subjects should have been randomly assigned to experimental and control conditions but were not even drawn from the same populations; and further, one of the major conclusions of the authors is not warranted by the results, since it involves the fallacious inference that the null hypothesis has been proven.

In view of the enthusiastic support for the use of T-groups in industry and elsewhere which the authors express ('one significant factor is that the detractors rarely include ex-participants'), and in view of the fact that the book is explicitly aimed not only at research workers, but also at practitioners who frequently lack any psychological training, a word needs to be said about adverse reactions following T-group experiences. Evidence is accumulating (see, for example, recent surveys reported by Walter Stone, Francis Kane and others in recent issues of the *American Journal of Psychiatry*) that acute psychopathological and even psychotic reactions are surprisingly common when screening of participants and other precautions are ignored. Nowhere in this book are such caveats mentioned, and the two fleeting references, both in the first chapter, to the

'unfavourable' and 'undesirable' changes noted by some investigators, are hardly sufficient to forewarn the uninitiated.

Marvin E. Shaw's advanced textbook of small-group theory and research is a much sounder work. It contains an unusually thorough and generally illuminating review of a wide range of important topics, including among others audience and coaction effects, brainstorming, the risky shift, interpersonal attraction, coalition formation, role taking, leadership, conformity, and an outline of some major theoretical contributions, somewhat overcondensed from an earlier work on this subject which Shaw co-authored with Philip Constanzo under the title *Theories of Social Psychology*. Such nourishing morsels are sandwiched between the obligatory chapters containing an inventory of unsatisfactory definitions (the author's own definition of a small group would clearly include the two opposing centre-forwards in a football game as a special case) and a defence of laboratory research against the occasionally well-founded charges that it is socially irrelevant. These chapters do not match the substantive body of the book in lucidity or insightfulness.

Two features of the author's writing style are bound to irritate and frustrate some readers. The first is the profusion of truisms and clichés: 'Man is a social animal'; 'Groups are composed of individuals and group products are the consequences of individual products'; 'The initial event in group interaction is the establishment of a relationship between two or more persons' and 'Groups do not function in isolation' are the opening sentences of a few of the early chapters. The second related feature, which I have already alluded to, is overcondensation. To be told nothing more about an experiment than what we are told on p. 88, that 'Summer (1959) noted that persons who sat near each other in the cafeteria of a large mental hospital interacted more than persons in more distant positions' left me wholly in the dark, and my bewilderment was not dissipated by the sententious conclusion which followed immediately, that 'It is clear, then, that proximity contributes to group formation'. At times, a deliberate effort of will was required to prevent myself from searching for a hidden profundity.

A minor oddity concerns the way in which the material is organized. One might be forgiven for thinking that a chapter entitled 'The Physical Environment of Groups' will centre on the effects of lighting, temperature, room size, outside noise and other aspects of the surroundings in which group members interact, but this chapter turns out to be concerned with more conventional research problems, notably communication nets. It is an excellent chapter, and it contains a useful account of the author's own research built on the foundations laid by Bavelas and Leavitt, but it is only indirectly related to the physical environment of groups in the usual sense of that phrase. Another chapter, entitled 'The Personal Environment of Groups', actually deals with the effects on group processes of individual differences, particularly among leaders. The author's inexplicable innovative usage of the word 'environment' emerges in passages like the following, which occurs on p. 155: 'The characteristics of individuals who compose the group thus constitute a personal environment in which groups must operate'. The categorization of material in terms of 'environments' does not make it any easier to find one's way around, nor does it create the 'vivid insight' which the publisher's blurb on the dust jacket promises from this organizational approach. The vivid insight is more soundly based.

ANDREW M. COLMAN

Social Psychology and Contemporary Society. By EDWARD E. SAMPSON. London: Wiley. 1971. Pp. xxv + 488. £5.00.

First impressions, as we all know, set a direction in terms of which subsequent information is shaped. Even book reviewers must often form some initial opinions which they will only relinquish after much contrary evidence. Just so with this new textbook of social psychology, which gives a first impression that does not, in my opinion, do justice to its mostly excellent content. The preface to the book is apparently an edited transcript of an interview between Sampson and an unspecified interviewer in which the author seeks to justify the writing of yet another text in social psychology; he is at pains to indicate how his book differs from the norm, both with respect to its style ('anecdotal', 'chatty asides', 'personal comments and opinions'), and with respect to its content and relevance to social problems in the world. Such supportive preambles are unnecessary in a good book and can be misleading.

If one accepts that this is really just another text, then the way the material is organized and presented becomes almost as important as the material itself, particularly when aimed at a student population, as this book is. In a brisk introductory chapter on the process of acquiring

knowledge about other people, Sampson comes down squarely, and quite properly, on the side of a world made out of our own constructions, setting the scene for a cognitive orientation which persists throughout. But he emphasizes a dual-transformation perspective for social psychologists: not only the transformations 'by man', centring on the knowledge processes, but also transformations 'of man', involving the rules, structures, values, ideologies and practices of the society and groups within which he lives. Hence the need for drawing upon the contributions of sociology and cultural anthropology. These two central themes form the basis of the divisions made in the book.

Part 1 contains some excellent chapters on mediation. Sampson does justice to the healthy modern concern with the processes of inference and attribution. His chapters on language and non-verbal mediators, understandably brief in a book of this scope, are very lucid and would make a good starting-point for further reading. In a general text aimed at American social psychology students, who may have no other grounding in general psychology, this point is important. The theoretical section on social comparison processes (Section III) takes the reader from Helson's psychophysical judgement model, through the social applications of the adaptation-level notion, as developed by McClelland and Atkinson and by Thibaut and Kelley, to the general principles of balance and consistency. The treatment of cognitive dissonance is lively. The author concludes that the ingenious experimentation on dissonance that we have seen in the past is not just a fad, but should lead on to an examination of the more basic questions concerning the conditions under which the dissonance principle holds. Perhaps what might have been discussed here is the general structure of the dissonance model and its status as a theory. It has been strongly criticized by many for generating no more than a string of rather vague principles which are only very loosely connected by a single assumption.

The section concludes with a chapter on status consistency which has long been an area of concern for sociologists like Goffman and Lenski. Sampson has himself worked in this area, and it is promising to see the interest now being shown by psychologists.

The final sections (IV and V) of Part 1 are concerned with selectivity and the socio-historical roots of cognition. The former presents the usual standard fare on the role of personality and needs in influencing what we perceive, recall and expose ourselves to. The latter discusses what the perspective of the sociology of knowledge offers us; we must interpret what we perceive and learn not just in terms of the groups and society to which we belong, but also in terms of the historical period in which were are reared and live. This adds a thought-provoking and challenging perspective.

One might argue that in these sections on acquiring knowledge some mention should be made of methods. There is, it is true, a very brief chapter on psychological experimentation, but nothing on attitude scales, rating techniques, the use of interviews, questionnaires, etc. However, to pay these topics the same attention that has been paid to the issues actually dealt with would necessitate a second volume.

Part 2 provides the justification for the title of this book. Sampson seeks not merely to illustrate current psychological thinking with topical examples of student unrest, social inequalities, prejudice and industrial strife in American society. He makes a genuine attempt to evaluate the contribution of social psychology to the understanding and diagnosis of these problems. He might even be accused (since social psychologists are not permitted to express their convictions) of taking a strongly moralistic stand on certain issues. In his final chapter on moral development, for example, Sampson takes up Kohlberg's distinction between three levels of moral thinking: the premoral, conventional and principled. In no uncertain way he applies the highest order level (i.e. principled) to those who resisted drafting to fight in Vietnam, and the conventional level to those who went to serve their country. The difference is essentially one of where the responsibility rests for taking another human life, and the 'advanced principled resister' can see no situation in which this is ever morally justified. It is clear with whom Sampson's sympathies lie!

Most of Part 2 is concerned with 'planned change' which is defined as occurring 'as part of an intentional effort to intervene in the ongoing state of a sytem. . .in order to produce a new state'. Subsumed under this heading are chapters on participation, T-groups, conformity and social influence processes, mass media and social movements, all excellent reviews of the relevant work, and surprisingly detailed for a book whose scope is so wide. Consistent with a general historical perspective, Sampson places the researches of Lewin, Hovland and their co-workers in the context of the world events which stimulated them. This is useful to the student who might otherwise have difficulty in grasping the particular significance of these now classical studies.

Altogether Sampson's book is extremely readable, lucid and up to date, and seems to strike a healthy note of enthusiasm and optimism about the state of our knowledge without overplaying the actual contribution of social psychology to our understanding of behaviour. This reviewer would strongly recommend the book to undergraduates of all levels, and it is to be hoped that it will soon appear in a paperback edition.

H. C. FOOT

Interstudent Attraction and Social Perception in the School Class. By SVEIN STENSAASEN Oslo: Universitetsforlaget. 1970. Pp. 282. 54 N.Kr.

This book is basically a research report of a theoretical and empirical investigation carried out at the Institute for Educational Research at the University of Oslo. The research was concerned with pupils' social perceptions in the classroom. It was based on the assumptions that feelings of attraction are developed among pupils through social perception and interaction and that interstudent perception and attraction are interdependent. The study's principal purpose was to explore this interdependence and, in addition, to investigate the potential relationship between interstudent feelings of attraction and students' perceptions of teacher approval–disapproval of individual classmates.

The sample consisted of 270 boys and girls, approximately 13–16 years old, in 11 classes of a coeducational comprehensive school. Data were collected by means of two instruments administered on the same day. The first consisted of a sociometric questionnaire in which the students were asked to make their choices with respect to the two criteria of spare-time and work companions. They were given unlimited choice to indicate whom they would and would not like as companions. In addition they were asked to indicate their three most preferred companions. They were also asked to guess by whom they expected to be chosen and rejected (ego-referent preferences and rejections) and then to take every classmate's place in turn and make three guesses as to his (or her) most preferred spare-time companions and the students by whom he (or she) thought he would be chosen (other-referent preferences). A second instrument consisting of two 'Guess Who' scales was devised for assessing pupils' perceptions of the frequencies with which their classmates met with teacher approval and disapproval.

Heider's theory of balance was substantially supported by the obtained results and in the main it was found that the teacher tended to influence the students' social status through his particular way of distributing praise and blame.

DEANNE BEALING

Purpose in Animal Behaviour. By F. V. SMITH. London: Hutchinson University Library. 1971. Pp. 192. £2.25; paper, 90p.

The author is tackling a very difficult subject. The material is presented in a clear and readable form, yet at the same time the writing goes beyond that found in a conventional text. The book not only instructs but also challenges the reader to think profoundly about the problems of causality in animal behaviour.

The first of the nine chapters is concerned with man's place in relation to the scale of nature and the second with some of the difficulties of explanation. It is here that one is led to believe that the problems suggested by the title will be confronted. Some of the material of this chapter, however – for example, the discussion of the work of Piaget and Werner – is less appropriate to the theme than it might be. At the end of the chapter in a short preview section the criteria by which behaviour may be distinguished as purposive are outlined.

The remaining chapters provide straightforward reviews of topics which include the problem of attachment, orientation, social organization, ritual in sexual approaches, territory, and the social organization of primates. The final chapter discusses 'purpose' and this is illustrated by drawing attention to such examples as 'the upstream migration of salmon against violent changing pressures and with frequent necessity to test alternative routes up or around waterfalls'.

In one sense it may not greatly affect the nature of contemporary studies of animal behaviour if the animal is regarded as having a purpose or not. Purpose like consciousness can be construed as an epiphenomenon, something of interest but definable only in terms beyond the confines of the investigation. The term can also be used to range across the spectra from movement of the smallest muscle fibre to the cosmic question of man's place in nature.

The method used by the author is to specify purpose by three criteria. In so far as animal behaviour can be shown to fulfil these criteria it must have purpose. The criteria are that the organism shows some degree of *independence* of environmental forces, that there may be *variability* in the nature and direction of successive acts, i.e. the same behavioural result may be obtained by the same animal in different ways, and that there is *retention* evidenced, for example, when behaviour is influenced by objects or situations not immediately present.

Purpose in this sense appears to be not all that different from what used to be called 'drive'. Here the reader may well find that he wants to describe purpose as something other than that suggested by Professor Smith. Indeed it is a measure of the success of this book that he should be stimulated to do so. Particularly appropriate are the questions of awareness, foresight and intention, which are becoming amenable to study in animals through a combination of experimental and electrophysiological methods, and which are passed over somewhat lightly in this book. For example, it is possible to record a decision potential from the brain of a cat long before the correct response is made in an avoidance situation if the animal has decided to make that response. There are important considerations here which might perhaps have received a fuller treatment.

STUART DIMOND

Development of Species Identification in Birds: an Inquiry into the Prenatal Determinants of Perception. By GILBERT GOTTLIEB. Chicago and London: University of Chicago Press. 1971. Pp. xi+176. £3.40.

This is an important monograph; it greatly advances factual knowledge of perceptual preferences of newly hatched precocial birds and it sets the new findings in a coherent theoretical framework. In a nutshell, postnatal experiments have shown that young chicks and ducklings are more responsive to auditory than to visual stimuli, and that they respond highly selectively to the former, but relatively indiscriminately to the latter. Although such hatchlings may be attracted to a wide variety of sounds, they nevertheless show a good recognition of the maternal calls of their own species in simultaneous auditory choice tests. Incidentally, domestication does not appear to have affected this ability in any way. Detailed findings in this field, where doubt had reigned previously, are valuable enough, but Dr Gottlieb has taken his investigations a good deal further. By means of ingenious operative techniques, he has succeeded in analysing the prenatal ontogeny of the ducklings' ability to respond discriminatively to appropriate maternal calls. These investigations are of a very searching kind, and it is impossible in a short review to give a summary account of them. Still, what emerges is that normally occurring auditory stimulation and self-stimulation of the embryo is instrumental in perfecting species-specific perception. The author clearly hopes that his own work, as well as further research involving increasingly sophisticated analyses of the ontogeny of behaviour, will eventually make the nature–nurture controversy, in its simple-minded form, outmoded. Dr Gottlieb's discussion as to how structure realizes itself through function is certainly valuable; but, above all, his truly invaluable contribution to the study of the development of early behaviour lies in his wide-ranging experimental findings.

W. SLUCKIN

Problemy Psychologii Matematycznej. Edited by J. KOZIELECKI. Warsaw: Panstwowe Wydawnictwo Naukowe. 1971. Pp. 432.

Mathematics provides certain theoretical tools which are being applied to an increasing number of areas of psychology. A particular set of such tools goes by the name of mathematical psychology; and this book in Polish, called *Problems of Mathematical Psychology*, is a valuable addition to the literature of the subject. There are altogether 13 chapters – ranging from psychometrics to information theory and decision theory – by nine contributors in all. They are: Choynowski, Walenta, Nowakowska, Kempisty, Mazur, Zimny, Ekel, Szaniawski and Kozielecki; some of them are already well known to English-speaking workers in this field. This book puts Polish mathematical psychology very much on the map. It is to be hoped that its publication will lead not only to a growth of interest in this subject in Poland but also to further developments in mathematical psychology generally, resulting from more interchange among workers in this field throughout the world.

W. SLUCKIN

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CORRECTION

John N. Nicholson & Jeffrey A. Gray: Peak shift, behavioural contrast and stimulus generalization as related to personality and development in children. *Br. J. Psychol.* **63** (1972), 47–62.

On page 55 the caption in Table 5 should read: 'Variables to the right of the dot...' instead of 'Variables to the left of the dot...'.

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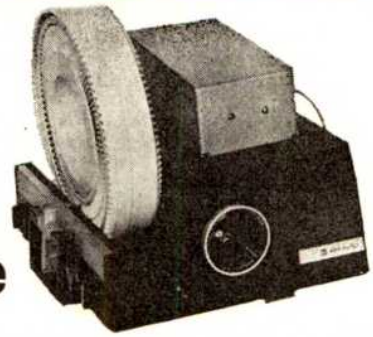

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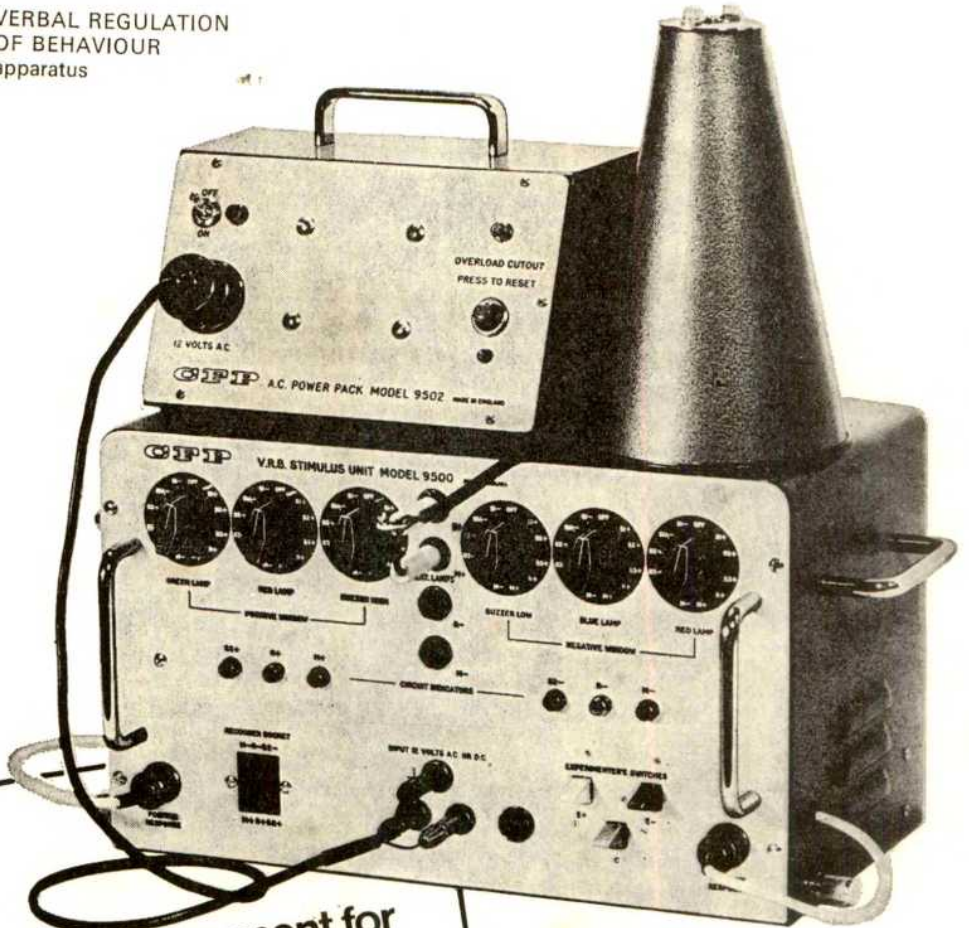

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The British Journal of Psychology is published quarterly.

Annual subscription (including postage) £7.50 net ($25.00 in the U.S.A.).

Second class postage paid at New York, N.Y.

Published by Cambridge University Press, Bentley House, 200 Euston Road, London, NW1 2DB and 32 East 57th Street, New York, N.Y. 10022.

Br. J. Psychol. (1972), 63, 3, *pp.* 327–341
Printed in Great Britain

LONG-TERM MEMORY: A STATE-SPACE APPROACH

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Some salient concepts derived from the information sciences and currently used in theories of human memory are critically reviewed. The application of automata theory is proposed as a new approach in this field. The approach is illustrated by applying it to verbal memory.

This paper has two aims. First, it attempts to survey some salient concepts used in theories of human memory which are derived from the 'information sciences'. Second, in an attempt to enrich the set of such concepts available for theory construction, it proposes the application of automata theory, especially in long-term memory.

Before turning to the proper subject matter of the paper, we shall re-emphasize certain methodological precautions. The author believes that a good deal of confusion is generated by unstated assumptions as to the nature of the concepts used by various theorists. We should like to suggest that theories of memory can be formulated at least at three different levels: physiological, structural and functional. These distinctions are by no means new, and we shall merely put them into an explicit form.

The level of physiological description needs little comment. At this level theories are concerned with the specification of physiological mechanisms for the implementation of memory functions, such as reverberation for short-term memory, synaptic changes for long-term memory, and so on. Since current theorizing in psychology tends to steer away from the direct use of physiological concepts, we shall not spend time in discussing these.

The next level of description is in terms of psychological structure. Structural theories involve the postulation of *physically* distinct components for the realization of psychological functions. Norman (1968) calls these 'box' theories, in the context of the short-term versus long-term memory distinction. The box theories imply that there are two separate physical storage systems (boxes), information being temporarily stored in one box before being transferred to the other. The level of structural description can also be illustrated by examples taken from electronics and computer programming. In electronics, the structural description usually takes the form of a detailed circuit diagram, showing all individual components, their values, and interconnexions. In computer programming, the structural description is a detailed statement of a program in terms of individual instructions (if machine code is used), or in terms of individual statements (if a higher-level programming language is used). In the circuit diagram, often separate components, or groups of components, can be related to distinctive functions of the circuit. In a program, distinct chunks of 'code' (groups of instructions) can also be separated out as fulfilling certain specific functions in the operation of the program.

At the level of functional description a certain division of the psychological processes is postulated, using units which can enter into the description of a process in a logical and intuitively satisfactory manner. We can extend our examples from elec-

tronics and programming to illustrate this level. In electronics, the functional description usually takes the form of a functional block-diagram, where each block is labelled with a specific function, and the interconnexions show the relationships of control, or transfer of information. In computer programming, the functional description takes the form of a flow-chart. This resembles a block-diagram, the boxes being labelled by the names of the processes represented by them, by the decisions to be made, etc., and the connexions showing the flow of control.

It should be emphasized that both functional and structural descriptions are usually of a hierarchical nature. This is due to the fact that one can group together a larger or smaller number of components into a single 'box', or merge several boxes into a 'super-box', obtaining a description which is simpler, but less detailed. We would also like to remark that the functional and structural descriptions do not necessarily map on to each other one-to-one. For example, a single structural component may participate in several functional blocks.

In the area of human memory, examples of concepts used in the structural sense are the short-term and long-term memory components, and the so-called limited-capacity channel; while examples of functional concepts are the primary and secondary memory systems of Norman.

Confusion is generated by the indiscriminate use of such concepts in some models. For example, the concept of an address (to be discussed in more detail below) is sometimes treated as a functional concept – for example, by Norman (1968) and Shiffrin & Atkinson (1969) – but sometimes as a structural concept by Broadbent (1970).

Some key concepts in theories of human memory

Here we shall discuss some important concepts to be used later, which seem prevalent in modern theories of human memory. There is a noticeable trend towards borrowing concepts from the rapidly developing information sciences, such as computer programming, information retrieval, and artificial intelligence (see Norman, 1969, for review).

Internal representation (images, codes, traces)

Most theories assume that the memory contents are represented in some coherent, organized 'data structures', making accessible all the information relating to an item in memory. These internal representations are assumed to have attributes, and the attributes to have values. The fact that an item can have several attributes makes the items multidimensional. An important idea here is that an item can contain 'pointers' to other representations, or other information. An item may even be entirely made up of such pointers to other information, which gives them a hierarchical structure, thereby relating this idea of an internal representation to earlier formulations like Bartlett's schemata, or the apperceptive mass.

In computer programming, these pointers are addresses specifying the location of the desired information. The use of such pointers has two advantages. First, it leads to storage space economy because it is sufficient to store only one 'copy' of each data structure, all uses of the structure being indicated by pointers, instead of replication. Suppose, for example, that a character string representing the name of an object has

to be used in a number of different contexts in various data structures. It is sufficient to store this character string only once, and to store pointers to the block of storage locations containing it, wherever the name has to be mentioned. Secondly, it makes storage organization easier because it gives us a mechanism for indicating that some data structures belong together, without actually storing them in the same physical neighbourhood. Pointers can be used to link together items which are physically scattered over any part of the available storage space, but which belong together, and often have to be retrieved together. Inserting and eliminating items from such groups is easy, for it is sufficient to manipulate and rearrange the pointers without the need for moving perhaps large and complex data structures around in store.

The number of such pointer chains in which an item can participate is unlimited and makes it possible to represent complex networks of relationships easily. The pointers themselves can acquire some structure by attaching labels to them in order to distinguish different kinds of relationships between items.

The accessing mechanism, and the concept of an address

There is fairly general agreement that most of the interesting problems of memory reside in the mechanisms of gaining access to information stored in the system, i.e. in the problems of retrieval. Contemporary computers are mostly of the so-called Von Neumann type, that is to say, they are organized in at least two distinct logical units, namely the processor and the storage unit. The storage unit itself is organized on the basis of addressable locations which are sequentially arranged and numbered, the numbers also serving as the addresses in gaining access to them. The idea of the address implies a distinguishable physical location which can be unambiguously identified from the knowledge of that address.

We would like to argue that this concept of an address, as a determiner of physical location, is not very useful in psychology. Human memory is organized on the basis of its contents rather than on the basis of the location of the contents. In contrast to the concept of fixed location addresses, modern programming techniques often eliminate the need for specifying absolute addresses in the machine by assigning symbolic names to locations, and also by introducing 'content addressing' (to be discussed below), thereby eliminating the need for even symbolic specification of locations. At any rate, accessing information in memory could perhaps be characterized in three different ways: by location, by attributes, and by a partial specification of the contents. The last two methods are not necessarily distinct depending on whether attributes are stored as part of the contents of an item.

The problem of accessing and retrieving information is best represented as a function which, when applied to the given information as an argument, yields the required information. The required information may be all or part of an internal representation. It may also be some information *about* a representation, which is not a part of it, as is the case in recognition, when we want to know whether an item is in store at all. In recall, on the other hand, a representation or some of its attributes are being retrieved.

Content addressing

A content-addressed memory is one which can be interrogated by specifying part or all of an internal representation, but not its address. A primitive implementation of content addressing in a Von Neumann type memory would be to search all locations and match their contents with the query until the required item is found. Clearly, this is an extremely time-consuming way of retrieving information, and in systems which have to operate in real time, a parallel, simultaneous interrogation of all or of a large number of locations is required. Various more efficient methods of simulating a content-addressed memory in a Von Neumann type system are now widely used in computer science. These methods achieve a more or less direct mapping of an item on to an address. Examples of such techniques are hash coding and decoding trees.

Hash coding is widely used for fast dictionary look-up of symbolic names. Suppose the symbolic name is represented as a character string. Each character has an equivalent numerical value (code), so that the string can be regarded as a sequence of integers. A so-called hashing function is applied to this sequence of integers, mapping the sequence in a random fashion on to an integer lying between specified limits, which is then interpreted as a storage location address. Ideally each name should be mapped on to a unique storage location where the information to be associated with the name could be stored. Due to the use of a randomizing hashing function, however, the mapping is usually many-to-one, although the probability of clashes can be made small. When clashes do occur they are resolved by one of several methods. For example, if the address produced by the hashing function is already occupied, then the neighbouring successive locations are searched and the item is put into the first empty one.

Decoding trees are more familiar to psychologists due to the work of Feigenbaum (1961) on a model of verbal learning in which the basic memory organization was a decoding tree. It is also widely used in computer programming. The essential idea is that a sequence of tests (or discrimination points) are arranged in the form of a tree. The process starts at the root of the tree, applying the test specified by that root node to the item under examination. The next test to be made is determined by the outcome of this discrimination, specifying a particular branch of the tree to be followed from the root point. By applying a sequence of tests we 'descend' into the deeper layers of the tree, until we reach a terminal node (no more branches). This terminal node is then the result of the decoding process. The item to be decoded may be a symbolic name, the tests being applied to successive characters in it, and the result may be an address on to which the name is mapped.

Search processes

As discussed in the previous paragraph, the organization of search processes is of extreme importance. Search processes are needed even in content-addressed systems which are capable of going directly to the required item on the basis of a specification. It is of the utmost importance, however, that in most cases it is not possible to provide a complete specification of the item required, necessitating the examination of a smaller or larger set of items, using some evaluation function. Here we assume that the evaluation function is applied by some other part of the total system, distinct from the memory system.

Essentially search processes can be divided into serial and parallel types. In serial search only one item is examined at a time. In parallel search several items are examined simultaneously, or even the whole contents of the storage system are examined in a single operation. To indicate the way in which this can be done, imagine that the internal representations are equipped with small radio receivers, and that the query specification is broadcast simultaneously to all of these representations. If the representations are themselves capable of matching the received specification to their own contents (so-called distributed-logic memories), then the whole contents of the memory system can be examined at once.

Similar remarks can be made as to the locus of processing power in the system. As we have just mentioned, it may be distributed over the whole system, or else it may be centralized into a so-called 'central processor', in which case the memory system is passive, and has no processing capabilities.

Distributed or lumped storage

In current computer systems internal representations are concentrated into small, well-defined regions of physical space. This is in contrast to biological systems, where the representations are usually spread over a considerable physical space leading to a distributed storage of information. A related issue is the question of a single or repeated representation of elements. Again, in biological systems a repeated representation is common, and one can guess at least two reasons for this. The first is the much discussed problem of reliability obtained by the duplication of elements. The second reason is what the author would like to call 'the wiring problem'. It is usually necessary to establish connexions between the internal representations. If there is only a single representation, then these connexions would necessarily have to run over considerable distances in some cases. On the other hand, if there is distributed storage with duplicated representations, then connexions can always be established by making short *local* connexions, since representatives of any item can always be found in any physical neighbourhood.

File organization

There are basically two distinct methods of storing items which are described by attributes. We shall call these direct and inverted organization. In the inverted organization regions of storage are set up, labelled with attributes, and the items to be stored are always put into the region corresponding to their attributes. With the direct organization the regions of storage correspond to items, carrying with them tags to indicate their attributes. Briefly, we can say that we either store items on attributes, or conversely, attributes on items. Notice that in the case of the inverted organization items are located in a multidimensional space. This of course is an abstract space and has nothing to do with the spatial organization of the storage system itself. The inverted organization leads to similar items being in the vicinity of each other in this abstract space. It is to be expected, therefore, that similar items would be retrieved together, and that retrieval errors would result in the retrieval of similar but incorrect items. With the direct organization there is no such systematic arrangement of the items.

Quite a considerable discussion of direct versus inverted file organization has

currently taken place in psychology. For example, the semantic theory advocated by Katz & Fodor (1963) implies a direct organization. Similarly, Yntema & Trask (1963) suggested such an organization in short-term memory. On the other hand, Osgood *et al.* (1957) and other mediation theorists assume an inverted file organization, i.e. the location of meanings in a semantic space. We would like to mention here that in information systems where pointers to items of information can easily be used it is now relatively easy to implement computer programs using list storage techniques and pointers which, from an abstract point of view, can behave as a system with either direct or inverted organization. (See, for example, the 'multilist' system, Prywes & Gray, 1962.)

Automata theory – a brief characterization

The main burden of this paper is to suggest that an alternative approach to psychological theories of human memory can be made from automata theory. This requires that at least some brief characterization of that theory be given. We shall not attempt an exposition or summary of this theory, since our aim here is a heuristic exploration of the key concepts and their usefulness in a theory of memory. Several good textbooks at various levels of mathematical sophistication exist (Moore, 1964; Minsky, 1967; Tou, 1968; Caianiello, 1966; Gill, 1962; Ginsburg, 1962; an extensive reference list is given in Moore, 1964).

The subject of automata theory is a relatively recent one, but it has gone through a very rapid development since about the middle of the 1950s. It forms a focal point for a number of disciplines like electrical engineering, linguistics, logic, computer science, and others. It should be remarked in particular that one precursor of automata theory was the work of McCulloch & Pitts (1943) on the logical description of processes in idealized nerve nets. Presumably that work was partly motivated by attempts at psychological explanation, and has given rise to a great deal of work in what was later called cybernetics and the theory of self-organizing systems. The concepts introduced in that work seem to have very little influence in current cognitive psychology. The reason for this is obscure, but may in part be due to the association of 'nerve nets' with physiology in the minds of psychologists. This is unfortunate, since automata theory is now quite an abstract mathematical discipline, just as neutral with respect to its field of application as, for example, statistics. What we are suggesting in this paper is an application of this theory in a particular area of cognitive psychology.

The elements: inputs, outputs and states

An automaton can simply be regarded as a 'black box' with input and output channels. The channels are capable of transmitting information.

Our interest is in describing the behaviour of the machine as a function of time. Time is treated in a discrete fashion, and the state of the system is ascertained only at specified discrete moments.

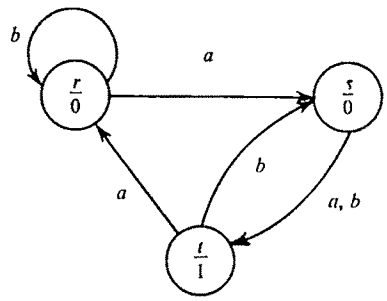
It is assumed that the machine (the black box) can be in one of a set of possible internal states. At the moment we are not inquiring into the way in which such a state is realized inside the machine. The study of the internal structure properly

States $S=\{r, s, t\}$
Outputs $O=\{0, 1\}$
Inputs $I=\{a, b\}$

Inputs

States	a	b	Outputs
r	s	r	0
s	t	t	0
t	r	s	1

Flow table



State graph

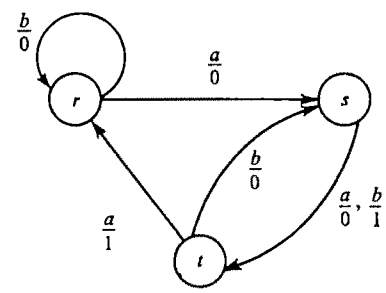
(*a*)

Inputs

States	a	b	a	b
r	s	r	0	0
s	t	t	0	1
t	r	s	1	0

Outputs

Flow table



State graph

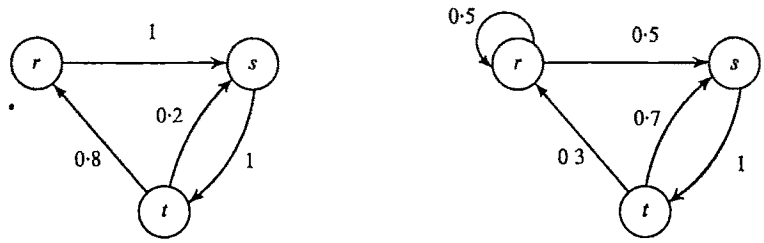
(*b*)

Fig. 1. Representations of automata. (*a*) Moore machine: produces outputs while in a state. (*b*) Mealy machine: produces output during transitions.

belongs to the discipline which makes use of the general theory. For example, it is the psychologist's task to specify the components, and interrelations between them, in a particular machine postulated for the purpose of studying a particular psychological process. If the number of these internal states is finite, the machine is called a finite-state machine or automaton.

The essential aspects of automata can thus be summarized using the concepts of states, inputs and outputs. At any given time the automaton can be in just one of the possible states. At these moments of time one of the possible inputs can be presented to the automaton. The state of the automaton is uniquely defined by the input and by the state of the automaton at the previous moment of time. The output of the automaton is determined by the current state of the automaton and by the current input (Mealy machine), or by merely the current state (Moore machine). The distinction between these two types of machines is that the Moore machine produces its output while it is in a state, while the Mealy machine produces its output during the transition between two states.

States $S=\{r, s, t\}$
Outputs $O=\{0, 1\}$
Inputs $I=\{a, b\}$



State graph for input a State graph for input b

		Inputs a			Inputs b				
		r	s	t	r	s	t		
States	r	0	1	0	0·5	0·5	0	0	Outputs
	s	0	0	1	0	0	1	0	
	t	0 8	0·2	0	0·3	0·7	0	1	
		$P(a)$			$P(b)$				

Fig. 2. Probabilistic automaton. In the flow table there are two transition probability matrices, $P(a)$ and $P(b)$. If the output is also probabilistic then two stochastic matrices, $Q(a)$ and $Q(b)$ would appear in the output part of the table.

In a more mathematical manner, an automaton can be characterized by five entities:

(1) A set of states S. (2) A set of inputs I. (3) A set of outputs O. (4) A transition function (or next-state function) $S \times I \to S$. (5) An output function $S \times I \to O$ (for a Mealy machine), or $S \to O$ (for a Moore machine).

The transition function maps the product of the state and input sets on to the state set; that is to say, given a state and an input it determines the next state to which the input 'drives' the automaton.

There are two common methods of describing an automaton: the transition (or 'flow') table, and the transition (or state) graph. For a simple three-state automaton these representations are shown in Fig. 1.

Stochastic automata

A stochastic automaton resembles an ordinary automaton in having inputs, outputs and states. However, the transition and output functions are no longer deterministic. A transition is governed partly by the input and partly by a set of probabilities (the so-called transition probabilities) associated with each state. There may be a

non-zero probability of going from state s_i to state s_j under the influence of an input. It is now necessary to specify a set of transition probability matrices (one for each input) to describe the transition function. Similarly, a set of state graphs is needed.

The outputs themselves may be probabilistically dependent on the states and inputs, in which case a set of stochastic matrices is also needed for the description of the output function.

For a three-state stochastic automaton the corresponding representations are shown in Fig. 2.

Further generalizations of the concept of an automaton are possible by relaxing some of the restrictions in its definition. This field is now undergoing an intensive development.

New approaches to memory models

Most theories of human memory are formulated at the structural level. It is generally assumed that there are two separate boxes, one labelled STM (short-term memory), and one labelled LTM (long-term memory). Some representative schematic diagrams can be found in work by Broadbent (1958), Waugh & Norman (1965), Murdock (1967), Bower (1967) and Shiffrin & Atkinson (1969). It can be seen from a perusal of these papers that all of these models incorporate an STM and an LTM 'box'. Information is assumed to be transferred between the STM and LTM systems, and a rehearsal loop is often inserted to provide recirculation of information in STM.

Some signs of a swing away from this structural conceptualization appear in the more recent work of Broadbent (1970), Norman (1968), Warrington (1970) and Neisser (1970). It should also be remembered that the structural STM–LTM distinction has been opposed for a long time by Melton (1963).

A structural schema based purely on the retention interval (short-term or long-term) has now become quite unsatisfactory, since in the case of many experiments we have to talk about the role of STM and LTM components irrespective of the retention interval, as Wright (1970) pointed out. Some alternative ways of thinking and talking about memory mechanisms seem to be needed.

As an alternative to making the assumption of separate structural components for short- and long-term storage, it is possible to start from the assumption that there is no separate memory 'box' at all, but that the different kinds of functional boxes (processes) all have some storage capabilities of their own. Even if one assumes that there is a memory system which is structurally separate from the processing apparatus, it can be argued that what are now called STM and LTM components are different functional aspects of the same memory system. This motivates the introduction of the terms primary and secondary memory by Norman (1968). The distribution of memory capabilities into the separate functional boxes, which are modality specific, is the approach adopted by Morton (1970), Warrington (1970), Warrington & Shallice (1969) and Sperling (1967). In a recent paper Broadbent (1970) also argues for a division of memory mechanisms into an addressing system and a long-term store (in addition to an iconic buffer) instead of the STM–LTM scheme.

It can be seen that ideas about the organization of human memory are in the melting pot. The notes which follow are to be taken as a contribution to this general fermentation of new ways of thinking about memory. The presentation is very

sketchy, and is meant to be heuristic rather than precise. Evidence for or against such a theoretical approach has not yet been surveyed in detail, and suggestions in this direction are welcome.

The state-space approach to memory

The approach to memory which is suggested here differs from other current theories in that it is not based on the conceptual background of the Von Neumann computer organization. Recall that the Von Neumann organization implies a location-addressed memory, which is passive, and is interrogated by a central processor through accessing individual storage locations. The other conception of memory suggested here is active, in the sense that the memory system itself has certain processing capabilities. It can undergo autonomous changes without the influence of a central processor, and it produces outputs in response to interrogation, rathei like an information-retrieval system, instead of merely providing a set of pigeon-holes into which information can be placed and later read out with complete certainty. We look upon memory as a subsystem within the overall information-processing system of the organism. We do not, however, assert that it is a completely separate system from, say, the perceptual subsystems. We are rather inclined to agree with Sperling (1967), Warrington & Shallice (1969), Neisser (1970) and Morton (1970) that the memory functions should be attached to the functional boxes of the human information-processing system.

At first we shall give a very concise characterization of the proposed theoretical approach, and then we shall illustrate it in the context of verbal memory.

(1) It is proposed that a memory system is an automaton. It communicates with other systems by receiving inputs and producing outputs. The memory system as a whole can be in a number of different states. The state of the system can change autonomously (when there is no input), and also under the influence of inputs.

(2) The 'contents' of the memory system are the different states. We shall say that a certain item of information is stored in the system if it has a state in which an appropriate output is produced. Placing information into the memory system (the 'read-in' process) consists in establishing appropriate states, and output functions.

(3) The retrieval mechanism of the memory system is the transition function of the automaton. Given a set of inputs, an item of information is retrieved from memory if the set of inputs drives the system (possibly after autonomous transitions) into the state appropriate to that item. If this state is then capable of producing suitable outputs, we say that the item has been recalled by the system. If the output is merely an indication that the state has occurred previously, then we say that the item has been recognized by the system.

(4) A search process corresponds to letting the memory system go through a sequence of states either autonomously, or under the influence of appropriate inputs. Notice that the outputs from the system at any given stage of the search can be used as the inputs in the next stage. This feedback loop, if monitored by some other subsystem, makes it possible to give direction to the search. A search process through autonomous transitions is completely determined by the transition function under null input.

(5) The transition function and the output function can both be modified by the action of other subsystems (through learning processes). These modifications are assumed to be permanent, or at least long-term changes.

(6) The state of the system is described by specifying the values of a set of 'state' variables. A physical representation of the state variables is the mechanism within the 'black box' of the automaton. The nature of the variables and their representation depends on the functional box to which the memory system is attached.

(7) The state variables define the so-called state space of the system. The intuitive picture of the functioning of the system is a representative point moving in a multi-dimensional state space. In this abstract space regions or neighbourhoods contain items (states) which are similar to each other. Moving the representative point of the system within this space takes both time and effort. The nature of the transition function determines the ease with which the system can move from one part of the space to another. The role of the learning processes (modifying the transition function) is to establish efficient trajectories (pathways) of transition sequences, so that certain items are easily accessible.

Word memory – an application

The preceding discussion has been in such abstract terms that the reader might now wonder how these concepts could find practical application. To illustrate this, we shall now briefly describe a memory system to deal with the storage and retrieval of words or similar verbal units. This brief description is based on a number of previous papers to which the reader is referred for further details (Kiss, 1969*a*, *b*).

The problem of word storage can of course be treated at several levels, ranging from the visual or auditory perceptual recognition to interpretation of word meaning. When we talk about a word store, we imply that in the *functional* sense there is a system concerned with the processes of word storage and retrieval, but in the structural sense a number of different mechanisms will contribute to the functional effects. For example, the auditory, visual and tactile perceptual mechanisms will all have a role in determining the overall properties of memory for words. In the present context we shall be concerned mostly with the level of word-form representation, which we assume to be somewhere between the perceptual and the semantic representations. The reader will find it easier to understand this stratification into levels if he bears in mind the abstract picture of an automaton (the word store) which has sub-automata (decomposition into component automata) doing part of the computation of the overall machine. For more discussion of the decomposition of automata, see, for example, Hartmanis & Stearns (1966), and for an interesting suggestion as to the usefulness of this concept in cognitive systems, Burstall (1969). The behaviour of the total system can be determined by describing the behaviour of the component subsystems. A schematic representation of an overall system, which should not be taken too seriously in its details, is shown in Fig. 3. It can be seen that the total system is made up from the series and parallel connexions of a set of sub-automata, interacting with each other. Not all of the possible connexions are shown in this diagram.

The state variables of each sub-automaton are regarded as the features relevant to the characterization of information at that level. At the level of phonetic features these may be the phonemes; at the level of articulatory features they may be syllables; at the level of visual features they may be letter or word shapes, etc.

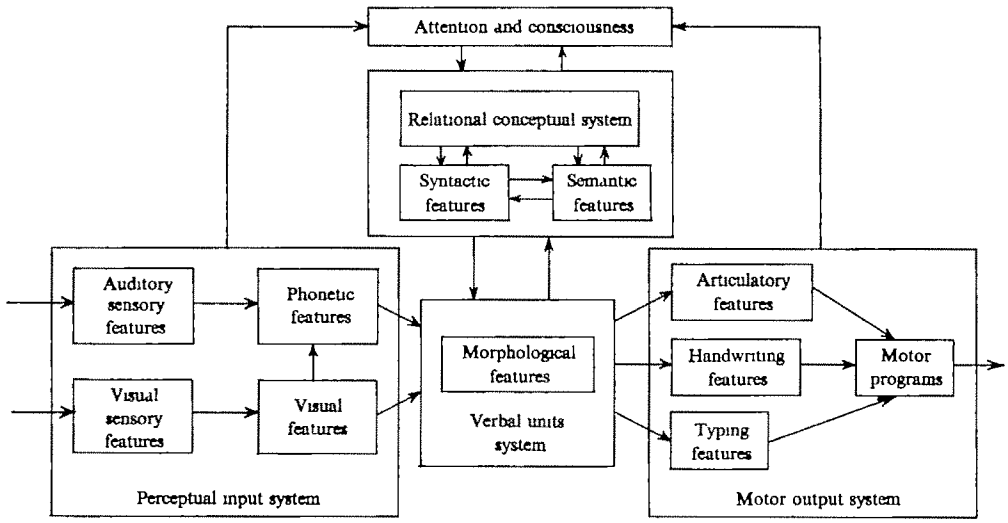


Fig. 3. A sketch of the sub-automata involved in verbal memory.

In the automaton labelled morphological features, the state variables are morphemes. The state of this automaton is determined by the current values of a set of variables, each corresponding to a morpheme. (To make this more palatable, suppose that the variable is something like a trace strength, or an activity level associated with the representation of that morpheme.) Similar remarks apply to the automaton enclosing this last one, labelled verbal units. These are words, or sequences of words, which commonly occur together.

As can be seen from the diagram, this automaton receives inputs from the perceptual input system, and produces outputs going to the conceptual system and to the motor output system.

The sub-automata in this scheme are not synchronous: for example, the transition rates of the input and output systems are relatively high compared with that of the verbal units or conceptual systems. By this we mean that several transitions of the input system may be required to drive the conceptual system into a new state.

Consider now, as an example, what happens when an isolated word is presented to this system. Suppose that the word is polysemantic, i.e. has several distinct meanings. Assuming an auditory presentation, this input is converted into a set of auditory sensory features by driving the relevant automaton into an appropriate state. The automaton acts as a pattern recognizer, and its outputs are the sensory features of the word. These outputs are fed into the phonetic automaton and drive it into the corresponding state. This means that the set of pertinent phoneme variables will take on a definite set of values. The output is a phonetic characterization of the word, and in turn drives the verbal-unit automaton into a certain state. In that state all the relevant morpheme variables take on a specified value. The outputs put the semantic and syntactic automata into the state which is determined by all possible interpretations of the input word. The most dominant one of these will determine the state of consciousness produced as the end result.

If the input is maintained (for example, by repeating the word over and over) the

phonetic, verbal, syntactic and semantic automata will go through transitions determined by the transition functions under constant input. If the phonetic automaton shifts to a different state, this causes transitions at the other levels. But even if the phonetic, verbal and semantic automata remain in the same state, consciousness will cycle through the possible interpretations of the word – a familiar phenomenon in satiation experiments.

If the input is removed, the subsystems will go through autonomous transitions, determined by the transition functions under no input. At the level of verbal units, subsequent states will be predictive of other verbal units which could occur in the vicinity of the stimulus, and similarly with the higher level systems. For example, in a word association experiment the output towards the motor system can be determined by autonomous transitions taking place at the phonetic, verbal-unit, semantic, syntactic, conceptual, or conscious levels, depending on the experimental setting, and on the subject's strategy. The model of the verbal-unit automaton described by the author (Kiss, 1969*a*, *b*) is reasonably successful in predicting word association responses from the autonomous transitions of the verbal-unit automaton.

As a second example of the application of the proposed approach to memory let us consider the free recall of word lists, or more generally, the emission of a sequence of verbal items which do not form connected discourse. Examples of such experimental paradigms are continued association response sequences with or without restrictions on the acceptability of response items. Using the terminology developed in the preceding sections, this kind of situation can be described as follows.

The task of the subject is to produce a sequence of outputs, and all of the outputs must belong to a certain set, defined by the instructions. In the case of free recall, this set is the set of words on the stimulus list. In the case of association sequences, it may be any word which seems relevant to the subject, or any word belonging to a certain class, like four-legged animals, names of cities, etc. In such an experiment the subject drives his word store through a sequence of states. The initial state of the sequence is more or less precisely determined by the conditions of the experiment. In the case of the continued association sequence, it is the state produced by the stimulus word, while in the case of free recall, it is the state produced by the presentation of the stimulus list. In the case of a taxonomic class, the starting state is probabilistically defined by the instructions.

The production of appropriate outputs is a cyclic process. Each cycle involves a transition of the word store, and a decision stage which determines the output in the current state. The output for some of these cycles may be null (i.e. all possible responses are suppressed as unsuitable). Both the transitions and the outputs are probabilistically defined. Within each state there are a number of possible candidate outputs with an appropriate probability distribution. There is also a probability distribution of the possible states to which a transition can be made autonomously, or under the influence of an input.

The examination of the candidate outputs, the outcome of the decision process, and the emission of an output all have feedback effects, i.e. they produce inputs for the word store. Other inputs are received from various components outside the word store in the cognitive system. By suitably regulating these inputs the subject is capable of controlling the trajectory of the transition sequence in the state-space.

By observing the output sequence certain properties of the state-space can be deduced. For example, the well-known clustering effects in free recall imply that the state-space has regions containing 'similar' items, and that the trajectory follows an orderly sequence from one such region to another.

Hopefully we have now said enough to illustrate the potential uses of state-space or automata theory in this area. Apart from the enrichment of our conceptual repertoire which this provides, it should be noted that there is a rapidly developing set of techniques for the mathematical and quantitative investigation of such systems. The exploration of the applicability of these techniques should now be attempted in detail.

This paper was first presented at the meeting of the Experimental Psychology Society, Cambridge, April 1970. Helpful discussions by Dr F. Craik and Mr H. Dewar are gratefully acknowledged.

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(*Manuscript received* 13 *August* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 343–358
Printed in Great Britain

THE DISTRIBUTION OF MANUAL ASYMMETRY

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Four different descriptions of lateral asymmetry are reviewed and their probable interrelations examined. Two main features are distinguished: first, a normal distribution of relative efficiency of the two sides which probably applies to all species subject to lateral differences; second, a factor specific to humans which induces a shift of the normal distribution toward dextrality. A brief discussion of the origins of asymmetry leads to the conclusion that the basic normal distribution probably depends on accidental variation while the human shift to the right may be a product of both cultural and genetic influences. The latter could imply that human right handedness is inherited while left handedness is not.

Human right and left handedness and pawedness in higher mammals have been described from several points of view and these descriptions have yielded apparently dissimilar distributions of lateral asymmetry. Four main types of description concern, first, differences between the sides in measures of skill; second, preference for each hand in common unimanual tasks, or for each paw in reaching for food; third, the distribution of types of preference in the population; fourth, the distribution of preference in families. It is the purpose of this paper to suggest how findings bearing on these problems might be interrelated. Through this exercise it is hoped to demonstrate which aspects of lateral asymmetry are fundamental and need to be accounted for in theories as to its origin.

A. Four descriptions of lateral asymmetry

A. 1. *The normal distribution of asymmetries of manual strength and speed*

At the instigation of Sir Francis Galton, some 7000 males attending the Health Exhibition of 1884 tried their strength by pressing a dynamometer with each hand. Analysis of this and other similar data over 40 years later by Woo & Pearson (1927) and Woo (1928) showed that differences between the hands in strength vary in a unimodal normal distribution in which the mean favours the right hand and the point of no difference between hands or equal strength takes its proper place in the sequence of differences, at about 0·44 standard deviations below the mean. Over a range of from 6 to 80 years no change with age was found in the extent or distribution of differences between hands.

Differences between the hands in time taken to move 10 dowelling pegs from one row to another have been measured in undergraduates and in school children (Annett, 1970*a*, *b*). The distribution of difference is unimodal and can be presumed bell-shaped as the number of individuals within each standard deviation of the mean approximate those expected under a normal curve. Within the school sample there was no change with age in the mean difference between hands. The mean differences between hands are given in Table 1 and the distributions are depicted in Fig. 1. There is a remarkable similarity between the school and university samples in the location of the point of no difference or equality between hands. The distance between

Table 1. *Left- minus right-hand times for peg moving (in sec.)*

	Males	Females	Total
School children 5–15 years			
n	76	97	173
Mean	0·60	1·31	1·00
S.D.	1·20	1·20	1·25
Z of point of equality between hands	0·50	1·10	0·80
Undergraduates			
n	71	79	150
Mean	0·32	0·84	0·60
S.D.	0·64	0·78	0·76
Z of point of equality between hands	0·50	1·08	0·80

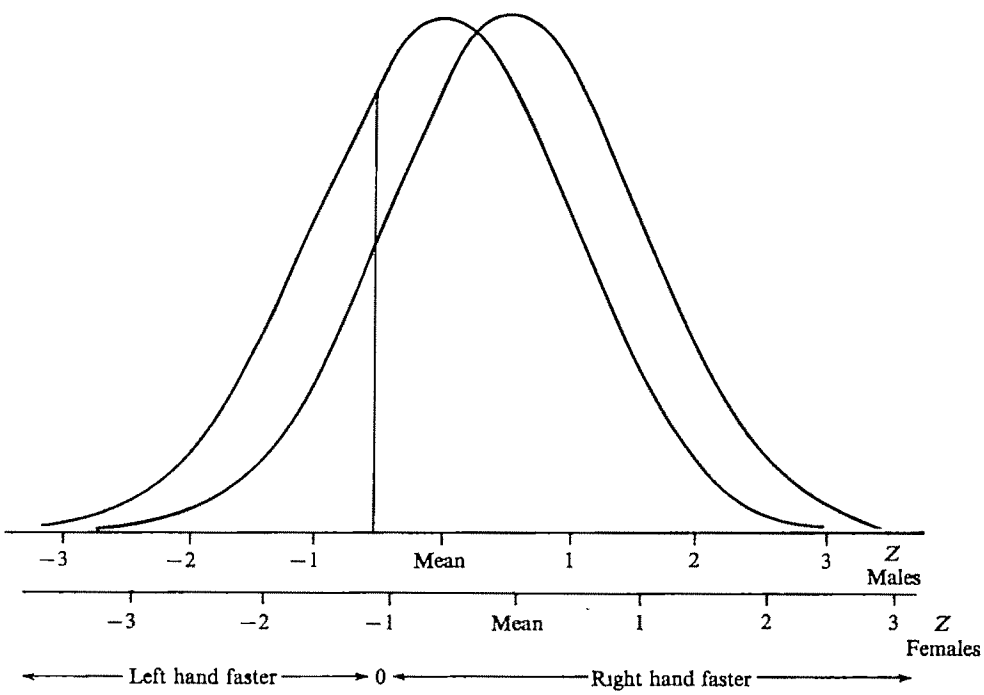

Fig. 1. Left- minus right-hand time for peg moving.

the mean and point 0 in schoolboys and male undergraduates is $0{\cdot}50\sigma$, which is close to the value for strength cited above; the distance for females in both samples is about $1{\cdot}10\sigma$. The difference between sexes, which is statistically significant in both samples, shows that females are more asymmetrical toward the right than males.

Left- minus right-hand speed was found to be systematically related to the number of actions performed with the left hand in both groups. In mice, Collins (1970) has shown that the grip strength of each paw, as tested by taping over one paw and allowing the mouse to grasp a wire mesh screen with the other while being picked up by the tail, is positively associated with paw preference. The distribution of differences in strength between paws was not described.

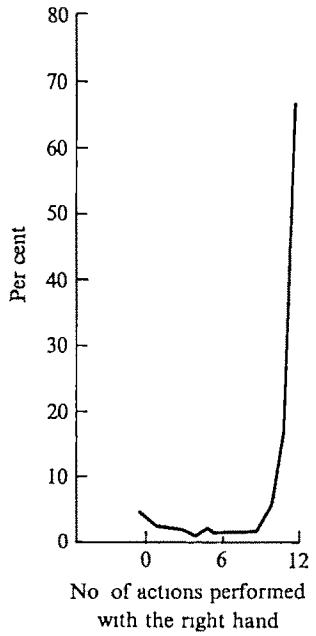

Fig. 2. Percentage of 241 recruits preferring the right hand for 0–12 actions.

A. 2. *The distribution of preference: J-shaped in humans and U-shaped in other higher mammals*

Hand preference in humans is usually assessed in terms of the number or type of skilled unimanual actions normally performed with each hand. When large numbers of subjects are asked about several actions and the percentages of individuals showing various degrees of preference calculated, the distribution is found to be J-shaped (Crovitz & Zener, 1962; Naidoo, 1961; Oldfield, 1969). Fig. 2 gives the percentages of male service recruits claiming to perform from 0 to 12 actions of a standard questionnaire (Annett, 1970*a*) with the right hand. The majority of individuals use the right hand for all 12 actions, several claim to use the left hand for one or two actions, very few claim equal preference for either hand and a small but slightly greater proportion are strongly or fully left handed.

Information of this kind has been used to characterize individuals according to 'indices' of left preference or according to classes based on various criteria. There is reason to believe that preferences vary continuously, just as degrees of asymmetry of skill (Annett, 1970*a*; Subirana, 1969). Comparisons of the sexes for preference often find more sinistrals among males (Burt, 1958; Oldfield, 1971) as would be expected from the sex difference in asymmetry of skill. A significant sex difference in preference is not always found (Dennis, 1958; Annett, 1967) but when present in normal samples is invariably in the direction of greater dextrality in females.

Collins (1968, 1969, 1970) has reported observations on reaches for food in mice, from both hybrid and highly inbred strains. Paw preferences were found in all groups and were consistent over time in individuals. In all samples the distribution of reaches was U-shaped, with 26–30 per cent of animals at each extreme (48 out of 50

Table 2. *Percentage of right, mixed and left handedness in human and non-human samples*

	Criterion of R or L (%)	*n*	Right	Mixed	Left
		(*a*) Human			
Undergraduates (Annett, 1970*a*)					
Male	100	967	66·6	29·0	4·4*
Female	100	599	65·1	30·7	4·2*
School children (Annett, 1970*b*)					
Male	100	99	61·6	32·3	6·1*
Female	100	120	75·8	20·8	3·3*
Parents of undergraduates (section A.4)					
Fathers	100	501	76·9	21·8	1·4*
Mothers	100	526	77·7	20·9	1·4*
		(*b*) Non-human			
Chimpanzee					
Finch, 1941	90	30	30·0	40·0	30·0*
Rhesus monkey					
Warren, 1953	90	84	27·4	45·2	27·4*
Ettlinger, 1961	90	42	14·3	45·2	40·5*
Milner, 1969, 1st measure	90	58	15·5	55·2	29·3*
Cat (Cole, 1955)	75	60	20·0	38·3	41·7*
Rat (Harper, 1970)	100	149	20·1	53·0	26·9*
Mouse (Collins, 1969)	*c.* 88	858	26·3	43·8	29·8

* Samples in binomial proportion.

consistent reaches) and only 2·6 per cent of animals in the central interval (equal numbers of right and left reaches). The distribution is continuous and the classification of animals into right- and left-pawed groups must be as arbitrary as it is for humans.

A. 3. *The binomial distribution of right, mixed and left handedness*

Most classifications of individuals according to handedness use only two categories, right and left. Annett (1964) suggested that several puzzles about laterality, and especially about the phenomenon of cerebral dominance for speech, might have arisen from a failure to distinguish consistent from inconsistent handers. The majority of self-styled 'left' handers are known to be inconsistent (Humphrey, 1951), and if these individuals are of mixed hand and speech laterality, the failure of 'left' handers to mirror the findings for right handers would be accounted for. As it is relevant to later sections of this paper it must be made clear that the main point of that analysis (Annett, 1964) was to suggest that whereas the majority of right handers have consistent dextral preference and left-hemisphere representation of speech, the majority of 'left' handers reported in the literature probably have inconsistent preferences and a facility for developing speech in either hemisphere. Consistent sinistrals, those as left handed as most dextrals are right handed, have been distinguished from other sinistrals so rarely that there is no firm evidence as to their cerebral speech laterality; it was suggested that this could be typically right-

Table 3. *Handedness in families*

Parents	♂×♀	*n*	Children (per cent) Pure right	Mixed	Pure left	Left-handed writing
Both right	R×R	431	67·3	28·5	4·2	11·6
One mixed	M×R	108	61·1	37·0	1·9	13·9
	R×M	123	47·2	46·3	6·5	15·4
Both mixed	M×M	85	37·7	52·9	9·4	25·9
One left	L×R or M	49	73·5	16·3	10·2	24·5
	R or M×L	71	47·9	32·4	19·7	39·4
Both left	L×L	47	48·9	27·7	23·4	40·4

sided. The main features of this analysis were embodied in a genetic model which proposed that consistent right and left handers might be dominant and recessive homozygotes and mixed handers heterozygotes of a single gene pair.

Subsequent work on the distribution of hand preferences in several samples of undergraduates and school children, and in reanalyses of data in the literature, revealed that whenever handedness is classified into the three types proposed, consistent right, mixed and consistent left handers always occur in the binomial proportions to be expected if handedness were dependent on a pair of factors randomly combined. Table 2 lists data for some human and animal samples and in all but the very large sample of mice there is good fit between the observed and expected numbers on the hypothesis of binomial proportions (Annett, 1967). This is true despite wide variation between samples in the percentages of each preference type. It will be shown below that the single gene pair hypothesis is quite unable to account for handedness in families, and the binomial proportions cannot be due therefore to a random combination of a factor for right and a factor for left handedness. Some other explanation must be sought for their repeated occurrence.

A. 4. *The distribution of handedness in families*

As previous studies have not separated mixed from left handers and have varied in the proportion of mixed handers called 'left', it was not possible to test the genetic hypothesis mentioned above against earlier findings. A test required the classification of both parents and children as consistent right, left or mixed handers and demanded that both generations complete the same questionnaire. Sets of questionnaires (Annett, 1970*a*) were sent to the homes of all new university entrants just before the start of each of two sessions; about two-thirds of those canvassed returned the forms and about one-quarter of these were unusable because one or both parents were unable to answer, or because a parent or student was a twin. Pure left handers among the undergraduates and their parents are relatively few, as expected. In order to gather more information on left handers, and especially families in which both parents are sinistral, appeals were made through the press and radio. All respondents were sent sets of questionnaires and those returning complete information for both parents and children (excepting twins) are included in Table 3.

The data in Table 3 does not represent a population sample. It is a combination of

data from several sources and may be biased in several ways. Two likely sources of error are, first, a greater tendency of left and mixed than right undergraduates to return family data and, second, a greater tendency of mothers than fathers to respond to appeals for information. Estimates of the incidence of right, mixed and left handedness in fairly complete samples were reported by Annett (1970*a*) (67·8 per cent right, 28·9 per cent mixed and 3·3 per cent left in 460 psychology undergraduates).

Despite the possible biases of Table 3, there are several points which can be made with some confidence. First, as mentioned above, the single gene pair hypothesis is clearly insufficient as it required all the children of right × right parents to be dextral and all the children of left × left parents to be sinistral (when right and left include only consistent handers); this is true of about two-thirds of children in the first type of family and about a quarter in the second.

A second feature of Table 3 is the suggestion that mixed or left handedness in the mother is associated with a greater incidence of filial mixed and left handedness than these types of handedness in the father. A similar trend is to be found in the data of Chamberlain (1928), Rife (1940) and Falek (1959). In another set of student data where ascertainment of parental handedness depended on filial report, there were 12 per cent left-handed writers among the children of 107 left-handed fathers and 33 per cent among those of 88 left-handed mothers. A further test of this difference between parents was made through the local press when left-handed parents were asked for information about their children's handedness; 19 per cent of 104 children over five years of age reported by 48 left-handed fathers were said to be left handed and similarly 30 per cent of 128 children of 52 left-handed mothers. The excess of left handers in the families of left-handed mothers is statistically significant in both sets of data.

The third point is that handedness is a familial condition in the sense that the incidence of consistent right handedness tends to be lower and consistent left handedness higher when there is parental mixed and left handedness. It is not possible to make detailed comparisons between these findings and those of earlier workers on account of the different criteria of left handedness, but bearing in mind that Rife's criteria would include those here called 'mixed' and Chamberlain's would be closer to those here called 'left', it is possible to see that the present results agree fairly well with those of both previous studies. It must be emphasized that the present work is the first to report handedness in families where both parents are consistent left handers in the strict sense used here. Whereas pure left handers are between 3 and 4 per cent of the general population they are almost a quarter of the children of two pure left-handed parents.

In rats there is no evidence that pawedness is a familial condition (Peterson, 1934). Collins (1969) found wide variation in paw preference in strains of mice which had been maintained by brother–sister matings for over 90 generations. Three generations of selective breeding for laterality did not affect the distribution of preferences.

B. INTERRELATIONS

Four sets of facts about lateral asymmetry have been described above and it is now proposed to suggest how these facts might be interrelated. It will be necessary to superimpose one distribution on another in ways which should not be interpreted

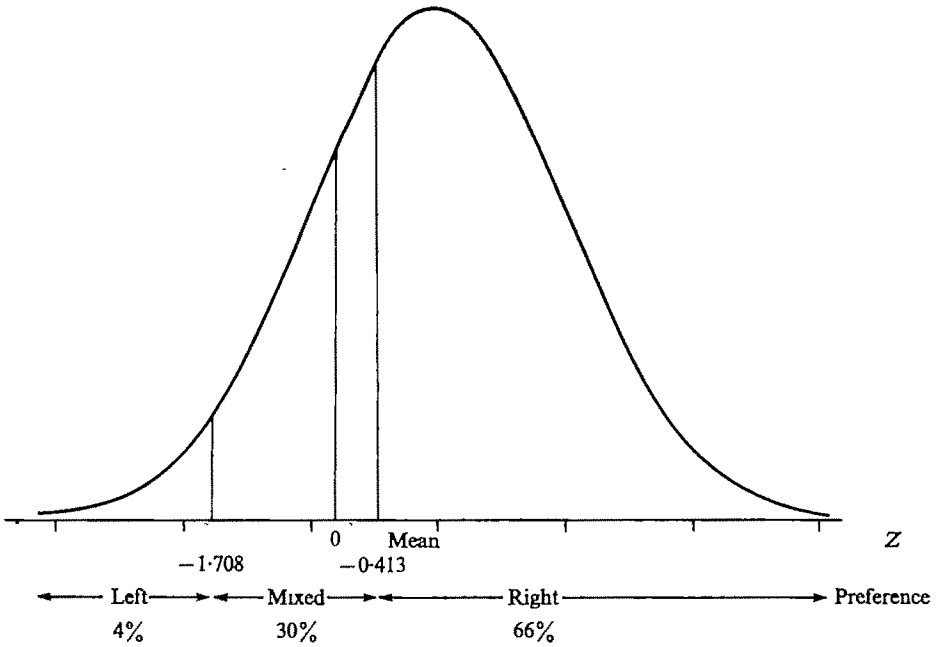

Fig. 3. The probable relation between the normal distribution of asymmetry of skill and right, mixed and left preference classes in man.

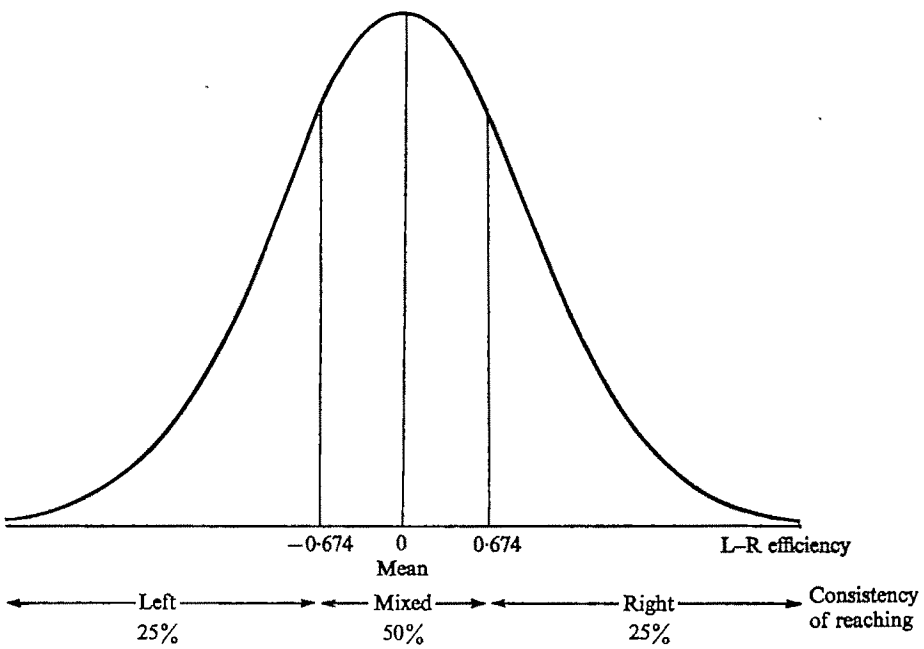

Fig. 4. The probable relation between asymmetry of skill and preference classes in non-human mammals.

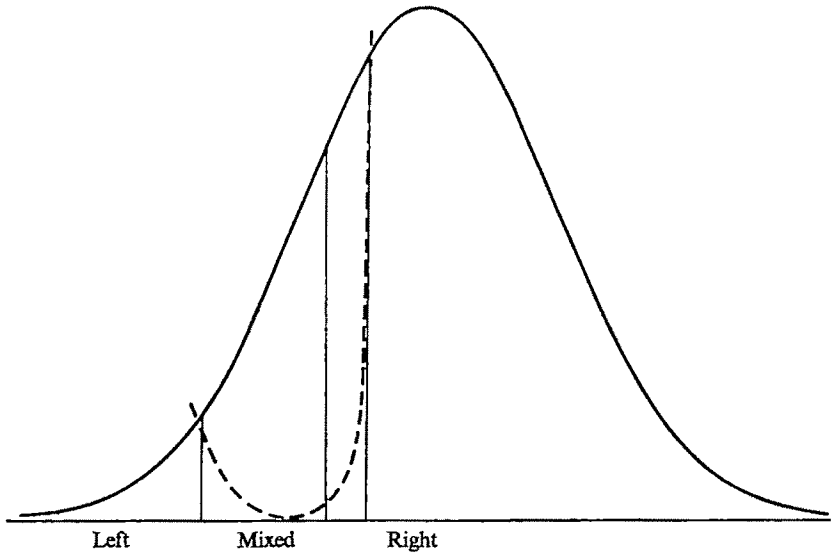

Fig. 5. The probable relationship between asymmetry skill and the J-shaped preference distribution in man.

literally or concretely but as demonstrating, in principle, how they probably arise from one another. Some overlap of subgroups would be anticipated in any empirical investigation.

B. 1. *The normal distribution of skill and the classification of preference*

It has been shown above that degrees of asymmetry of manual speed are closely correlated with degrees of manual preference and relation of paw strength with number of reaches. It is a question of some importance how the normal, unimodal distribution of differences in skill is related to classes of preference, whether right/left or right/mixed/left, or any further subdivision.

For the present purpose, asymmetry of manual speed can be represented for both sexes together in one distribution as in Fig. 3. At about the point of no difference between hands (0·8z below the mean) there must be many individuals whose hand speeds do not differ significantly. Assuming that mixed hand preferences are most likely to occur in these individuals whereas those some distance to the right of the point 0 are likely to be consistently dextral, and those some distance to the left consistently sinistral, the probable boundaries of the region of mixed handers (in undergraduates) would be those drawn in Fig. 3. The boundaries would be similar but slightly to the left for the parents of undergraduates.

Incidences of 'left' handedness reported in the literature range from 1 to 34 per cent (Wile, 1934; Hécaen & de Ajuriaguerra, 1964). Inspection of Table 2 and Fig. 3 shows how this range depends both on the sample and on the size of the segment cut off the end of the normal distribution; it depends on whether consistent sinistrality is demanded as at the lower boundary of the mixed region for parents, or a total absence of left preference as at the upper boundary of this region for undergraduates.

In animals, the distribution of asymmetry of strength can be represented as in

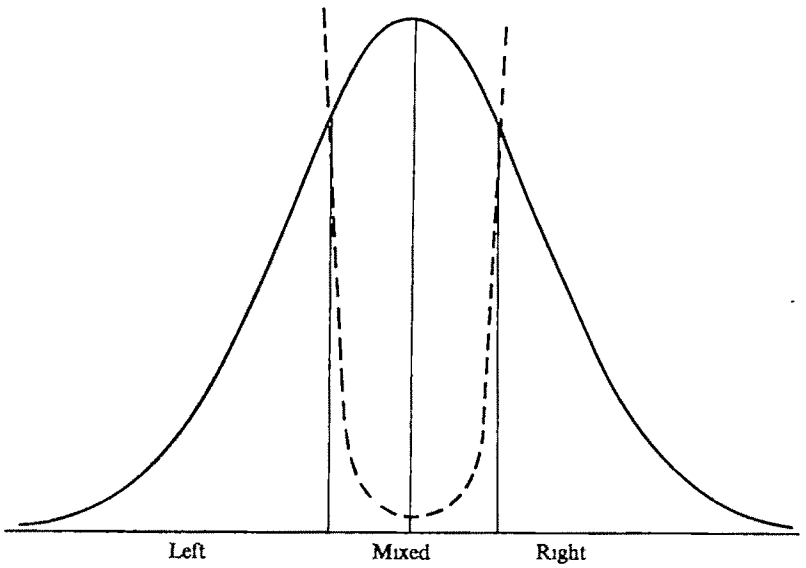

Fig. 6. The probable relation between asymmetry of skill and the U-shaped preference distribution in non-human mammals.

Fig. 4. Although species vary in incidence as shown in Table 2, all have a higher than human incidence of left preference; the incidence of mixed preference is usually higher than that for either consistent right or left usage. The probable distribution is represented in an idealized form in Fig. 4 with the mean and point of no difference coinciding and the proportion of left-, mixed- and right-pawed individuals as those required to satisfy binomial proportions, 25, 50 and 25 per cent respectively.

B. 2. *The normal distribution of skill and the* J- *and* U-*shaped distributions of preference*

The right handers represented as consistent in Fig. 3 would, by definition, perform all significant unimanual actions with the right hand and all the left handers similarly for the left hand; all variability of preference depends on the mixed handers. When degrees of left-hand usage are plotted as in Fig. 2, all intermediate points between the two extremes are filled by mixed handers. The J-shaped distribution of preference can be superimposed on the normal distribution of asymmetry as in Fig. 5. Although mixed handers are more numerous than consistent left handers they are distributed over a range of scores, hence the smaller number of individuals at most intermediate points.

In animals, by the same argument, the distribution would be expected to be U-shaped as shown in Fig. 6. Although about 50 per cent are not completely right- or left-pawed, they are distributed over a range of scores whereas all of the 25 per cent right-pawed animals and 25 per cent left-pawed animals fall at the extreme points of the range.

B. 3. *The normal distribution and the binomial proportions of preference*

One of the main questions raised by the discovery that right, mixed and left handers occur in binomial proportions (when $n = 2$) is how this can be true of several species, which differ markedly in the incidence of each type, when the single gene pair hypothesis which could have accounted for this observation is untrue. Part of a possible explanation is to be seen by inspection of the distances between the two lines drawn to separate the left from mixed and the mixed from right handers in Figs. 3 and 4.

Considering ideal cases first, the incidence of left, mixed and right handers approximates in animals 25, 50, 25 per cent, and in humans 4, 32, 64 per cent. The distances along the abscissa represented by the middle terms are $1{\cdot}35\sigma$ and $1{\cdot}39\sigma$ respectively (the x values of the normal probability function). In the real samples reported in Table 2 the corresponding x values for mixed handers are $1{\cdot}29\sigma$ for undergraduates and $1{\cdot}46\sigma$ for their parents (sexes combined). The x value of the middle interval rises from a minimum of $1{\cdot}35\sigma$ as p and q deviate from 0·5, but the rise is very gradual until p becomes extreme (for $p = 0{\cdot}999$ the middle interval is $1{\cdot}875\sigma$ and for $p = 0{\cdot}999999$ it is $2{\cdot}426\sigma$).

This relation between the binomial expansion and the normal probability function implies that if mixed handers are a class with boundaries a constant distance apart in all species, and this distance is in the region $1{\cdot}35\sigma$, the recurrence of binomial proportions despite differences in incidence would be accounted for. No explanation can be offered at present for the empirical observation that the distance between pure left and pure right handers under the normal distribution is about constant and such as to fit the middle term of the binomial expansion.

These observations suggest that in all samples, human and non-human, the distribution of lateral asymmetry differs in only one significant respect, the extent to which the distribution is shifted as a whole to the right. The overall distribution is always bell-shaped and the distance between consistent left and right handers is constant; in so far as the distribution is pushed as a whole toward dextrality, the incidences of left and mixed handedness diminish, but always to an extent compatible with their position in the total normal distribution.

B. 4. *The distribution of skill and preference in families*

The finding that mixed- and left-handed children are more numerous in the families of left-handed mothers than fathers can be considered in relation to the sex difference of relative manual skill. In Fig. 1, females were shown as more asymmetrical to the right than males. If the mechanisms responsible for handedness are the same in both sexes but the distribution for females shifted further to the right (by unidentified modifying factors), females manifesting left handedness would be expected to be more strongly genotypically sinistral, as a whole, than left-handed males. Alternatively, it could be said that left-handed females have less of the factor which induces the shift of the distribution toward the right. If the factors responsible for the shift to the right depended even partially on genetic mechanisms, a greater incidence of left handedness in the children of left-handed females than males would be anticipated. This

Facts	Sources of variation A. Genetic	B. Accidental	C. Social
1. Normal distribution of asymmetry of skill in human and non-human species	Not in mouse or rat	Possible	Unlikely
2. Shift to right in human but not other animals	Possible	Unlikely	Possible
3. Binomial proportions of right, mixed and left preference in all species	?	?	Unlikely

Fig. 7. Possible explanations.

interpretation of the facts resembles that suggested by Carter (1961) for the disorder pyloric stenosis.

C. Implications for the origin of lateral asymmetry

This analysis of manual asymmetry distributions suggests that there are two main features to be explained; first, the normal bell-shaped distribution of relative skill, and secondly, the shift of the distribution in humans toward the right. All other aspects of manual asymmetry, including the binomial distribution of right, mixed and left handedness, depend on these two fundamental characteristics. Whereas previous discussions of handedness have sought an explanation for a supposedly single characteristic, this analysis suggests that there may be two processes involved, one universal to all species subject to laterality and one specific to human beings. During human development these two factors may operate independently or in conjunction, but at this stage of inquiry, it is necessary to consider the possible basis of each of the factors independently.

Variations between the members of a species can arise from three main sources: they can be transmitted genetically, they can depend on factors operating accidentally in the course of development and they can be transmitted culturally. The work on laterality in families appears to show that human handedness, considered as a unitary character, cannot be accounted for by any simple genetic mechanism. If human handedness depended on two independent mechanisms either one or both could have a genetic foundation.

Accidental phenotypic variation can be envisaged as arising in several ways. The development of the two sides of the body may be directed initially by identical sets of genetic instructions but there could be minor variations in the translation of these instructions in the course of morphogenesis. Such accidents would be equiprobable on the left and the right and would lead to some individuals having a more efficient nervous, muscular or circulatory system on one side or the other, while the majority of individuals would be relatively symmetrical. Glees & Cole (1951) point out that individual differences in the proportion of crossed and uncrossed fibres in the spinal cord might be related to limb preferences; such variation is presumably of accidental origin. Other accidental effects might be determined by factors operating *in utero* such as the position of the placenta, the effects of gravity during mother's activity or rest, or the presence of a twin. Accidental variation might arise also from the early handling of the child (see discussion by Burt, 1958).

Cultural influences on lateral variation include all effects which depend on the

presence of other human beings, through unconscious or deliberate imitation, through specific training or practice of skills on one side and through effects of prejudice against the use of the left hand. These three sorts of influences – genetic, accidental and cultural – must be considered as possible explanations of the two essential characteristics: the normal distribution and the human shift toward the right hand. A third feature, the binomial proportion of right, mixed and left handers, is also worth considering from the causal point of view. The three main facts and the three main possible explanations are set out in Fig. 7.

C. 1. *The bell-shaped distribution of asymmetry of skill*

This is the fundamental characteristic of asymmetry, which probably applies equally to all species subject to lateral differences. A continuous bell-shaped distribution suggests the operation of several factors, each of small effect and in random combination. Such a distribution can depend on either multifactorial genetic mechanisms, or on accidental congenital variations. It seems unlikely that it could be a product of cultural factors alone as these would be biased toward producing right and not left handedness; cultural influences may affect the normal distribution but could not be responsible for its origin.

The distribution of pawedness in mice, shown above to probably depend on a normal distribution of asymmetry of skill, cannot be changed by selective breeding. Laterality in non-humans is not a familial condition in the genetic or the cultural sense. It would seem that the normal distribution of asymmetry in animals should be attributed to the effect of random accidental variations probably operating during embryonic growth. As mentioned above, such congenital variations would be expected to produce approximately equal numbers of right- and left-pawed animals, with the majority between these extremes; animals with a more efficient neuromuscular system on one side would be consistent in the use of that side, the majority would vary in the paw used for reaching but relatively few would be expected to reach equally often with both paws.

In humans, two things must be accounted for: the normal distribution and the shift to the right. It is possible that the normal distribution has the same origin as suggested above for non-humans – accidental congenital variation. The shift to the right cannot be accidental but it is possible that it affects a distribution which is basically a product of the same sorts of congenital variation as in non-human animals. It seems reasonable to assume, at this stage, that the normal distribution of lateral asymmetry in all species, human and non-human, is probably a product of accidental variation. It is unlikely to be a product of genetic or cultural factors.

C. 2. *The human shift to the right*

It is possible that in some animal species the bell-shaped distribution is shifted toward dextrality or toward sinistrality, but in none is this movement of the whole distribution as clear as in man, where the distance between the mean and point of symmetry is about one half a standard deviation in males and about one standard deviation in females. A systematic shift of a whole population cannot be due to chance; the congenital accidents considered above to be responsible for the shape of the total distribution cannot be responsible for the displacement to the right. Both

genetic and cultural influences have strong claims to a role in the inducement of human beings toward dextrality.

Cultural factors, whether involving deliberate training or incidental to life in a right-handed society, would be expected to induce most of those who are equally capable of using either hand (about 50 per cent on the accidental variation hypothesis) to develop the use of the right hand for skilled activities. Only strongly left-handed individuals (about 25 per cent) would continue to show some tendencies to left-hand usage and the extent of resistance would, of course, increase toward the sinistral end of the distribution. Variations in the incidence of left handedness between human societies and between generations (as between undergraduates and their parents) within any society could be attributed to differences in the severity and the consistency of pressures toward dextral conformity. The fact that not all can conform and that a few intractable left handers persist in all groups would depend on the existence of individuals so far to the sinistral end of the normal distribution that their left-hand superiority could not be countermanded by social forces. There can be no doubt that cultural pressures have some role in the shift of the human distribution to the right.

Against the supposition that cultural factors alone are responsible for the right shift, it may be suggested that if human populations were, at birth, equally balanced between tendencies to right- and left-hand usage, it is extraordinary that no society has ever been found which favoured left handedness; if accidental variation and a cultural shift were all that were required, at least some societies should have adopted left handedness as the norm. It was seen above that left handedness has some association in families, although the effect is slight. It might be argued that this finding could be an effect of familial imitation and training superimposed on a basic accidental normal distribution, but it could also be due to inherited variation in a genetic factor responsible for the right shift. There are no certain means of distinguishing these possibilities at present.

The suggestion that a genetic factor may be involved in the shift toward dextrality but not in the origin of the basic bell-shaped distribution which underlies all lateral asymmetry has the paradoxical implication that right handedness may be inherited while left handedness is not. Left handedness can be thought of as the result of accidental factors which endow the left upper limb with greater skill, together with a weak dose of the factors which induce most humans toward dextrality. Right handedness can be thought of as a result of accidental factors giving greater skill to the right limb or to a combination of relative bilateral symmetry and the presence of a factor inducing dextrality.

If it is supposed that when the 'right shift' factor is present, those inclined through accidental variation toward mixed handedness would manifest right handedness and those similarly inclined toward left handedness would manifest mixed handedness, the presence of this factor in about 0·8 of the population would account approximately for the observed incidences of the three varieties of preference. (Right $= 0{\cdot}25$ (acc. r.) $+ 0{\cdot}8 \times 0{\cdot}50$ (acc. m.) $= 0{\cdot}65$; mixed $= 0{\cdot}2 \times 0{\cdot}50$ (acc. m.) $+ 0{\cdot}8 \times 0{\cdot}25$ (acc. l.) $= 0{\cdot}30$; left $= 0{\cdot}2 \times 0{\cdot}25$ (acc. l.) $= 0{\cdot}05$.) On this analysis, the 'right shift' factor would be present in 92 per cent of phenotypic right handers and 67 per cent of mixed handers; it would be absent in pure left handers.

In the absence of the 'right shift' factor, the distribution of human handedness would be expected to resemble that for non-humans; that is, right, mixed and left handedness should approximate the proportions $\frac{1}{4}$, $\frac{1}{2}$, $\frac{1}{4}$. It was shown that in the families of two pure left-handed parents almost one quarter of the children were consistent sinistrals as expected on this hypothesis. But the proportions for right and mixed handers were the reverse of those expected. Considering the writing hand, it would be expected that in the absence of 'right shift', about half the children should be right- and half left-handed writers. In the present sample 40 per cent of the children of left × left parents were sinistral writers; in Chamberlain's (1938) data the corresponding incidence was 46 per cent. Thus, the hypothesis of accidental variation and absence of 'right shift' comes very close to accounting for the findings for left × left families. Instead of an even balance there is a slight excess of tendencies toward dextrality. This may be due to a persistence of 'right shift', albeit at a minimal level, perhaps attributable to the presence of some pathological left handers among the parents, or to the effect of cultural pressures toward dextrality.

So far, the nature of the factor inducing the 'right shift' has not been discussed. An important fact about this shift is that it is greater in females. Unless it is to be argued that females are more subject to cultural pressures than males, this sex difference suggests that the bell-shaped distribution is modified by factors which are sex-linked in the genetic sense. Another well-established difference between the sexes which applies particularly during early stages of development concerns the rate of growth and efficiency of certain language functions such as vocabulary size and reading skill (Clark, 1970; Kimura, 1963; McCarthy, 1954). The advantage of girls in early development of these verbal skills together with their greater dextrality suggests that the shift to the right and language development may both depend on a common factor and that this factor may be related to whatever induces the left cerebral hemisphere to serve speech in the majority of the human species.

In section A.3 above a two-factor model was described to account for the fact that while some individuals are strongly inclined toward the lateralization of speech in the left hemisphere, others are capable of developing speech on either side. The present analysis suggests that one factor alone is sufficient; in the presence of this factor there is a tendency toward left-hemisphere speech representation and toward right handedness; in its absence, speech may depend on either or both hemispheres. The suggestion that phenotypic hand preference depends not only on the presence of the 'right shift' factor (now tentatively identified with whatever induces speech in the left hemisphere) but also on the effects of accidental variation is sufficient to account for the lack of one-to-one correspondence between manual and speech laterality. If this is the case, the values suggested above for the presence of the 'right shift' in phenotypic right and mixed handers would agree with calculations as to the frequency of left-hemisphere representation of speech in right and mixed handers (Roberts, 1969).

C. 3. *The binomial proportion of right, mixed and left preference*

The fact that the three main varieties of preference occur in binomial proportions in both humans and animal samples despite marked differences in the incidence of each type can be interpreted, as shown above, as a corollary of the movement of a bell-shaped distribution toward dextrality, when the boundaries of the class of mixed

handers remains a constant distance apart. This observation strengthens the interpretation that laterality depends on a bell-shaped distribution of differences between the sides whose mean can be shifted to the right (and presumably to the left in some species) of the point of symmetry or no difference. What is added by the persistence of binomial proportions in these samples is the suggestion that individuals within a relatively fixed distance of the point of symmetry on either side will be found to have mixed preferences. The proportion of mixed handers falls as the distribution is moved as a whole toward the right but always to an extent compatible with the area under the normal curve represented by about $1{\cdot}35\sigma$. This regularity supports the belief that phenotypic hand preference is a function of an underlying distribution of relative skill. Hand usage may be modified by cultural pressures but social influences alone could not account for the occurrence of three types of preference, always in proportions which are a function of distance from the point of symmetry in a normal distribution.

Conclusion

Genetic, accidental and cultural factors all have a role in the development of handedness, not in an indiscriminate admixture but according to a recipe whose main outlines can be discerned. The basic characteristic of laterality is a normal distribution of differences between the sides; this probably arises from accidental influences on early development. A chance combination of factors which incline one side to be more efficient than the other, both sides having an equal chance of being favoured, is sufficient to account for the basic distribution of lateral differences in the human and non-human species.

In addition there is a factor specific to humans which inclines the distribution as a whole toward greater skill on the right. This factor probably has a genetic foundation but its effect could be accentuated by social pressure toward conformity with the dextral majority. The factor inclining most human beings toward the use of the right hand is probably linked with whatever underlies the tendency of most human beings to develop speech in the left hemisphere. In the analysis presented in this paper, it has been necessary to distinguish the two basic influences on human laterality in order to show that they may have independent origins. How the two interact together in the course of human development remains to be worked out; it is evident, however, that the operation of two independent factors, albeit combined by chance, could give rise to some conjunctions of hand and speech laterality which were less advantageous than others, for the development of skilled hand usage and language.

If it is the case that human handedness depends on two factors – one accidental and congenital but non-genetic, and the second possibly genetic but modified by cultural influences – it is not surprising that the causes of lateral differences have been difficult to elucidate. In so far as inherited factors are involved, it is possible that they produce right handedness and left-hemisphere speech representation but not left handedness and right-hemisphere speech representation.

I should like to thank all those whose cooperation in returning completed handedness questionnaires and performing the peg task has made this analysis possible. I am grateful to Dr C. E. Blank, and to Professor T. Lewis for discussing with me aspects of this work. I am indebted to Professor A. D. B. Clarke for facilities to work in his department and to the Medical Research Council for financial support.

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(*Manuscript received* 8 *June* 1971)

Br. J. Psychol. (1972), **63**, 3, *pp.* 359–367
Printed in Great Britain

THE REGRESSION TO RIGHT ANGLES TENDENCY AND THE POGGENDORFF ILLUSION. I

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The theory that 'angle' visual illusions like the Zöllner and the Poggendorff are due to the lines forming the sides of angles being deflected in such a way as to make the angles nearer to right angles, was tested by obtainmg judgements of the orientation of both lines in simplified versions of the Poggendorff and Zollner illusions. The theory was confirmed, although the deflexion was only slight in the cases of lines in a vertical orientation.

The theory that a number of visual illusions, such as the Zöllner, Ehrenstein and Poggendorff illusions and the Wundt and Hering figures, are due to a tendency to overestimate acute and underestimate obtuse angles has been held for nearly a century. Various reasons as to why subjects should show this regression to a right angle tendency have been put forward. Wundt (1893) explained it in terms of eye movements and Helmholtz (1886) in terms of irradiation. Both writers also considered the possibility that it was a manifestation of contrast or dissimilation. Sanford (1897) suggested an explanation in terms of perspective. The irradiation and contrast explanations have not lacked supporters in recent times (e.g. Virsu, 1967, 1968; Chiang, 1968; Glass, 1970) and a Gestalt assimilation of the effect to an orthogonalization tendency and therefore to Prägnanz has been put forward by Rausch (1966). Developments in neurophysiology have, however, been the main source of new ideas concerning the effect. Ganz (1966) and Burns (1968) have explained it in terms of lateral inhibition between neurones in the retina or lateral geniculate nucleus, whilst Andrews (1964, 1965), Bouma & Andriessen (1970) and Blakemore, Carpenter & Georgeson (1970) hold that the inhibition takes place between orientation detectors in the striate area. But to the authors' knowledge, apart from the study by Blakemore *et al.* (1970), no evidence has been presented for the existence of this tendency by those advancing this explanation, other than what the illusions themselves manifest.

But 'what the illusions manifest' is not in itself sufficient since, in cases like the Zöllner or the Ehrenstein illusions or the Wundt or Hering figures, the illusion could be due to the interaction of the two parts of the figure, the distorting and the distorted parts seen as wholes, and may not operate at the level of individual isolated angles at all. Such a view is implicit in Gregory's inappropriate constancy scaling theory with its associated notion of a typical perspective position. As for the Poggendorff illusion, this does not even manifest the regression to right angles tendency. We do not see the short transversal line, upon whose distortion the illusion is said to depend, as bent so as to make a larger angle, as we do in the other illusions see the parallels as bent. Of course, if the transversal *were* bent in such a way as to make a larger angle, then the illusion would follow, but we cannot tell whether this is so except by experimenting. Certainly, there have been psychologists, such as Köhler & Wallach (1944) and Tausch (1954), who did not believe that the illusion was due to a regression

to right angles effect in the perception of angles. It is important to emphasize this point because attempts have been made to determine whether contours will be displaced so as to form a greater angle with one another by using what is in effect a form of the Poggendorff illusion. This, for example, is the reasoning applied by Bouma & Andriessen (1970), whose method of measuring is to vary the position of a dot until it appears to 'lie on the extension of the test line', which is on the far side of a longer 'induction' line and at an angle to it. But this is simply the conventional form of the Poggendorff illusion with a dot substituted for one of the transversals and the parallel, to which it would have been attached, removed.

One attempt different from these has, however, been made to test the theory. This was done by Lucas (1967) in experiments which were subsequently replicated by Fisher (1969) with larger numbers of subjects and wider range of angles but virtually identical results. Thirty-six upward-facing angles ranging in steps of 5° from 0° to 180° were judged and found to show the regression to right angles tendency. However, a second experiment, comparing sideways with upward- and downward-facing angles, not only found that the sideways-facing angles did not show this tendency but also obtained different results from the first experiment with regard to the upward-facing angles. The method used was to ask subjects to give verbal judgements of the size in degrees of each angle or, in a later experiment by Fisher (1969), to draw angles of different numbers of degrees in response to the experimenter's request. Even for trained subjects this does not seem likely to be an accurate method of finding out the angle they were actually seeing, still less for untrained subjects, as these apparently were.

Logically, the obvious method to discover apparent angle is to get subjects to match the standard with various comparison angles. This in fact is what was done in the present study and was also done in the study by Blakemore *et al.* (1970), referred to above. Since the latter study, though carried out independently, was done subsequently to ours, which was done in early 1969, it will not be referred to again until we come to the discussion.

The aim of the present investigation was quite limited and modest. It was simply to test the hypothesis that two lines at an angle to one another, which are component parts of certain illusions, are in fact judged as being at a greater angle to one another than they actually are. This was done for the Zöllner and the Poggendorff illusions. The angle taken was one conventionally used in drawings of these illusions, namely 30°. The two standard figures for the experimental groups that were used in all experiments are shown in Fig. 1.

EXPERIMENT I

This experiment was concerned with the angle at which the short transversal lines were seen. We have explained why we wished to measure the transversal in the Poggendorff illusion. As for the Zöllner, this illusion is always presented as manifesting a distortion of the longer lines, parallel to one another, which the transversals cross. But if the transversal in the Poggendorff illusion is distorted, so also should it be in the Zöllner. This would suggest that the Zöllner figure presents two illusions though it is designed to reveal only one of them. Further, if indeed *both* lines which form an angle with each other are distorted, then this would detract from the plausibility of

(*a*) Zöllner-type figure (*b*) Poggendorff-type figure

Fig. 1. Standard figures used in Expts. I, II and III.

the size-constancy theory of illusions in the case of this and other 'angle' illusions, as Hotopf (1966) has argued. For the sake of verisimilitude, in addition to the Zöllner component figure shown in Fig. 1 (*a*), a small complete Zöllner figure consisting of two parallel lines and three pairs of transversals was used as an additional standard.

In the experiment the transversals of the standard figures were matched with each one of five comparison figures, consisting of a single line of the same length as the transversal of the appropriate standard and either in the same orientation or in orientations plus or minus 1° or 2° from it. Control groups had as their standard only the transversal lines of Fig. 1 (*a*) and (*b*). These were matched against the same comparison figures as the experimental groups had. In other words, for the control groups the presumed influence of the vertical interacting lines was removed.

Method

Apparatus

The stimuli were presented to the subject on a series of white cards mounted on Duralamin plates, measuring 7·62 by 8·89 cm. They were displayed in a wooden stand which had two slots, enabling the two cards (standard and comparison) to be shown side by side.

Material

The lines were drawn in black ink on the cards. The length of the 90° (vertical) line was 5·08 cm; of the 60° (transversal), 2·54 cm in the case of the Zöllner (Fig. 1 (*a*)) and 1·27 cm in the case of the Poggendorff (Fig. 1 (*b*)). The lines, whether on standard or comparison cards, were always in the same position, and therefore level with one another when being compared. The third standard, the small Zöllner, consisted of vertical parallels with three pairs of transversals crossing them. The parallels and transversals were of the same length as the vertical and transversal in Fig. 1 (*a*). The distance apart of the parallels was 2·03 cm and the points of intersection of the transversals on each parallel were such as to divide the parallels into four equal parts.

Subjects

There were three groups of subjects. Group A was an experimental group of 16 subjects. These judged the standard Fig. 1 (*a*). Group B was an experimental group of eight subjects who judged Fig. 1 (*b*) and the small Zöllner in that order. Groups C and E were control groups of eight subjects each who judged the 2·54 cm and 1·27 cm transversals respectively. The subjects were university students of both sexes.

Procedure

The method of constant stimuli was used. There were four trials for each judgement, two with the comparison card on the right and two on the left, two with an ascending and two with a descending tendency. This is described as a 'tendency' because the order of presentation was

irregular. The descending tendency was 62°, 59°, 61°, 60°, 58° and the reverse for ascending tendency. The purpose of this was to reduce the chances of the subject guessing that the middle variable was the correct one. Exactly the same procedure was followed in the control condition.

The subject sat in a chair facing the display stand so that the centre of the display area was central to his line of vision. The distance, measured from the approximate position of the eyes to the stand, was 114·3 cm. Subjects in Groups A, B and C, in comparing the comparison with the standard, were asked to give one of two judgements, 'equal' or 'different'. The procedure for Group E is described in Expt. II; it permitted the response of this group to be treated in the same way as those of the other groups.

Results

The averages of the average angle that each subject judged equal to the standard for the two experimental and the two control groups are given in Table 1. The angles referred to are those formed by the transversal with the vertical. This makes it possible to indicate the amount of effect independently of the orientation of the component lines of the angle. As the last column of this table indicates, the proportion of judgements where the subjects thought that standard and comparison were 'equal', upon which the figures in the table are based, ranged between 36 and 51 per cent for the different groups. It will be seen from the table that the differences of experimental from control groups are in the predicted direction for each of the three pairs of groups and that these differences are significant, as judged by the Mann–Whitney U-test, one-tailed (Siegel, 1956).

Table 1. *Table showing the amount of distortion in the transversal lines of the Zöllner and Poggendorff test figures*

Illusion condition	Group	No. of subjects	Mean point of equality	S.D.	Significance test results: Mann–Whitney U-test (one-tailed)	Average percentage of judgements of equality
Zollner-type (Fig. 1(*a*))	A (experimental)	16	30·56°	0·44	$u = 19, P < 0·01$	42
	C (control	8	29·95°	0·30		36
Poggendorff-type (Fig. 1(*b*))	B (experimental)	8	30·56°	0·37	$u = 0, P < 0·001$	51
	E (control)	8	29·68°	0·45		28
Zöllner	B (experimental)	8	30·36°	0·34	$u = 12·5, P < 0·05$	47
	C (control)	8	29·95°	0·30		36

EXPERIMENT II

This and the next experiment are essentially repetitions of the first experiment, differing only in that *both* lines and not just the transversals were judged and that the small Zöllner figure was no longer used. The present experiment deals with the Poggendorff component figure.

Method

Material

These included the standard and comparison figures of the Poggendorff component figure in the first experiment. In addition there were comparison lines for the 90° (vertical) line which were of the same length as the standard and set at the same orientation or at this, plus or minus 1° or 2°.

Procedure

The general procedure was the same as before except that subjects were asked to make one of three judgements. These were 'steeper', 'flatter' or 'equal', in the case of the 60° standard, and 'leans to the left at top', 'leans to the right at top' or 'equal' in the case of the 90° line. The descending order of presentation for the 90° standard was 92°, 90°, 89°, 91°, 88°. This was reversed for the ascending order. Half the subjects judged the 60° standard before the 90° one, and half judged them in the reversed order.

Subjects

There were two groups of subjects, experimental (Group D) and control (Group E), with eight subjects, who were university students of either sex, in each group.

Results

Table 2 shows for the experimental and control groups the averages of the average angle that each subject judged equal to the standards in the case both of the transversal and of the vertical lines. The angles referred to are those formed by the two lines with one another. It will be seen from the table that the differences between experimental and control groups are in the predicted direction for both the transversal and the vertical line and that these differences are significant as judged by the Mann–Whitney U-test (one-tailed).

Table 2. *Table comparing amounts of distortion in the vertical and transversal lines of the Poggendorff figure*

Illusion condition	Group	No. of subjects	Mean point of equality based on 'equal' judgements	S.D.	Significance test results: Mann–Whitney U-test (one-tailed)	Average percentage of judgements of equality
Poggendorff-type 60°	D (experimental)	8	30·42°	0·66	$u = 10, P < 0·01$	32
	E (control)	8	29·68°	0·45		28
Poggendorff-type 90°	D (experimental)	8	30·38°	0·23	$u = 11, P < 0·05$	37
	E (control)	8	30·07°	0·20		23

Experiment III

This experiment does for Fig. 1(*a*) (Zöllner-type) what the previous one did for Fig. 1(*b*) (Poggendorff-type), namely determine whether not only the transversal but also the vertical is distorted in the predicted direction. But an additional hypothesis was also tested. It is well known that the Zöllner illusion in its conventional form with the transversal at an angle of 30° is increased if the parallels, instead of being in the vertical or horizontal position, are in an intermediate position between them. It was hypothesized that the vertical or horizontal positions are particularly stable ones, whether on grounds of learning to adjust to the forces of gravity or because of the perhaps consequential arrangement of neural structure (see, for example, Maffei & Campbell, 1970), and would therefore resist distortion more than lines in other positions. It was also hypothesized that the regression to right angles tendency is constant. Hence it was predicted that when the parallels of the Zöllner figure are seen at an angle of, say, 50° to the horizontal, then the transversals would be less distorted

and this to an amount sufficient to conserve the summed illusion of the two lines. In other words, the increased angle, seen as a result of displacement of both sides away from one another, would be the same whatever the orientation of the sides.

This additional hypothesis, as well as the main one, was tested by obtaining judgements from the subjects not only for the 90° and 60° lines of Fig 1 (*a*), but also for this figure rotated through 40°, i.e. with lines of 50° and 20° to the horizontal.

Method

Materials and procedure

Fig. 1 (*a*), rotated so that the long line was at an angle of 50° to the horizontal and the short one at 20°, was presented first and the unrotated form second. In each case the short line was judged first. The ascending and descending orders for the 90° and 60° lines were as already given. For the 50° and 20° lines they were respectively 48°, 51°, 49°, 50°, 52°, and 18°, 20°, 21°, 19°, 22°. The descending orders were the reverse of these. The control group followed exactly the same procedure as the experimental one.

Table 3. *Table showing amounts of distortion in both lines of Zöllner figure at different orientations*

Illusion condition	Group	No. of subjects	Mean point of equality based on 'equal' judgements	S.D.	Significance test results: Mann–Whitney *U*-test (one-tailed)	Average percentage of judgements of equality
Zöllner-type 20°	H (experimental)	8	30·77°	0·83	$u = 12, P < 0·05$	25
	J (control)	8	30·02°	0·48		35
Zöllner-type 50°	H (experimental)	8	30·63°	0·55	$u = 16$, n.s.	41
	J (control)	8	30·13°	0·16		43
Zöllner-type 60°	H (experimental)	8	30·60°	0·70	$u = 8, P < 0·01$	30
	J (control)	8	29·88°	0·30		42
Zöllner-type 90°	H (experimental)	8	30·15°	0·30	$u = 26$, n.s.	34
	J (control)	8	30·00°	0·26		25

Results

Table 3 shows for the experimental and control groups the averages of the average angle that each subject judged equal to each of the four standards. As before, these are the angles formed by the two lines with one another. All the differences are in the predicted direction and these are significant except in the case of the 90° and the 50° component lines of the standards. The latter of these two, however, borders on significance since $P = 0·052$.

As for the additional hypotheses, mentioned above, there was evidence from this and the previous experiment that the 90° line was particularly stable and therefore likely to be judged correctly. Only 6·25 per cent of the 320 judgements of the 90° line in the control groups of the two experiments were incorrect, and 20·6 per cent in the experimental groups. The proportions of the 640 responses given to the three other angles that were incorrect were 36·38 per cent for the control and 55·47 per cent for the experimental groups. These differences in proportion of errors between judgements of the 90° and those of whatever other angle yielded the lowest proportion of errors were highly significant both for experimental and control groups. This of

course fits in with what is known about the relationship between visual acuity and orientation of the stimulus (Taylor, 1963; Howard & Templeton, 1966).

It is also evident from Table 3 that the increase in angle for 50° was greater than that of 90°. Though this confirms the subjective impression given when the Zöllner illusion is presented in an oblique position, the difference between the two angles was not significant (Mann–Whitney $U = 19$, $P = 0{\cdot}097$, one-tailed). What is not confirmed is the deduction that the 20° transversal would be less distorted than the 60° one. The difference is in fact in the opposite direction. Unfortunately, the order in which the angles were presented was not varied, hence as far as Expt. III goes an order effect may have masked the effect we were looking for. However, the results of Expt. I with the Zöllner-type figure are relevant to this question too. Since in Expt. I the 60° transversal was the only one judged, which is tantamount to being judged first, the results are comparable with those of this experiment, where the 20° transversal was presented first. A comparison of Table 3 with Table 1 for the two angles in question shows that there is very little difference in degree of distortion for the two angles. Furthermore, in Expt. II, where the order of presentation was varied, there was no evidence that angles that were presented second tended to be less distorted than those that were presented first. The tendency was in fact in the opposite direction. Hence we may conclude from these experiments that there is no evidence that the total amount of distortion remains invariant when the figure as a whole is presented in an oblique position such that the distortion of the parallels is increased.

Discussion

These experiments show that there is in fact a tendency in the case of the 30° angle, conventionally employed in figures of the Poggendorff and Zöllner illusion, to be seen as larger than it is. They also show that this increase is probably contributed by both the lines forming the angle since each when separately measured is judged as displaced away from the other, though in the case of the Zöllner figure the displacement of the 90° line is not statistically significant.

The results of the experiment by Blakemore *et al.* (1970), already referred to, are in broad agreement with these results. They measured angles from 0° to 360° in steps of 15°, keeping one line of the standard fixed at 30° to the horizontal and changing the angle by varying the other. They measured the apparent displacement of the fixed line of the standard by adjusting a comparison line until it appeared to be parallel with the standard. Their method seems superior to that used in the present experiments and certainly showed less variability of judgement. However, they only measured one of the two component lines of the angle and were limited to two subjects who were authors of the experiment, so it is useful that there is independent confirmation of these results.

A further difference between the two sets of experiments is that Blakemore and his co-authors took as their standard a single angle rather than figures which contained two or four angles through one line crossing the other or both lines crossing one another as in Figs. 1 (*a*) and (*b*). The latter set-ups were of course used because they were the minimum requirements for the illusions in question. Blakemore *et al.*'s results are, however, of interest because of a particular criticism of the theory applied

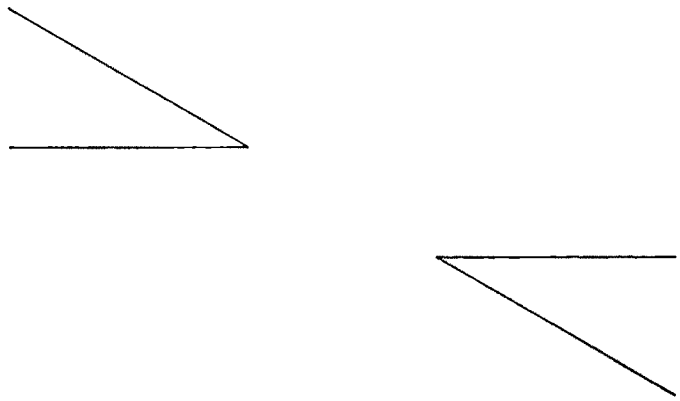

Fig. 2. The form of the Poggendorff illusion, which reverses the usual direction of effect.

to the Poggendorff illusion that comes from Brentano (1894). This is that when the Poggendorff illusion is drawn in such a way that the transversals form only acute angles with the parallels, as in Fig. 2, the illusion not merely disappears but, as Pierce (1901) pointed out, reverses in direction. Recent studies by Green & Hoyle (1964) and Restle (1969) have subjected this to measurement and confirmed it, though only the latter investigation gave significant results. But if we accept the results of Blakemore *et al.* to the effect that the kind of figure used in the Green & Hoyle and Restle experiments *does* show an increase in apparent angle size, then it is clear that this increase is not a sufficient condition for the occurrence of the Poggendorff illusion. This, as well as the fact that the amount of effect discovered in our experiments (about half a degree for the transversal on its own) appears rather small for the achieving of the illusion as normally portrayed, indicates the need for further experiments to test this theory. It is to this that our second series of experiments is devoted.

This research was supported by a research grant to the junior author (Christine Ollerearnshaw) from the Social Research Division of the London School of Economics. We would like to thank Mr A. E. M. Seaborne for helpful discussions and Dr Graham Kalton for valuable statistical advice and useful comments on the manuscript.

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(*Manuscript received* 18 *August* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 369–379
Printed in Great Britain

THE REGRESSION TO RIGHT ANGLES TENDENCY AND THE POGGENDORFF ILLUSION. II

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The regression to right angles explanation of the Poggendorff illusion was tested by varying the distance apart of the parallels. The varying amount of illusion this produced was shown to be consistent with a constant angular distortion. When, however, the apparent angle of the transversal was also measured, this was not found to be sufficient to account for as much as half the illusion with a 30° transversal. It was shown also that the extra amount of illusion could not be accounted for by the Müller-Lyer component of the Poggendorff illusion nor by any tendency to see the distance between the parallels as smaller than it was.

In a previous paper (Hotopf & Ollerearnshaw, 1972) it was shown that the transversal in a Poggendorff-type figure does in fact manifest the regression to right angles tendency, at least for the angle of 30° conventionally employed for that figure. The degree of angular displacement, as assessed by the average comparison angle with which the transversal of the standard was equated, was, however, rather small, no more in fact than about half a degree, which would seem too small to account for the total amount of the illusion. This small amount may, however, have been due to the fact that only part of the Poggendorff figure was presented, namely one transversal and one line at an angle to it. This latter line was furthermore relatively short and there was some evidence from the earlier investigation suggesting that when two lines of unequal length cross one another at an angle, then the shorter shows greater displacement than the longer. From this it could be inferred that the greater the difference between two lines at an angle to one another, the more the shorter one would be displaced. Furthermore, the proximity of the comparison transversal to the vertical edge of the plate on which it was drawn in this experiment may also have introduced some displacement in the comparison object itself. This would mean that the total amount of displacement suffered by the standard would be underestimated. In view of these considerations, it was decided to compare the amount of angular displacement of the transversal in a full Poggendorff-type figure with the amount of illusion that the subject underwent.

For this purpose apparatus was constructed in which a Poggendorff figure was displayed, such that the position of one of its transversals (that on the bottom left) could be varied in a vertical direction until it seemed to the subject to be in alignment with the other fixed (top right) transversal. This measured the amount of the illusion. To the right of the Poggendorff figure was an arm, identical in dimensions to the top right transversal and at the same level as it, whose orientation could be altered until it seemed to the subject to be in the same orientation as the transversal on the Poggendorff figure. This measured the regression to right angle effect. Fig. 1 presents this diagrammatically, with arrows indicating the directions in which the two variables could be moved.

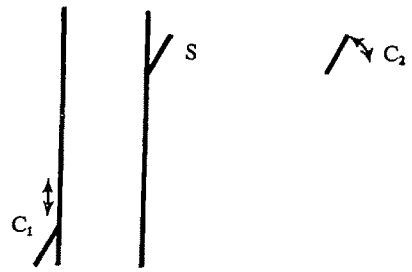

Fig. 1. The display used in measuring the Poggendorff illusion and the apparent angle of the standard transversals. C_1 and C_2 are the comparison stimuli whose direction of movement is indicated by the arrows. S is the fixed standard stimulus.

The apparatus was twice altered in order to secure improvements in the design, but the experiments with the earlier forms yielded some data of interest and these will be reported in turn.

EXPERIMENT I

The apparatus for this experiment had an additional feature in that the distance apart of the two parallels could be varied. Clearly if the illusion were due to angular displacement of the transversals, then the amount of the illusion should increase as the distance between the parallels increases. Unfortunately the variable comparison arm (C_2 in Fig. 1) in this apparatus rotated about its centre. Consequently the left-hand end of this arm changed its position in the vertical coordinate relative to the vertex of the fixed standard transversal (S in Fig. 1). This gave subjects an additional clue in their matching task and no angular displacement towards a right angle was observed. Consequently we shall limit ourselves in this experiment to describing the relationship between amount of illusion and distance apart of the parallels. Only that part of the apparatus relevant to this will be described.

Method

Apparatus

The Poggendorff figure, as shown in Fig. 1 with the transversals at an angle of 45° to the verticals, was presented to the subject upon a flat white display board which measured 54·29 cm by 79·06 cm. The figure was made of slats of wood 1·27 cm ($\frac{1}{2}$ in.) square in cross-section and painted black. The verticals were 62·87 cm long, and the two transversals (S and C_1 in Fig. 1) were 13·65 cm long, joining the verticals at positions 17·46 cm from the top for the right-hand vertical and from the bottom for the left-hand one.

Two features of the apparatus could be varied. The first was the left transversal, which could be moved up and down the left-hand vertical along a slot in the display board. By means of this slot, which was concealed from the subject's view by the vertical, the experimenter could vary the position of the transversal from 10·80 cm from the base of the vertical to 17·15 cm from the top, giving a range of vertical movement of 33·66 cm in all.

The second variable feature was the amount of separation between the two verticals. The right vertical with its attached transversal could be fixed in any one of six positions, giving separations of 5·08, 10·16, 15·24, 20·32, 25·40 or 30·48 cm between the inner edges of the lines.

Procedure

Subjects viewed the stimulus from a distance of 3·35 m in uniform neon lighting. The experimenter was concealed behind the apparatus and moved the variable transversal according to

instructions issued by the subject. Two groups of subjects were used. Group 1 judged both the amount of illusion and the angle of the right-hand transversal. For the reasons given above, the results of the latter are not reported. For judging the amount of illusion they were given six trials, three with the left-hand transversal ascending and three with it descending. The distance between the verticals was held constant at 25·40 cm. Group 2 only judged the amount of the illusion. They did this with two trials, one ascending and one descending, for each of the three distances apart of the verticals, 10·16, 20·32 and 30·48 cm.

Subjects

Group 1 contained four male and two female undergraduate and research students. Group 2 contained fifteen male and one female undergraduate.

Results

The amounts of illusion yielded by the different widths of illusion figure are shown in Table 1 below, together with standard deviations and the angles implied by these amounts. Fig. 2 presents these results graphically.

As can be seen from Table 1, there is a very slight increase in the angle implied by

Table 1. *Relation between distance apart of the verticals and amount of illusion*

Subject group	Width of illusion figure (in.)	Amount of illusion (cm)	S.D. (cm)	Implied angle
1 (n = 6)	10	5·28	4·68	50·97°
2 (n = 16)	4	2·31	0·85	50·72°
	8	4·17	1·94	50·73°
	12	6·28	2·11	51·00°

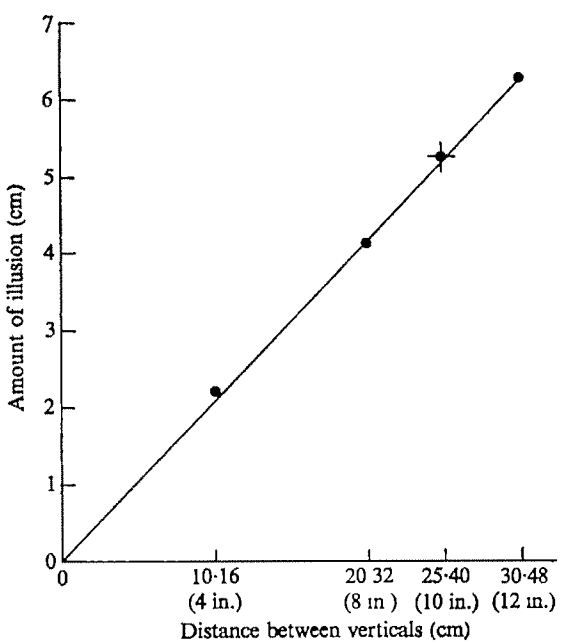

Fig. 2. Relation between amount of illusion and width of Poggendorff figure. ✦ = Group 1, ● = Group 2.

the illusion with increasing distance between the verticals. This is from 5·72° to 6°. Analysis of variance, however, showed that there was no significant difference between subjects' average implied angle scores at widths of 10·16, 20·32 and 30·48 cm ($F = 0{\cdot}06$; d.f. = 2, 45; n.s.); and a Mann–Whitney U-test produced no significant difference in average scores between the two groups ($U = 35$; $P > 0{\cdot}1$, two-tailed). The results are therefore consistent with the hypothesis.

EXPERIMENT II

The apparatus used in this experiment was an improvement on that used in the first experiment in that the variable comparison arm (C_2 in Fig. 1) rotated about its end which was at the same level as the vertex of the fixed standard transversal (S in Fig. 1). The ability to vary the distance apart of the two parallels was, however, given up, and a display, in which the two parallels and the fixed transversal were mounted in black tape on a sheet of perspex through which two variable stimuli could be seen, was used instead. This experiment was concerned with the relation between the amount of illusion and the amount of angular displacement of the transversals.

Method

Apparatus

This consisted of two matt white rectangular sheets of metal, which were of the same height, 55·88 cm, but of differing widths 40·32 and 25·40 cm. Mounted on wooden stands and placed side by side so as to face the subjects, they carried the two variable arms (C_1 and C_2 in Fig. 1). The wider one, which was on the subject's left, carried the arm, C_1; the narrower one, C_2. Sheets of perspex of the same size as the background were placed 0·64 cm in front of them. The narrower one was blank, but on the wider one the Poggendorff figure, minus its left-hand transversal, was placed in black 'Chartpak' tape. The verticals were 36·83 cm long and 0·64 cm wide and 10·80 cm apart from one another. The right-hand transversal was 6·35 cm long and 0·64 cm wide. It formed an angle of 30° with the right-hand vertical and joined it at a point 8·89 cm from the top. The left-hand transversal (C_1) was a metal arm faced with the same 0·64 cm tape and of the same dimensions as the other transversal. The arm was fixed to a runner which moved along a vertical slot cut through the background. When viewed from only a short distance away, this metal arm on the background appeared to be part of the rest of the Poggendorff figure on the perspex, the slot it moved along being entirely hidden by the left vertical of the figure. The variable arm, C_2, was of the same dimensions as the transversals of the Poggendorff figure. One end was mounted on a spindle so that it rotated around a point, which was at the same vertical height as the point where the right transversal of the Poggendorff figure joined its vertical.

Procedure

Subjects viewed the stimulus at a distance of 2·29 m with the chin-rest placed so that the mid-point between their eyes was level with and directly in front of a point equidistant in the horizontal plane between the two stimuli being judged. This position had to be changed by 19·69 cm to the right or left, depending upon which judgement task was being undertaken.

Design

For measuring both illusion effect and angular distortion four readings were taken from each subject – two trials starting from the top of the vertical slot and two from the bottom, in the case of illusion effect; and two from 20° and two from 40° clockwise from the vertical, in the case of angular distortion. Order of presentation of the two measurement tasks, and of ascending and descending series within them, were systematically varied as between subjects.

Subjects

There were ten subjects – six male and four female students.

Results

The average angle at which the 30° standard was perceived was 32·5°, which is significantly different from 30° and in the expected direction ($t = 3{\cdot}35$; d.f. = 9; $P < 0{\cdot}005$, one-tailed). The average amount of illusion was 4·43 cm above the correct position. This also was in the expected direction and significant ($t = 10{\cdot}7$; d.f. = 9; $P < 0{\cdot}0005$, one-tailed). The angle of the standard that this implies, 36·2°, which is close to what was observed in the previous experiment, is, however, significantly larger than the one actually measured of 32·5° ($t = 3{\cdot}76$; d.f. = 9; $P < 0{\cdot}01$, two-tailed). It is evident from this experiment, therefore, that the regression to right angle effect is insufficient to account for the whole of the Poggendorff illusion.

Experiment III

Another theory of the Poggendorff illusion is that there is a Müller-Lyer type component in it (Tausch, 1954). Although, as Hotopf (1966) has shown, this cannot account for the whole of the illusion, it may be a contributory factor in it. Müller-Lyer pointed out that the sides of obtuse angles appeared longer and of acute angles shorter than lines without angles attached to them. The illusion named after him he attributed to a multiplication of this effect (Boring, 1942). Applied to the usual form of the Poggendorff illusion this would mean that the upper transversal would be displaced upwards and the lower downwards. Consequently some part at least of the Poggendorff illusion should follow.

To test this, two additional forms of the lower, left-hand transversal (C_1 in Fig. 1) that was used in Expt. II were made. In one this transversal was placed at an angle of 30° *counterclockwise* from the vertical. This produced an upward-facing angle which should, according to Müller-Lyer's reasoning, lead to an upward displacement of the arm which would cancel the effect of the downward displacement of the other arm. In the third version the left-hand transversal was replaced by a small rectangular stud which, being at right angles to the vertical, should show no Müller-Lyer type displacement. The three different conditions are illustrated in Fig. 3. The predictions are that with Condition B the amount of the illusion would be exactly predicted by the amount of distortion of the angle formed with the vertical by the upper right-hand transversal, as measured by the rotating arm to the right of the figure. Under Condition D there should be more illusion than could be predicted by the angular distortion, and under Condition C there should be half the excess of D over B, since not both but only one of the transversals is, according to the hypothesis, giving the additional Müller-Lyer effect.

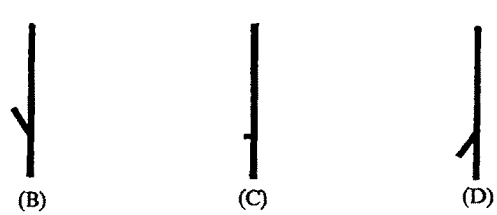

Fig. 3. The three conditions of presentation of the comparison stimulus on the left-hand vertical used in Expt. III.

Method

Apparatus

The apparatus and its display were the same as in the previous experiment, except that the variable comparison arm for judging the angle of the right-hand transversal was not on a separate metal sheet from the one on which the Poggendorff figure was mounted. Instead the whole display was on a single metal sheet, 68·58 cm high by 66·04 cm wide, which was fixed on a wooden base. This eliminated the vertical line, formed by the edges of the sheets on which the Poggendorff display and the variable rotating arm were mounted, which, coming between them, may have exercised a distorting effect of its own. Further, the left-hand transversal was detachable from the runner so that it could be replaced by one of the same dimensions, but at an angle of 30° counter-clockwise from the vertical, or by a stud, the visible part of which measured 0·64 cm square ($\frac{1}{4} \times \frac{1}{4}$ in.).

Procedure

As in the previous experiment, the illusion, whether presented under Conditions B, C or D, and the apparent angle of the right-hand transversal (Condition A), were measured four times in succession – twice with ascending and twice with descending order of presentation. Each subject made judgements under the three different illusion conditions as well as judging the apparent angle. The 24 different possible orders of combination were each allocated to one subject. Half the subjects (Group 1) then repeated the earlier experiment. The other half (Group 2) repeated the experiment with two rather than four measurements for the four different judgement tasks. The change with Group 2 was carried out because of evidence of strain with the subjects in Group 1 as well as difficulty in completing the task in the time that had been allotted to the experiment.

Subjects

There were 24 subjects of both sexes, all of whom were students.

Table 2. *Comparing amounts of illusion under Condition A with those implied by the regression to right angle hypothesis under Conditions B, C and D*

Condition	Average amount of illusion (cm)	S.D.	Average angle	Comparison of implied angles with amount of effect in A
A	—	1·57	+1·45°	
B	4·11	1·70	+5·70° (implied)	$t = 9·41$*
C	3·17	1·63	+4·23° (implied)	$t = 6·02$*
D	3·45	1·60	+4·67° (implied)	$t = 7·21$*

* $P < 0·001$. t-test, two-tailed.

Results

The amount of regression to a right angle of the upper right-hand transversal (Condition A), as well as the amount of Poggendorff illusion under Conditions B, C and D, are given in Table 2. The table combines the results for Groups 1 and 2 because they were in the same direction and did not differ significantly from one another. Two things are immediately apparent. First, the amount of regression to right angle is insufficient to account for the amount of illusion under any condition. The differences between the measured angle of the upper right-hand transversal (Condition A) on the one hand and the angles implied by the vertical displacement of the left-hand trans-

versal (Conditions C, B and D) on the other, are all, as Table 2 shows, highly significant. Secondly, the amount of additional illusion is consistent with the Müller-Lyer summation hypothesis only in the case of Conditions C and D, but not that of B. Condition B instead of giving the least gives the greatest amount of illusion. It differs significantly from Condition C ($P < 0{\cdot}01$, analysis of variance, Duncan's 1955 multiple range test (Edwards, 1968)), though none of the other differences between these three conditions is significant. Furthermore, if *ex hypothesi* we take the difference between Conditions C and D as giving half the total Müller-Lyer effect, then it is clear that this, which is 0·42°, is nothing like enough to account for the difference (2·8°) between the amount of illusion predicted by the regression effect and the amount actually obtained in Condition C. Therefore, even if we assume that the Müller-Lyer illusion is having some effect and attribute the failure of Condition B to conform to this to some complicating additional factor, the combination of this and the regression to right angles effect still falls far short of accounting for the whole of the illusion.

But it may be asked whether we can be sure that the regression to right angles tendency is in fact contributing anything to the illusion. It certainly *should*, if the right-hand transversal is in fact seen as being at a greater angle than it actually is, but this might be swamped by some other and more powerful factor working in the same direction. That this is probably not so is, however, suggested by the fact that there was a positive correlation (product moment) of 0·38 of subjects' distortion scores in Condition A with their averaged scores in Conditions B + C + D, which is significant at the 0·005 level.

Experiment IV

A second hypothesis to account for the discrepancy between the amount of regression to right angle and amount of illusion in the Poggendorff figure is one using the inappropriate constancy scaling theory of illusions. According to this theory it should follow that when a part of a diagram is seen as figure it should look smaller than when it appears as ground. To reject this deduction on the grounds that this would be a case of secondary and not primary scaling would be impossible in view of the evidence (Hotopf, 1966; Lucas, 1967) that the Necker cube, which is a similar phenomenon, does show changes in apparent size as the perspective reverses. Now, given that the transversals in the Poggendorff figure are seen as being at a greater angle to the rectangle formed by the parallels than they actually are, the illusion thus set up would be increased if the distance between the parallels were perceived as less than it actually is. This of course rests on the assumption that the distance apart of the transversals themselves is not also seen as decreased. We can make this assumption because the transversals are seen as more distant than the rectangle formed by the parallels behind which they pass. This theory was tested in the present experiment by getting subjects to judge the width of rectangles, which were of the same size as the figures enclosed between parallels in the previous experiment and viewed under exactly the same conditions.

Method

Material

The standard figure was a black outline rectangle 12·07 × 36·83 cm, the lines being 0·64 cm (¼ in.) thick. This figure was made of black Chartpak tape (as in Expts. II and III) on a white card measuring 40·32 × 55·56 cm. The comparison stimuli consisted of seven horizontal black lines, 0·64 cm thick, ranging in length from 10·16 to 13·97 cm in 0·64 cm steps. These were tapes placed on cards of the same dimensions as the standard card. The standard was presented with its long side in the vertical dimension and the comparison line was always in a position alongside half-way up the rectangle.

Procedure

Each of the comparison lines was presented alongside the standard twice on the left and twice on the right. The order in which the comparison lines were presented was determined by a table of random numbers. The subjects, who sat at a distance of 2·29 m from the cards, were asked to judge whether the comparison lines were wider, narrower or equal to the width of the rectangle from outside edge to outside edge.

Subjects

The subjects were 24 male and female students.

Results

The results did not bear out the hypothesis. The P.S.E., as calculated by simple interpolation on the stimulus-probability plot, came to 12·37 cm; in other words, the rectangle was judged as larger than it actually was rather than smaller.

It can also be demonstrated that the illusion does not depend upon the parallel lines being seen as outlining a figure behind which the transversal line runs. In Fig. 4, where the transversals are drawn inside two figures, and are separated by ground, it can be seen that the illusion still operates. It would seem from this that the Poggendorff illusion is not an instance of inappropriate constancy scaling in Gregory's sense.

Fig. 4. The Poggendorff illusion is still present when the transversals are separated by ground and not figure.

DISCUSSION

The aim of this series of experiments was to test the hypothesis favoured by earlier experiments (Hotopf & Ollerearnshaw, 1972) that the Poggendorff illusion was determined by a tendency to see acute angles as increased in size. Studies published subsequently to the present investigation (Bouma & Andriessen, 1970; Weintraub & Krantz, 1971) have been against this hypothesis. Since our experiments were carried out independently of them, we will at present consider only the evidence of our own experiments, and such of theirs as bears directly on points arising out of our own.

The first experiment which showed an increase in illusion with increasing distance between the parallels was, as we saw, entirely consistent with the hypothesis. Other studies (Burmester, 1896; Weintraub & Krantz, 1971), which varied the angles of the transversals as well as the distances apart of the parallels, have produced similar results. Burmester in his exemplary study, using five different angles ranging from 20° to 60° and five different distances apart of the parallels, expressed the amount of illusion in terms of a constant multiplied by the product of the distance apart of the parallels and the cotangent of the angles of the transversals. This constant was 0·157. Burmester had only a single subject, but Weintraub & Krantz with 850 subjects, four angles ranging from 16·7° to 67·4° and four distances apart, obtained one of 0·162, this being determined by a least-squares best-fit straight line with the function constrained to pass through the origin. In Expt. I of our study the constant was 0·185. This was not determined by a least-squares fit, but a glance at Fig. 2, where a straight line was drawn by eye, shows that there clearly is a close fit. Weintraub & Krantz's fit was the least good of these three studies, not surprisingly considering the large and heterogeneous nature of their sample and the fact that each subject gave only one judgement.

Now if the illusion were determined by more than one factor and if the regression to right angles effect, as Expts. II and III suggest, only contributed a half or a third of the total effect, one could only have got the results we have just described if the different factors varied concomitantly with one another, according to angle and width apart of the verticals. This seems unlikely, though it is logically possible. Could it be that the method we have used of judging the apparent angle of the standard transversal seriously underestimated the regression to right angles effect? If we compare the earlier series of experiments (Hotopf & Ollerearnshaw, 1972), which yielded an average effect of about half a degree, with Expts. II and III in the present series, which yielded 2½° and 1¾° respectively, we see that the method of measurement does affect the amount obtained. We assumed that the increase in the effect was due to the greater length of the vertical relative to the transversal in the present compared with the previous experiments, but we have no hard evidence that this is so. How do the measurements obtained in our own experiments compare with those of others?

We have seen by comparing Expt. I with Burmester's and Weintraub & Krantz's results that the amount of Poggendorff illusion we obtained is comparable with theirs. Our Expts. II and III with a 30° transversal yielded a constant of 0·179, which is also close to theirs. Novak (1966), using a constant width of the Poggendorff figure and with transversal angles of 67·5°, 45° and 22·5°, obtained results which entail constants of 0·169, 0·180 and 0·097 respectively. Here only his results with the small angles depart much from other results, including his own with the larger angles. It would seem then that our results with the Poggendorff illusion are representative.

What is the position with the regression to right angles effect? As we pointed out in our earlier article, no direct measurements have to our knowledge been carried out on this. In that study, however, we showed that the basic Zöllner figure gave closely similar results to the basic Poggendorff figure. Wallace, in a number of studies of the Zöllner illusion (Wallace, 1964, 1966, 1969; Wallace & Crampin, 1969), has sometimes obtained distortions over 3°, but these were obtained with the optimum angles of intersection, which were only 10° or 15°. For larger angles they ranged between 2½°

and $\frac{1}{2}°$. To give the reader some idea of the size of the Zöllner illusion expected if the distortion were of the amount obtained with the Poggendorff illusion, Fig. 5 has been drawn. Fig. 5(*b*) shows a Zöllner illusion yielded by transversals at an angle of 30° to the parallels. Fig. 5(*a*) shows what the illusion of the parallel lines should look like if the distortion was one of 2° and Fig. 5(*c*) illustrates how it would appear if the amount of distortion, obtained in our measurement of the Poggendorff illusion, were the amount obtained with the Zöllner illusion. The figure shows that, if the Poggendorff illusion is due to the same mechanisms as those responsible for the Zöllner and similar 'angle' illusions, then for some reason the effect is very much stronger.

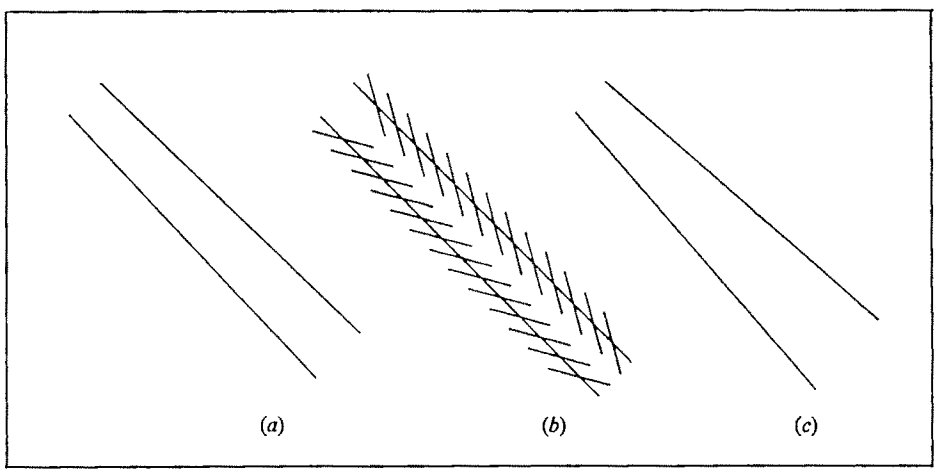

Fig. 5. A comparison between (*b*) the Zollner illusion and (*a*) lines that diverge from one another by 2°, and (*c*) lines that diverge by 4°. The latter is the amount of Zöllner illusion that would be expected if the size of the illusion were the same as the Poggendorff illusion.

Further experiments by the authors, in which the right-hand line of the Poggendorff figure was no longer vertical but presented at an angle of 45° to the horizontal and the standard transversal presented at angles of 80° and 100° to it, suggested that an additional factor in the Poggendorff illusion, which might account for the discrepancy we have noted, was a tendency to err when making alignment judgements in the direction of the horizontal or vertical spatial coordinate, whichever was nearer. To measure this properly, however, a new apparatus had to be constructed. The results of these experiments and their relation to the work of Bouma & Andriessen (1970) and Weintraub & Krantz (1971) will be presented in a separate article.

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(*Manuscript received* 18 *August* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 381–386
Printed in Great Britain

THE REORDERING OF THREE-TERM SERIES PROBLEMS BY BLIND AND SIGHTED CHILDREN

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The experiment compared strategies used by blind and sighted children in attempts to solve two forms of the three-term series problem. Order of magnitude and temporal order were congruent in one, while in the other they were not. Results showed that both groups of children used a sequential rather than a logical order as a guide in answering questions, and that there was no indication that sighted children used a strategy of spatial coding. Strategies used were illustrated by analysing incorrect responses.

In three-term series problems one solves, or fails to solve, quantitative comparisons involving three terms and two relationships. One example is A > B:B > C. The terms are A, B and C, while the two comparisons to be made are A with B and B with C. From this it follows that A > C. Another example, using the relationship of 'greater than', is B > C:A > B. Both types of statement can be posed by using the same terms but with the relationship of 'less than'. Further complexity can be given by using 'greater than' and 'less than' in the same proposition. An example is A > B:C < B. The questions which can be asked for comparative statements of this kind are: which is the 'greatest', 'least' and which is the 'middle' term?

Both Hunter (1957) and Huttenlocher (1968) have presented reviews of the solutions offered to such problems. Hunter notes that three restrictions are usually involved. Presentation is verbal; there are only three terms and not more; and only declarative statements, not negatives, are used. Hunter also draws attention to previous studies by Piaget (1921, 1928) and Burt (1919). Piaget presented non-verbal forms of the problem, and noted that it can be solved by children at the age of 7 or 8, whereas most of the experiments by Huttenlocher (1968) or Hunter (1957) have been carried out with older subjects. The central question is: by what psychological processes do people attempt to derive solutions to the problem on the basis of the premises presented?

One basic conclusion of the more recent studies is that the subject organizes verbally presented items along an imaginary scale. Various terms have been used to describe this strategy. For example, Hunter (1957) quotes James (1891) to make the point that the two premises of a comparison must be isotropic and that if they are not, the subject must reorganize them to make them so. Reorganization in Hunter's description requires one of two procedures. The first, reordering, is the changing of the order of the two premises, e.g. B > C:A > B will be reordered to give A > B:B > C. The second, converting, means changing a form such as A > B:C < B into A > B: B > C. In what follows we are largely concerned with reordering, while Huttenlocher is concerned with converting. Huttenlocher (1968) concludes, after a review of several studies, that items in any problem are ordered along a spatial dimension. She states: '*S* imagines items to be material objects which he must move about in space to

correspond to the verbal descriptions given in the two premises. The mental operations involved in obtaining answers to these reasoning problems, it is proposed, correspond to those involved in making actual spatial arrangements of real objects according to analogous types of verbal descriptions.' If, in fact, such a strategy is commonly employed, it would be of interest to determine what strategies would be used by those for whom spatial imagery might be difficult, if not impossible, i.e. for those blind from birth.

It has been shown (O'Connor & Hermelin, 1972) that blind as well as sighted children tend to organize auditory input temporally rather than spatially. Propositions such as B > C:A > B might require not only reordering along an imaginary spatially arranged scale of magnitude; there is also incongruity between order of magnitude and temporal sequential order. Thus in the proposition B > C:A > B, item A would be the tallest, prettiest, etc., whereas B is the sequentially first-occurring item. Thus this type of proposition would need reorganization regardless of whether strategies were spatially or temporal-sequentially mediated, and would be more difficult than if stated in the form A > B:B > C. If subjects give responses on the basis of temporal order, this should be reflected in the error patterns which might be expected to mirror this temporal order. The experiment described below explores the performance of blind and normal children in attempting to solve three-term series problems.

Method

Two forms of the three-term series proposition were presented. The first had sentences of the type A > B:B > C. The other was of the form B > C:A > B. An example is the proposition 'The English book is thicker than the French book, and the German book is thicker than the English book'. Thus in the first kind of sentence the logical and sequential order coincide, while in the second they do not. Nine sentences of each kind were presented in random order to each child, and the sentences used are given in Table 1. Sometimes, according to a predetermined order, the question asked first was 'Which is the middle one?' and sometimes the question about greatest magnitude, i.e. 'Which is the tallest, fastest, etc?' was asked first. Each child was thus asked two questions, the second being asked after the first one had been answered.

If the sentence was, for instance, 'The horse is older than the dog, and the dog is older than the cat', the questions were: 'Which is the oldest?' and 'Which is the middle one?' 'Which is the least old?' was not asked because of the likelihood that children would answer with the one remaining term which they had not used in answering the first two questions.

Subjects were always given the following example at the beginning: 'The pine tree is taller than the oak tree, and the oak tree is taller than the fig tree'. They were then asked: 'Which is the tallest tree?' and 'Which is the middle one?' They were not corrected if wrong, but were always encouraged.

Subjects

Ten sighted and 10 blind subjects were selected for the experiment. Sighted children were chosen by an empirical procedure to determine whether they could answer enough of the questions without being so advanced that they could correctly answer all. It was found that children aged between 7 and 9 years were most suitable. Criteria used were that a subject should be able to answer correctly more than one third of the 18 'easy' questions of the form A > B: B > C but not more than 32 of all the 36 possible questions.

Blind children were selected according to the same criteria. The mean age of the sighted children was 8·83 years (s.d. 0·34) and the mean age of the blind 8·09 years (s.d. 0·61).

Results

Results were analysed in two ways: first, on the basis of correct scores and second, on the basis of errors. The answers given by both groups to the two kinds of questions in both types of series, A > B:B > C and B > C:A > B, are given in Table 2. It can be seen that in the first part of the table A is given as the correct answer by both groups to the 'greatest' question in most cases and B as the correct answer to the 'middle' question. In the second part of the table, where correct answer and sequential position do not correspond, the first-named item (B) is given in many cases to the 'greatest' question and the second-named (C) to the 'middle' question. These latter are, of course, incorrect answers.

The correct answers were compared between groups and positions (i.e. A, B and C) and between the two types of three-term series propositions. The details of the analysis are given in Table 3.

Answers to 'easy' questions, i.e. where logic and sequence were congruent, were

Table 1. *Propositions used in three-term series experiment*

(Numbers indicate order of presentation.)

A > B:B > C ('Easy' problems)

1. The horse is older than the dog and the dog is older than the cat.
3. Sheila was prettier than Janet, and Janet was prettier than Jean.
5. The rabbit ran faster than the hare, and the hare ran faster than the hound.
6. Sarah came earlier than Richard, and Richard came earlier than Tom.
8. Daz washed whiter than Persil, and Persil washed whiter than Omo.
11. The Russian book is thicker than the Spanish book, and the Spanish book is thicker than the Greek book.
13. Ruth is taller than Dick, and Dick is taller than Ann.
14. Andrew came earlier than Ian, and Ian came earlier than Brian.
17. The yellow string is longer than the brown string, and the brown string is longer than the pink string.

B > C:A > B ('Hard' problems)

2. The English book is thicker than the French book, and the German book is thicker than the English book.
4. The seagull called louder than the crow and the parrot called louder than the seagull.
7. Joan was prettier than Alice and Margaret was prettier than Joan.
9. The red car is faster than the blue car, and the green car is faster than the red car.
10. The church candle burnt brighter than the Christmas candle, and the birthday candle burnt brighter than the church candle.
12. The red string is longer than the blue string, and the green string is longer than the red string.
15. The house is older than the church, and the castle is older than the house.
16. The piano played louder than the trumpet, and the drum played louder than the piano.
18. John is taller than Mary, and Peter is taller than John.

Table 2. *Frequency of answers given by sighted and blind children to three-term series problems*

	A > B:B > C						B > C:A > B					
	'Greatest'			'Middle'			'Greatest'			'Middle'		
	A	B	C	A	B	C	B	C	A	B	C	A
Sighted	52	15	23	3	75	12	44	13	33	25	41	24
Blind	77	11	2	4	75	11	36	10	44	24	55	11

significantly more often correct than answers to 'hard' questions, i.e. incongruent ones. Significant interactions were analysed by the t test and showed that the blind answered the 'greatest' questions correctly more often than the sighted (Table 2), although answers to the 'middle' question did not differ. They also showed that whereas 'greatest' and 'middle' questions were equally often correct in the congruent series, the 'greatest' question was answered correctly more often in the incongruent series.

These results are of interest in so far as they confirm Hunter (1957) in suggesting that questions of the A > B:B > C type are easier than questions of the B > C:A > B type. In the main, however, interest for this paper lies in the analysis of the incorrect answers given to the incongruent series. These are presented in the second part of Table 2.

In Table 2 the frequency of answers is shown according to the order of the occurrence of terms in the two premises. For example, in the proposition, 'The horse is older than the dog and the dog is older than the cat', the order in which the names occur is horse, dog, dog, cat. In the proposition, 'The English book is thicker than the French book and the German book is thicker than the English book', the order of terms is English, French, German and finally English. It is clear that in the first type of proposition the 'oldest' is also the first occurring name, and the 'middle one' is the 'middle' name in sequential order. In other words, the order is congruent. In the 'hard' proposition (i.e. B > C:A > B), however, order of magnitude and sequential order are not congruent, the 'thickest' book is not the English book, which is mentioned first and the 'middle one' is neither the second-named book nor the third-named, but the first-named. We analysed only the incorrect responses to these incongruent or 'hard' propositions.

Analysis based on the incorrect responses set out in Table 2 enabled an answer to be obtained to the question concerning the strategy the children adopted when they failed to solve the problem correctly. The correct solution to the 'greatest' type question in the 'hard' problem was always the third-named person, animal or thing, so that the frequencies of the incorrectly given first two terms could be compared. The hypothesis was that the first-mentioned name would be selected most often. In the 'middle' type question in the 'hard' problem, the first term was the correct answer and the second- and third-named items were incorrect. Our hypothesis was that in this instance the second or sequentially middle term would be given most often.

Reference to Table 2 shows that incorrect responses to the questions concerning the

Table 3. *Analysis of variance table based on correct scores*

Source	D.F.	V.E.	V.R.	Significance
Groups	1	15·31	2·29	n.s.
Positions	1	0·61	< 1·0	n.s.
Conditions	1	292·61	70·71	0·001
Groups × positions	1	17·11	4·14	0·05
Groups × conditions	1	2·81	< 1·0	n.s.
Position × conditions	1	30·01	7·25	0·01
Groups × positions × conditions	1	2·13	< 1·0	n.s.
Between people	18	6·70	—	—
Residual	54	4·14	—	—

greatest magnitude and to the question 'Which is the middle one?' were given by following a sequential strategy. The first-mentioned term was given most frequently as an answer to the question 'Which is the greatest?' and the sequentially middle term to the question 'Which is the middle one?'. The use of this strategy was verified by the median test (Siegel, 1956) comparing the two possible incorrect answers to each kind of question. The χ^2s derived from these calculations for the sighted children in answer to the 'greatest' and 'middle' questions were 14·40 ($P < 0{\cdot}001$) and 3·60 (n.s.) respectively. The comparable results for the blind children were 8·22 ($P < 0{\cdot}01$) and 18·00 ($P < 0{\cdot}001$).

The second question which can be answered from Table 2 was whether or not the two groups used a temporal sequential strategy to the same extent. Distribution of scores from the blind and sighted were compared, and χ^2s showed that error scores in the responses to the question of greatest magnitude were similarly distributed. In the case of the 'middle' question ($\chi^2 = 6{\cdot}86$; $P < 0{\cdot}01$) the blind adopted the strategy of incorrectly responding with the sequential middle term more often than did the sighted children, who gave a larger number of non-sequential random responses.

Discussion

This experiment set out to test what strategies would be used by blind and sighted children to solve three-term series problems. Previous investigators, particularly Huttenlocher, have concluded that sighted subjects 'understand the meaning of the sequences of premises when they have created from them a spatial array of items along a directionally marked axis'. Our experiment did not allow conclusions about the strategies followed by those subjects who were able to solve the problem correctly. However, the fact that correct solutions were obtained with equal frequency by blind and sighted children indicates at least the possibility of alternative approaches.

Incorrect responses seem to have been based on strategies using a temporal-sequential rather than a spatial-sequential dimension. This tendency is more marked in the blind, but it is also clearly present in the sighted. Such an approach would automatically result in correct solutions of the three-term series problem when it is stated in such a form that temporal and logical orders coincide, but would be unsuccessful when they do not. Although both groups solved the first type of problem significantly more often than the second kind, it is possible that both problems were answered without awareness of the logical questions involved, though this would only be apparent with the non-congruent type of proposition. Huttenlocher concludes that verbally presented three-term series problems are solved in a similar way to that used with real objects, presumably by the manipulation of items in imagined space. Our results could be accounted for on the basis of a temporal-sequential ordering strategy. If sighted people used spatial coding, then there ought to be a difference between sighted and blind children, since the latter could not code spatially. The absence of such a clear-cut difference might imply that even the sighted children did not, in fact, use a spatial code in attempts to solve the problems.

We wish to thank the headmaster, staff and students of Dorton House School and Hanover Primary School for their cooperation in this study.

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(*Manuscript received* 4 *June* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 387–393
Printed in Great Britain

SEMANTIC CONSTRAINTS ON LOCATION OPERATIONS IN SIMPLE SENTENCES

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Subjects were timed in their recognition responses to single words taken from short active sentences. The pool of stimuli included items which were synonyms of words present in the sentence, or items which would make a meaningful insertion into the sentence frame. The response latencies derived from this study suggest a general 'priming' effect on near associates of critical words, this being constrained by the function of the sentence as a whole. Synonymous verbs were rejected relatively rapidly, but the latency to reject nouns differed for those relating to the subject of the sentence (which were slow), and those relating to the object (which were fast). These effects are discussed in terms of the varying degrees of 'definition' of the subject, verb and object of the sentences.

Underwood (1965) proposed that when a list of words is learned and subsequently tested using a recognition memory task the occurrence of implicit associative responses (IARs) leads to the false recognition of associates of list items. The effect obtains for a variety of associative relationships. Kimble (1968) replicated Underwood's findings and further concluded that since such a wide range of associative types is falsely recognized there is a strong suggestion that IARs occur as a by-product of the general evocation of a 'concept' relating to each critical stimulus word.

In further work Hall *et al.* (1969) have shown that even when false recognition responses do not occur overtly, reaction latencies are slowed significantly for associates of stimulus items. They proposed a model of the IAR effect based on Egan's (1958) application of signal-detection theory to recognition memory. Increments in the judged 'familiarity' of words are seen as accruing from simple repetition of critical items, but also, though to a lesser extent, from the occurrence of IARs. In a typical experimental situation the subject is assumed to establish a criterion on the 'perceived familiarity' axis and on any given trial if perceived familiarity exceeds this value the item is judged as 'old', otherwise it is seen as 'new'. In terms of this model the probability density functions for classes of stimulus items lie at different points along the dimension of perceived familiarity. Thus the probability of an 'old' judgement is greatest for repeated words (correct response), but high associates will also be judged 'old' at a rate significantly higher than control items. In addition, response latency may be expected to decrease as a function of the distance on the perceived familiarity dimension between the established criterion and the judgement made on a particular trial.

From this description it can be seen that Hall *et al.* are proposing a type of 'strength' model of memory where the likelihood of a response is some function of the number of cues or sets of features eliciting it (e.g. McLeod *et al.*, 1971). For simple lists of words each list item may be viewed as a feature complex where the feature sets for associated words overlap. In this case few constraints are imposed on either the strength or direction of cued associates. Thus false recognition of synonyms and antonyms is equal using simple lists as stimulus material (Fillenbaum, 1969). How-

ever, when phrases or adjective–noun pairs are used the free occurrence of all types of IAR is greatly attenuated, suggesting that the phrase, as a propositional entity, arouses only certain specific associates. For example, Anisfeld (1970) showed that false recognition rate to phrases was high when synonymous phrases were used, but not for antonymous words, even when these were equally high associates of stimulus items. Hall & Crown (1970) found similar results using whole sentences: false recognition rates greater than chance only obtained when an associate could have occurred as a meaningful insertion into the sentence frame.

This paper sets out an attempt to use the changes in IAR occurrence resulting from semantic constraints to examine some features of the associative network aroused by the perception of a complete sentence. In a number of studies (Kennedy & Wilkes, 1969, 1970, 1971) it has been suggested the latency of recognition responses to words taken from previously learned sentences varies partly as a function of the 'semantic load' carried by a stimulus item in the sentence as a whole. This work has normally been carried out using a pool of neutral items to be rejected, and taking response latencies to positive stimuli as an index of the changes in 'availability' of stimulus words. The present study attempts the systematic variation of the set of items to be rejected and uses the latency of this decision as an index of the changes in threshold of associates attributable to the influence of specific words within the sentence frame.

EXPERIMENT I

Method

Materials

Four sentences were constructed, each of the form Pro Adj_1 $Noun_1$ Verb Pro Adj_2 $Noun_2$, using three-, four- or five-letter words found as stimulus items in published Association Norms (Palermo & Jenkins, 1964; Postman & Keppel, 1970). The sentences used were (1) *That thin boy ate some hot food.* (2) *My white mouse took that hard cheese.* (3) *The tall priest found the sick child.* (4) *Her warm hands held this soft cat.* For the five critical 'content' words (i.e. two adjectives, two nouns and the verb) within each sentence four types of 'associate' were derived: (*a*) An associate occurring with reasonable frequency and which could make a plausible insertion into the sentence frame but not maintaining the same meaning. (*b*) A similar associate but with a meaning much closer to that of the matched stimulus item – in some cases synonyms could be found. (*c*) An associate to the critical stimulus word but one which could not make a possible insertion into the sentence frame. (*d*) A randomly chosen word simply of roughly equal frequency of occurrence to the stimulus item. In this way a pool of 20 words to be rejected was formed, and random sequences were drawn up consisting of four repetitions of each stimulus word together with these 20 associates for each of the four sentences.

Procedure

Each subject learned all four sentences. Orders of sentence presentation were completely counterbalanced twice in the design. The learning phase consisted of subjects reading a sentence from a card and then recalling it with the card concealed. After three successful recalls the subject was presented with a randomized set of 40 test items. Stimuli appeared in a tachistoscope displayed for 2 sec. and the subject responded by pressing a right-hand button to indicate presence of the stimulus in the original sentence, and a left-hand button to indicate absence. Response latencies were recorded in msec.

The learning phase for successive sentences immediately followed the preceding test series. The experimental session lasted approximately 30 min. and was carried out in an acoustic booth.

Subjects

Forty-eight undergraduate students in the University of Dundee served as subjects.

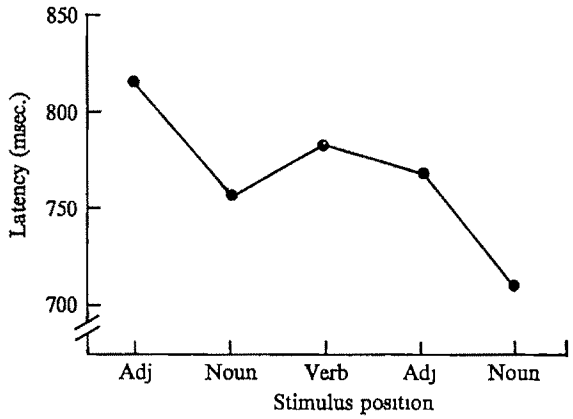

Fig. 1. Mean response latency for judgements of presence for all sentences combined.

Results

Latencies of judgements of presence. Mean response latency for judgements of presence for all sentences combined was 766 msec. Fig. 1 shows that marked differences in latency were found for responses to stimulus items taken from different positions within the sentence frame. These data were subjected to a simple repeated measures analysis of variance which showed a highly significant effect for stimulus position ($F = 21{\cdot}45$; d.f. = 4, 188; $P < 0{\cdot}001$). Comparisons using Scheffé's test showed that both nouns were recognized significantly faster than their qualifying adjectives, and the final (object) noun was identified faster than all other words. Error rates overall were less than 1 per cent. Mean response latencies at each stimulus position are shown in Fig. 1.

Latencies of judgements of absence. Mean response latencies to the four categories of stimulus items were (*a*) 'neutral' controls, 803 msec., (*b*) 'associate' controls, 839 msec., (*c*) 'meaningful insertions', 855 msec., and (*d*) 'same meaning' items, 871 msec. These differences were highly significant ($F = 27{\cdot}44$; d.f. = 3, 141; $P < 0{\cdot}001$). An analysis of variance was carried out on the data from the 'meaningful' and 'same meaning' items only, in which factors were item type ('meaningful' or 'same meaning'), and position (the relative location of the corresponding matched item in the sentence frame). Significant effects were item type ($F = 3{\cdot}01$; d.f. = 1, 47; $P < 0{\cdot}05$), position ($F = 12{\cdot}53$; d.f. = 4, 188; $P < 0{\cdot}001$), and the interaction, item type × position ($F = 4{\cdot}03$; d.f. = 4, 188; $P < 0{\cdot}01$). The nature of the interaction can be seen in Fig. 2 where stimulus items are displayed in positions corresponding to their matched words in the sentence frame. An analysis of variance on the data from 'meaningful' items alone showed position to be a significant factor ($F = 3{\cdot}67$; d.f. = 4, 188; $P < 0{\cdot}01$); Scheffé's test demonstrated that response latencies to the final position were faster than at positions 1 and 2. In the case of the 'same meaning' items position was highly significant ($F = 14{\cdot}28$; d.f. = 4, 188; $P < 0{\cdot}001$), with significant differences between all points except 3 *v.* 5 and 2 *v.* 4.

Error rates overall were less than 1 per cent.

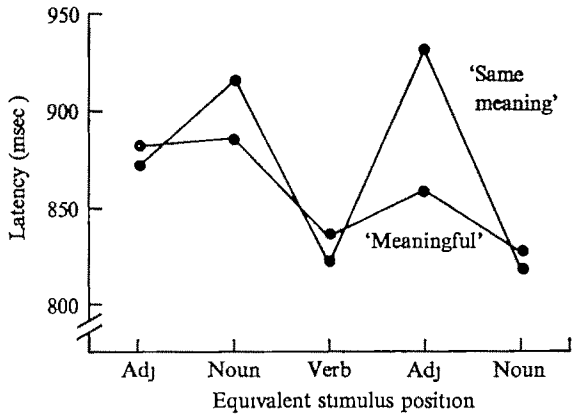

Fig. 2. Mean response latency for judgements of absence for stimulus items as a function of their corresponding matched position within the sentence frame.

Discussion

In the present study positive responses were made considerably more quickly than negative, in contrast to two previous findings (Kennedy & Wilkes, 1969, 1970). This result is difficult to interpret, however, since in the experiment reported here each positive stimulus word was presented four times and the absolute level of response latencies possibly reflects this bias.

The relative response latencies for the present results when compared with those of earlier studies shows that the latencies to positive stimuli followed a very similar pattern – head nouns being identified quickly relative to their qualifying adjectives. The significant absolute difference in latency for the two nouns, suggesting some primacy in the recognition of the object nouns, replicates the finding of Kennedy & Wilkes (1969). Speed of recognition of the verb falls at an intermediate position.

The results from the negative stimulus words can be interpreted using the model discussed earlier. In these terms a relatively fast response would suggest that the judged 'familiarity' distributions for the negative item and its matched positive are well separated, or that the overlap in feature sets is relatively low. A retarded latency would suggest the converse where more evidence must be assessed to initiate a response. The difficulty with this argument is to know from what base line to measure relative speed and at present no better estimate can be made than the mean latency for a particular condition. In the present design, however, no direct answer need be given to this question, since valid comparisons may be made between the two main classes of negative items ('meaningful' and 'same meaning'), if it can be assumed that the obtained effects are not peculiar to the particular set of words used in the present experiment. To examine this possibility a second experiment was conducted to investigate rejection latencies when the set of words occurred as a simple list.

Experiment II

Method

Materials

The 20 critical words taken from the four sentences in Expt. I were used together with the total set of 80 controls and associated words.

Procedure

Each subject was presented with a random sequence of the 20 critical words and asked to remember them, though not their particular order of occurrence. Stimuli were presented once for 2 sec. at a 1 sec. rate. Following this list presentation a randomly ordered presentation of the 20 positive words together with the 80 negative words was made. The subject responded by pressing a right-hand button to indicate an 'old' response, and a left-hand button to indicate 'new'. The 100 stimuli were presented at a 3 sec. rate in two blocks of 50 interrupted by a short rest period.

Stimulus items were presented on a CRT display remotely interfaced with a PDP-12A computer which sequenced presentation orders and recorded latencies.

Subjects

These were 10 undergraduate students in the University of Dundee.

Results

Latencies of 'old' judgements. Mean response latency for all 'old' judgements was 977 msec. An analysis of variance was carried out on these data with word class as a factor. No significant differences were found ($F = 1{\cdot}67$; d.f. = 4, 36). The error rate for 'old' judgements was 24 per cent.

Latencies of 'new' judgements. Mean response latencies for the four classes of stimulus words were (*a*) 'neutral' controls, 934 msec., (*b*) 'associate' controls, 1000 msec., (*c*) 'meaningful' items 1168 msec. and (*d*) 'same meaning' items, 1031 msec. An analysis of variance on these data showed a significant effect for item type ($F = 4{\cdot}99$; d.f. = 3, 27; $P < 0{\cdot}01$). The hypothesis that latency differences might be specific to the particular choice of items was tested in an analysis of variance on data from the 'meaningful' and 'same meaning' items only, in which factors were type ('meaningful' *v.* 'same meaning') and function (equivalent to 'position' in the analysis of Expt. I data). No significant differences were found: in the case of type the difference in latency approached significance ($F = 3{\cdot}94$; d.f. = 1, 9), but no effects were found for either function ($F = 1{\cdot}22$; d.f. = 4, 36) or the interaction ($F = 0{\cdot}22$; d.f. = 4, 36).

Errors (false positives) across the four types of item were: 'neutral' controls, 5·5 per cent; 'associated' controls, 6·0 per cent; 'meaningful' items, 11·7 per cent; 'same meaning' items, 9·4 per cent.

General discussion

The results of Expt. II illustrate the usual pattern of errors and latencies (e.g. Hall *et al.*, 1969). Associative proximity (contrasting the two sets of 'associated' items with the controls) appears to increase both latency and the incidence of false judgements of 'old'. However, no support can be found for the argument that the results of

Expt. I may be in some way specific to the words chosen. Rather, the conclusion is strengthened that the changes in latency at different 'positions' for 'new' judgements are the result of semantic constraints imposed by the sentence frames.

The most striking feature of Fig. 2 is the consistently rapid response made to reject either class of 'negative' verb. In terms of the model discussed it appears that the pivotal function of the verb within a sentence, relating as it does the subject and object, drastically reduces the effective activation of associated verbs. This is a point of high definition within the sentence with the clear implication that the result stems not from some property of verbs as such, but from specific contextual constraints.

As might be predicted from other studies, adjectives appear to possess high feature overlap, this being greater the closer in meaning the associate becomes to the original stimulus. However, in the case of 'meaningful' adjectives no significant differences in latency appear between those which might potentially modify the subject and those which might modify the object of the sentence. This is not the case when the adjectives have a similar meaning: responses to the second (object) adjective are nearly 50 msec. slower than to the first. The constraints imposed by the sentence structure seem to widen the range of application of adjectives qualifying the object while narrowing those qualifying the subject. The results here may be compared to response latencies obtained to positive stimuli using passive sentences. Adjectives qualifying the grammatical subject (logical object) in this case are identified slowly, possibly reflecting the greater uncertainty concerning grammatical relationship within the passive construction (Kennedy & Wilkes, 1970). The effect stands in marked contrast to the results for the first and second nouns themselves. Responses to the first (subject) noun are significantly slower than to the second (object), and this effect is accentuated for nouns with similar meaning.

Taken together, the results outlined here point to the operation of a set of mutually interdependent changes in the associative network aroused by learning a sentence. These are most probably alterations in detectability brought about in the first place by selective changes in the thresholds for the availability of all words relating to some critical stimulus word – the change being greater as the associative overlap between the two increases. In addition, more subtle changes are apparent. These appear to be attributable to the differing degree of definition of the subject and object of active sentences. The object, like the verb, appears as a point of high definition. In contrast the 'spread of activation' (indexed by relatively retarded latency) around the subject is much greater.

In terms of such an analysis the results for the adjectives remain paradoxical. A possible (speculative) resolution of this difficulty might be provided by considering that a component part of the judgement of presence or absence in the case of adjectives might involve testing their applicability in relation to the two nouns. The higher number of associated features aroused by the subject, which delays rejection of equivalent nouns, speeds up the matching process for adjectives since it effectively reduces the range of possible qualifiers. The converse would then obtain in the case of the object.

This work is supported by the Social Science Research Council. Thanks are due to Mrs M. Ellen and Mrs B. Wilkinson who carried out the testing.

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(*Manuscript received* 25 *June* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 395–400
Printed in Great Britain

REASONING AND A SENSE OF REALITY

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An experiment was performed to determine whether the use of realistic materials would improve performance in a deceptive reasoning problem. The task involved selecting from a set of envelopes those which, if they were turned over, could violate a given rule. The rule concerned either a realistic relation ('if a letter is sealed, then it has a 50 lire stamp on it') or else an arbitrary relation between symbols ('if a letter has an A on one side, then it has a 3 on the other side'). Twenty-two of the 24 subjects made at least one correct answer with the realistic material but only seven of them did so with the symbolic materials. The verbal formulation of the rule was also varied but yielded only a marginal interaction with the main variable. It is argued that the critical factor is the intrinsic connexion between items rather than their specific nature.

It is a well-established fact that the content of a problem may have a significant effect upon insight into its underlying structure. Perhaps the clearest demonstration of this phenomenon in a purely deductive task is Wilkins's (1928) classic study of syllogistic inference. She discovered that problems with a familiar everyday content were generally easier than those with a purely symbolic or totally unfamiliar content: her subjects committed fewer fallacies even though the familiarity of the material provided no cue to what was, or was not, the valid conclusion. To anyone approaching thinking from a strictly formal point of view (such as the logically oriented psychologist or genetic epistemologist) this finding is both surprising and perplexing because in making a deduction the same mental operations are presumed to be carried out regardless of content. Hence, why should it be harder to execute them with one sort of material than with another? Our investigation was designed to re-examine the phenomenon and to try to answer this question.

The particular deductive problem that we chose to study was one developed by Wason (1968). The subject is presented with the four cards shown in Fig. 1, together with the following rule: *If a card has an A on one side, then it has a* 3 *on the other side.* He knows that each card has a letter on one side and a number on the other side; and his task is to choose just those cards which it is necessary to turn over in order to discover whether the rule is true or false.

This is an extraordinarily deceptive problem even for the most intelligent of subjects. The majority of them appreciate that it is necessary to select the 'A' card: if there was a '2' on its other side, the rule would be decisively falsified. But hardly any of them appreciate the converse need to select the '2' card. Yet, if there was an 'A' on its other side, the rule would be just as well falsified as in the first case.

There are several explanations for failure in this 'selection' task (cf. Johnson-Laird & Wason, 1970); and there are several procedures which enable the individual to gain a progressive insight into it (cf. Wason, 1969). But one simple way of inducing an initially correct selection might be to change the content of the problem so as to create a sense of reality about the task.

Imagine that you are a post-office worker sorting letters. It is your job to ensure that they conform to the following rule: *If a letter is sealed then it has a* 50 *lire stamp*

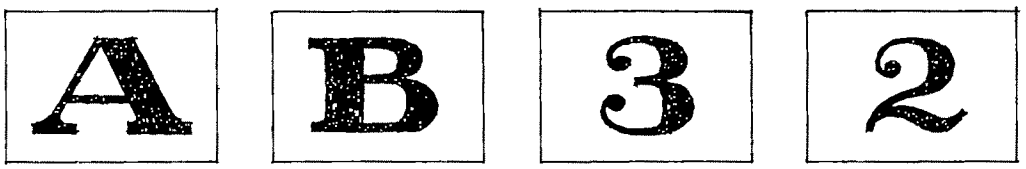

Fig. 1. Four cards used in Wason's 'selection' task.

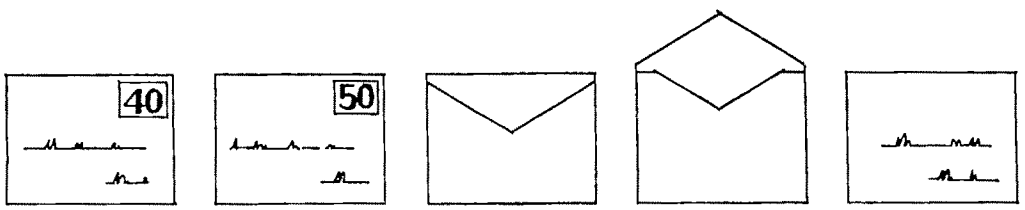

Fig. 2. The envelopes used in one condition of the experiment. The third envelope from the left was clearly sealed and the fourth envelope from the left was clearly unsealed. It was not possible to tell whether the remaining envelopes were sealed or unsealed.

on it. Which of the envelopes depicted in Fig. 2 would you need to turn over in order to discover whether or not they violated the rule? With such familiar material, where the cards themselves have become real objects from daily life, it seems relatively easy to appreciate that it is necessary to select (1) the sealed envelope (third from left in Fig. 2), (2) the envelope with the 40 lire stamp on it, and (3) the envelope with no stamp at all. The present experiment accordingly compared this 'realistic' condition with a 'symbolic' condition in which the rule referred to arbitrary numbers and letters on the envelopes. It was predicted that realism would lead the subject to a greater insight into the logical structure of the task.

The verbal formulation of the rule was also investigated, because it too seemed likely to influence insight into the task (cf. Legrenzi, 1970). Consider the following two rules:

(1) If an envelope is sealed, then it has a 50 lire stamp on it.

(2) An envelope is sealed only if it has a 50 lire stamp on it.

Logically, the two rules are equivalent: they are falsified only by a sealed envelope without a 50 lire stamp on it. There is, however, a clear difference in their meaning. Rule (1) implies that by sealing an envelope one renders it necessary to stick a 50 lire stamp on it; whereas rule (2) implies that by *not* sticking a 50 lire stamp on an envelope one renders it necessary *not* to seal it. (The difference is reflected in a more striking fashion by statements of a causal kind, e.g. 'If he's embarrassed he blushes', 'He's embarrassed only if he blushes'.) In other words, although the two formulations are identical in what renders them true or false, they differ in their implications about such contingencies. Furthermore, the formulation of rule (2) focuses attention precisely upon those contingencies customarily neglected in the selection task, i.e. upon negative instances of the second clause in the rule. This formulation should therefore produce an enhanced performance of the task. However, since this was already likely to occur with the realistic material, it was predicted that there would be an interaction between the two variables: the facilitating effect of the *only if* formulation would be greater with the symbolic material than with the realistic material.

Method

Design

Each subject acted as his own control and performed the four versions of the selection task generated by combining the two sorts of material (realistic *v.* symbolic) with the two sorts of verbal formulation of the rule (*if-then v. only if*). Since the lexical material was varied from one condition to another in order to minimize residual effects, the following four rules were used:

(1) If a letter is sealed, then it has a 50 lire stamp on it. (Realistic, *if-then.*)

(2) A letter is sealed only if it has a 5*d.* stamp on it. (Realistic, *only if.*)

(3) If a letter has an A on one side, then it has a 3 on the other side. (Symbolic, *if-then.*)

(4) A letter has a D on one side only if it has a 5 on the other side. (Symbolic, *only if.*)

The order of presentation of the conditions was counterbalanced, with Williams Squares, in order to detect transfer effects from one condition to another.

Materials

Five ordinary envelopes (12 × 8 cm) were used in each of the four conditions (see Fig. 2). For rule (1), they had Italian addresses, and the stamps were 40 or 50 lire. For rule (2), they had English addresses, and the stamps were 4*d.* or 5*d.* For rule (3), they consisted of the following items: 'A' on the front of an envelope, 'B' on the front of an envelope, '3' on the back of an envelope, '2' on the back of an envelope, and, finally, a blank envelope face uppermost. An equivalent set of envelopes was prepared for rule 4.

Subjects

Twenty-four undergraduates of University College London, acting as paid volunteers, were tested individually. They had no previous experience with this type of task.

Procedure

The subjects were told that the experiment concerned reasoning but that it was not a test of their intelligence. On the first occasion that a subject encountered a realistic condition he was asked to imagine that he was a post-office worker sorting letters; otherwise, he was merely told that he would have to examine some envelopes. The specific rule was then presented and the subject instructed to 'select those envelopes that you definitely need to turn over to find out whether or not they violate the rule'.

After the four selection tasks had been carried out, the subject was asked whether they were similar and, if so, in what way.

Results

There was a striking difference between performance in the realistic and symbolic conditions. Table 1 shows for both conditions the number of subjects making zero, one or two correct answers. Twenty-two out of the 24 subjects produced at least one correct answer in the realistic conditions, whereas only seven of them did so in the symbolic conditions. Clearly there was considerable insight in one condition and negligible insight in the other.

The correct response, symbolizing the rules as 'if p then q' and 'p only if q', is to select the 'p' and 'not-q' envelopes. This is more insightful than the selection of 'p', 'q', and 'not-q' or of 'p', 'not-p', and 'not-q', which in turn are more insightful than the selection of 'p' or of 'p' and 'q'. Table 2 summarizes the frequencies with which such selections were made in the four conditions. In fact, 22 out of the 24 subjects made more insightful selections in the realistic conditions, and the two remaining subjects showed equal insight in the two conditions. Obviously, this is an exceedingly reliable difference ($P = 0{\cdot}5^{22}$, sign test, one-tailed).

Table 1. *The numbers of subjects making a correct selection on both trials, one trial, and neither trial, in the realistic and symbolic conditions*

	Realistic conditions	Symbolic conditions
Both correct	17	0
One correct	5	7
Neither correct	2	17

Table 2. *The frequencies of the main types of selections in the four experimental conditions*

		Selections				
Conditions		p, not q	p, q	p	p, q, not q	Miscellaneous
Realistic	If p then q	21	1	1	1	—
	p only if q	18	—	2	1	3
Symbolic	If p then q	2	14	4	2	2
	p only if q	5	12	3	1	3

Table 3. *The distribution of the subjects in terms of whether or not they made the same selections in the two realistic conditions, and whether or not they made the same selections in the two symbolic conditions*

		Realistic conditions		
		Same selections	Different selections	
Symbolic conditions	Same selections	6	2	8
	Different selections	12	4	16
		18	6	24

There are, of course, two 'not q' envelopes in each condition: one of them has an item other than q, and the other a blank in place of q. In the majority of cases, a subject would treat both these envelopes in an identical fashion. However, on three occasions subjects selected the 'p' envelope together with just the blank envelope (the three miscellaneous selections of rule 4 in Table 2); and on another three occasions subjects selected the 'p' envelope together with just the other 'not q' envelope.

There was no overall effect of the verbal formulation of the rule upon performance. But there was a trend in the predicted direction towards a weak interaction between this variable and the content of the problem. Considering the *only if* formulation, a combination of a slight enhancement in the symbolic condition together with no detectable effect in the realistic condition, yielded results in favour of the interaction for 10 subjects and against it for four subjects, with 'ties' for the remaining 10 subjects ($P = 0{\cdot}09$, sign test, one-tailed).

Finally, it was observed that the selections were more stable in the realistic than in the symbolic conditions. Table 3 gives the distribution of subjects in terms of their consistency in the two conditions. It shows that 12 subjects combined consistency in the realistic conditions with inconsistency in the symbolic conditions, whereas only two subjects behaved in the opposite fashion ($P < 0{\cdot}02$, sign test, two-tailed). In the

past, selections with symbolic rules have not tended to be so labile (but cf. Wason & Johnson-Laird, 1969), hence we surmise that the present phenomenon is in part due to the effects of intermingling real and symbolic problems. However, there was a striking lack of any direct form of transfer between them; and, according to the introspective reports, only two subjects realized that there was an underlying similarity in their logical stucture.

Discussion

Realism in the task induced a remarkable degree of insight into its logical structure. Indeed, the level of performance was considerably higher than any which has so far been reported for the selection problem: 91 per cent of the subjects made a correct selection in at least one of the two realistic conditions, and just over 70 per cent of the subjects made a correct selection in both of them. The only comparable level of performance was obtained by Wason & Shapiro (1971). They used a selection task with a 'thematic' rule of the form: *Every time I go to Manchester I travel by car*, together with a set of cards representing various destinations and modes of transport. The framework of this experiment is obviously imaginary because the cards which represent journeys remain simply cards. Yet it seemed to produce a sufficient sense of realism for the subjects to be able to think not of the cards themselves but of the journeys they represented, and 62·5 per cent of them succeeded in making the correct selection. In the present experiment, however, the cards are replaced by real objects, and the task simulates a feasible and realistic activity. The subject is not required to test a rule, but to test whether or not objects conform to a rule whose truth is guaranteed. But, of course, when the rule concerned an arbitrary relation between symbols on the envelopes, the subjects relapsed to the customary level of performance in the selection task. This demonstrates that the important factor *is* the realism of the task, and this is confirmed by the almost complete absence of transfer from the realistic to the symbolic conditions. It is the content of the problems which is crucial rather than their structural identity. Hence, it is scarcely surprising that the verbal formulation of the rule had at most a marginal effect upon performance.

It is by no means clear what constitutes the fundamental cause of the greater insight with the realistic materials. One simple possibility is that it is easier to appreciate the 'reversibility' of real objects. It will be recalled that subjects treat cards with 'A' on one side and '2' on the other side in different ways depending upon which side is uppermost. It seems that the reversibility of the cards temporarily escapes them: they fail to grasp that the cards are identical apart from their orientation. Working with envelopes, however, may prevent the subjects from losing this basic operational knowledge. But if this was the real explanation, a similar facilitation in performance should occur with symbolic materials. We carried out a more stringent test of this hypothesis in a pilot study. A comparison was made between symbolic material written on cards and symbolic material written on envelopes (as in the present experiment). The latter potential aid to reversibility in no way improved performance in the task.

There remain two other possible explanations. First, the individual may find many realistic objects easier to visualize, to remember, and to manipulate mentally. This is a plausible factor, but in our view unlikely to be the critical one. The materials in

many of the previous 'symbolic' experiments have been easy enough to remember or to visualize, yet the task has remained obstinately intractable. For example, Wason & Johnson-Laird (1970) used stimuli such as circles with borders round them, but only 5·5 per cent of their subjects made an initially correct selection.

Secondly, it could be the realistic *relation* between the contingencies in the rules which leads to the superior performance. The individual is used to considering the connexion between such items as destinations and modes of transport, or postal rates and envelopes. A study by Legrenzi (1971) provides circumstantial evidence for this hypothesis. He found that when subjects had to discover for themselves the nature of an arbitrary rule relating symbolic materials, their performance in a subsequent selection task was markedly superior to that of subjects who were merely given the rule to test. Discovering the rule seemed to make it almost as familiar as a realistic one. In both cases, it is presumably easier to try out the various combinations of items in a systematic fashion, and to grasp what is pertinent to the truth or falsity of the rule.

If this hypothesis is correct, then it might be possible to discover realistic connexions which in fact hinder the process of deduction. This is precisely the result obtained by Johnson-Laird & Shapiro (cf. Wason & Johnson-Laird, 1972). They constructed reasoning problems that could be solved only by making a hypothetical assumption. When the causal aspects of the premises were consistent with the logical requirements of the task, it was a simple matter to draw the correct conclusion. But when the two ran counter to one another, causal thinking prevailed and it was extremely difficult to draw the correct conclusion.

It is a task for the future to conduct direct tests of this explanation of the results, and to determine the precise nature of a realistic connexion between events. It may be that a sense of reality is no more than a feeling of familiarity.

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(*Manuscript received* 3 *September* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 401–405
Printed in Great Britain

QUALITATIVE REWARD SHIFT IN THE DOUBLE RUNWAY

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Ten rats received 90 trials, one per day, in an L-shaped double runway. For 70 acquisition trials reward in both goal boxes was two 45 mg standard diet Noyes pellets. On 10 of the subsequent 20 test trials reward in the first goal box was changed to two 45 mg sucrose pellets (a preferred food). During the test period first alley run times fell below the training level. Second alley start times following sucrose were greater than those following normal reward, although this difference decreased over trials. There was no difference between second alley run times following normal and shifted reward. Results are discussed in relation to 'elation' effects following a quantitative upshift in reward.

Crespi (1942) reported facilitation of responding following shift to increased reward, termed an 'elation' effect. While later studies have failed to repeat this finding and have offered methodological criticisms (e.g. Spence, 1956), recent attention has been directed to the measurement of responding immediately following increased reward, for which the double runway (Amsel & Roussel, 1952) has been used, increasing reward in the first goal box (GB 1). Such studies have not yielded consistent results. Bower (1962), Meyer & McHose (1968), Karabenick (1969), Strongman & Wookey (1969) all reported that subjects ran slower in the second alley (A 2) following increased reward than following normal reward. Prytula & Braud (1970) reported little effect of GB 1 reward magnitude in that A 2 speeds converged quickly to a common asymptote. Comparison with control groups always receiving the larger reward has in only one case (Meyer & McHose, 1968) indicated facilitative effects of increased reward. As Strongman *et al.* (1971) point out, these differences may be a reflexion of the variety of procedures used in the various studies. A further problem is that of response suppression, i.e. greater drive reduction following the larger reward, which has been held responsible for within-subject suppression.

One method of isolating 'elation' effects from those of response suppression is to use a qualitative reward shift, i.e. a shift to the same quantity of a preferred food. The purpose of the present study was to investigate the effects of a shift in GB 1 from a standard diet reward to an equal quantity of sucrose, preference for the latter being well established (Young, 1961).

Method

Subjects

Ten male hooded rats of the Lister strain, experimentally naive and aged about 100 days at the start of training, served as subjects.

Apparatus

An L-shaped double runway constructed of chipboard lined with perspex was used. The interior walls of the start box (SB), first alley (A 1) and first goal box (GB 1) were black; the floor of these sections was black with $\frac{1}{8}$ in. transverse grooves at $\frac{1}{2}$ in. intervals. The interior walls and floor of the second alley (A 2) and of the second goal box (GB 2) were white; the floor of these sections was smooth. The entire maze was covered with hinged sections of darkened perspex. The interior width was 3 in. throughout, except for GB 2 which was $4\frac{1}{2}$ in. wide. Internal height was 3 in., except for SB and GB 2 which were both 5 in. high. The lengths of the various sections were

as follows: SB 14 in.; A1 4 ft.; A2 11 ft.; GB2 17 in.; GB1 was L-shaped with a total maximum length of 21 in. The food cup in GB1 was a depression in a piece of blue perspex set into the floor at the end nearer GB2; in GB2 a small glass dish was located at the end of the box.

Solenoid operated guillotine doors separated the SB and GBs from the alleys. Ten microswitch trip panels (MTP) were located in the floor of the maze as follows: MTP1 in A1, 6 in. from SB door; MTP2 in A1, 3 ft. from MTP1; MTP3 in GB1, 3 in. before exit door; MTP4 in A2, 6 in. from GB1 door; MTP5–9 in A2, at 2 ft. intervals from MTP4; MTP10 in GB2, 3 in. before the food cup. A Sodeco impulse counter fed by a $\frac{1}{10}$ sec. impulse generator was associated with each MTP, allowing recording of time in each section of the maze.

Programming and recording apparatus was situated outside the experimental room. White noise masked extraneous sounds inside the room.

Procedure

The experiment lasted for 100 days, divided into four periods. During days 1–6 the subjects were habituated to handling and their weight was reduced to approximately 85 per cent of free feed level, at which they were maintained for the duration of the experiment. Water was freely available in the home cage at all times. On each of days 7–10 the subjects were placed individually in GB2 for 2 min. with two 45 mg standard diet Noyes pellets available. By day 10 all subjects ate immediately following placement.

On each subsequent day each subject received one trial in the maze. During acquisition (days 11–80) the procedure for an individual trial was as follows: the subject was placed in SB; the experimenter left experimental room, started programme and white noise; after 5 sec. SB door opened; the subject traversed A1, entered GB1, received two 45 mg standard diet Noyes pellets; activation of MTP3 closed entrance door, started GB1 delay timer; after 10 sec. GB1 exit door opened; the subject traversed A2, entered GB2, received two 45 mg standard diet Noyes pellets; activation of MTP10 closed GB2 door, started GB2 delay timer; after 5 sec. white noise ended, the experimenter entered room, the subject was removed from maze and returned to home cage, receiving daily ration after 15 min.

The test period (days 81–100) differed from acquisition only in that on 10 out of the 20 trials the reward in GB1 consisted of two 45 mg sucrose pellets. The shifted reward trials (S) were interspersed with normal reward trials (N) according to the following schedule: SNSNNSSNNSN-SSNNSSNSN.

The following measures were recorded for each subject on each trial: (1) time from opening of SB door until activation of MTP1 (A1 start time); (2) time from MTP1 to MTP2 (A1 run time); (3) time from MTP2 to MTP3 (A1 goal time); (4) time from opening of GB1 exit door until activation of MTP4 (A2 start time); (5) time from MTP4 to 5, MTP5 to 6, MTP6 to 7, MTP7 to 8, MTP8 to 9 (A2 run 1, run 2, run 3, run 4, run 5); (6) time from MTP9 to MTP10 (A2 goal time). A2 run time was defined as run 1 + run 2 + run 3 + run 4 + run 5.

RESULTS

Following the introduction of sucrose in GB1 on 50 per cent of the trials, A1 run time decreased (see Fig. 1). No such decrease is evident over the last 25 acquisition trials, obviating the possibility that this is a learning effect. A similar trend occurred in A1 start time.

A2 start and run times (see Fig. 2) following normal and shifted reward were compared by analysis of variance (reward type, trials, subjects; Kirk, 1968). A log transformation of the individual scores was performed to normalize the data. Start time was significantly greater following sucrose ($F = 15{\cdot}3$; d.f. = 1, 9; $P < 0{\cdot}01$). The reward type × trials interaction was also significant ($F = 3{\cdot}1$; d.f. = 9, 81; $P < 0{\cdot}01$) implying a reduction of the start time effect over trials. Run time did not show any differential effects of reward type. On both analyses a significant main effect of subjects was found. Inspection of the data indicated that this can be attributed to individual differences in speed.

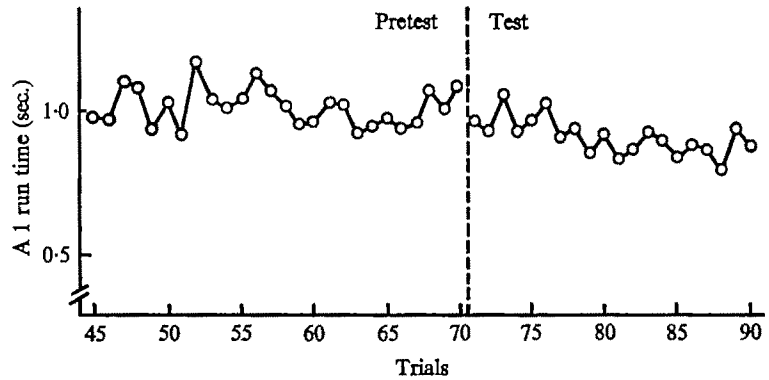

Fig. 1. Group mean run time in the first alley for acquisition trials 45–70 and test trials 71–90.

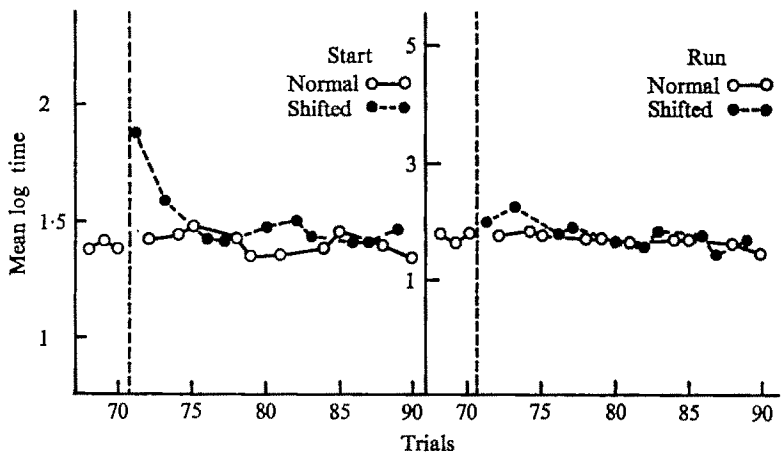

Fig. 2. Group mean log start time and log run time in the second alley for acquisition trials 68–70 and test trials 71–90. The two curves for each measure in the test period represent performance following normal and shifted reward.

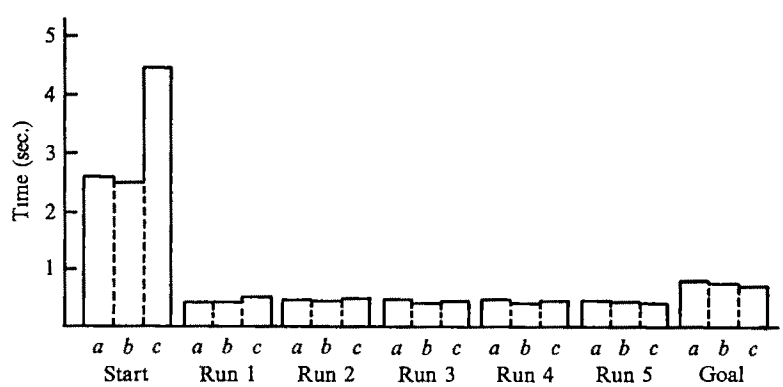

Fig. 3. Time per unit distance in each of the seven sections of the second alley, runs 1–5 corresponding to consecutive 2 ft. segments. (*a*) Group mean over acquisition trials 61–70; (*b*) group mean over the 10 normal reward test trials; (*c*) group mean over the 10 shifted test trials.

Analysis of time in each section of A 2 is shown in Fig. 3. Clearly the effect of GB 1 reward is confined to start time. As the distance over which this was measured is small, it appears that the effect of shift to a preferred reward is to delay the onset of responding. The instrumental performance is unaffected.

DISCUSSION

Three main points emerge from the present results: (1) subjects remain longer in GB 1 following preferred reward than following normal reward; (2) this effect is transitory (cf. Prytula & Braud, 1970); (3) performance in A 2 is independent of the quality of reward in GB 1.

The absence of any effect on running in the second alley is consistent with the majority of studies of quantitative reward increase, indicating that within-subject performance decrement following increased reward may be attributed to drive reduction. However, this is in conflict with the 'apparent elation effect' reported by Meyer & McHose (1968), the only double runway study to find facilitation (on both start and run performance) following increased reward. The discrepancy between the latter and other studies of quantitative increase may be a consequence of the different schedules employed. Meyer & McHose shifted reward on one trial in six, whereas Strongman & Wookey (1969) used a 50 per cent increase; Karabenick (1969) and Prytula & Braud (1970) presented increased reward on all post-shift trials. This may imply that a larger reward often presented quickly ceases to give rise to 'elation'. The rapid development of a 'frustration effect' following normal reward after brief experience of increased reward (McCain & McVean, 1967; Strongman & Wookey, 1969) lends support to this as does the convergence of performance in the second alley irrespective of GB 1 reward magnitude (Prytula & Braud, 1970). But such a hypothesis does not resolve the difference between the 'apparent elation effect' and the present results. *Prima facie* it could be argued that in the present case the longer start times following sucrose reflect frustration following normal reward. However, this is clearly not so since at no point in the second alley are pretest run times different from those following normal reward during the test period (see Fig. 3). These considerations suggest that qualitative and quantitative reward increases have different effects. This is possible – the general field of 'elation' is not notorious for consistent results (see Strongman *et al.*, 1971 for review) – but more extensive analysis of the problem is necessary before it can be regarded as probable.

The study was supported in part by a Science Research Council Postgraduate Studentship to the first author and by Medical Research Council grants G/967/271/B and G/969/269/B to the second author.

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(*Manuscript received* 8 *June* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 407–409
Printed in Great Britain

DISCRIMINATION LEARNING IN THE PIGEON WITH TWO RELEVANT CUES, ONE HARD AND ONE EASY

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Pigeons were trained on a colour/shape discrimination. Colour was shown to be the operative cue. No learning of shape was demonstrated after 500 overtraining trials, whereas the shape cue alone was learned in fewer trials than this. Learning of the 'hard' shape cue was thus suppressed by the presence of the 'easy' colour cue. It was not eliminated, however, for the shape cue was learned after 1000 overtraining trials.

Certain theories of discrimination learning (e.g. Sutherland, 1964) propose that the more an animal learns about one cue the less it will learn about any other cue. In support of this position Sutherland & Holgate (1966) showed that when rats were trained with two relevant cues there was, for individual subjects, a negative correlation between the amount learned about each cue. A similar result was obtained by Trabasso & Bower (1968) using human subjects. They suggested that it was implausible to suppose this result was due to insufficient training on the non-dominant cue, i.e. there seemed to be some suppression of learning about one cue by the presence of another. Lovejoy & Russell (1967), using rats, showed suppression of learning about a hard cue by the presence of an easy cue; from this study they conclude that 'once an animal has solved the problem by means of the easy cue, he ceases to learn about the hard cue', either because additional learning about all cues stops when the problem has been 'solved', or because having solved the problem in terms of one cue this is learned to the exclusion of others. Other evidence (e.g. Sutherland & Holgate, 1966) indicates that with extended training the negative correlation between learning each of two relevant cues disappears; animals eventually learn about both cues. A parallel study with humans (James & Greeno, 1967) showed that during initial learning attention is selective but with overtraining selection is relaxed. The present study investigates the effect of suppression of learning about a hard cue by the presence of an easy cue, with a test for learning of the easy cue after an extended period of training.

Method

Apparatus.

The apparatus was a modification of one previously used (Williams, 1967). It consisted of a hardboard sheet which was fitted over the whole front of the bird's home cage in place of the normal door; the cage was approximately 160 cm square. In the board were 50 circular apertures, 2·5 cm in diameter, arranged in five rows of 10; the rows were offset beneath one another so that all the apertures were placed within an area 18 cm high and 90 cm wide. On the back of the board was bolted a backboard with 50 cavities matching the apertures. Between the boards was placed a sheet of white tissue paper, on which stimuli were printed by means of rubber stamps. A single maple pea was placed in the cavity behind the positive stimulus. Birds responded by pecking at the stimuli, thus breaking the tissue paper to obtain the seed.

Stimuli

An open square, an open symmetrical cross, and an open circle were used, each of an area 0·64 cm^2 drawn in lines 0·15 cm wide.

Subjects and procedure

Experimentally naive hybrid pigeons between 6 and 18 months old were used. They were maintained at 80 per cent of their free-feeding body weight. During pretraining no stimuli were presented, and on the first few trials holes were made in the tissue so as to expose the seeds. On training trials there were 25 reinforced and 25 unreinforced stimuli per board, with five boards presented each day. A board was removed from the cage when a response had been made to each of the 25 positive stimuli.

Three birds were trained on a square/cross discrimination with black shapes and cross positive. Twelve other birds were trained on a blue-square/red-cross discrimination with red cross positive. These were then divided randomly into three groups. Group C received a generalization test on the fifth and sixth boards. Group E 500 received 500 reinforced overtraining trials, and Group E 1000, 1000 reinforced overtraining trials, following the first four training boards; they then received two further boards with a generalization test on each.

A generalization board consisted of 15 of each of the training stimuli, and 10 each of two reinforced generalization stimuli. On the first generalization board, to test for learning of the shape cue, the test stimuli were black square/black cross. On the second board, to test for learning of the colour cue, the test stimuli were blue circle/red circle. A generalization trial was terminated when a response had been made to 10 of the test stimuli.

RESULTS

All birds trained on the colour–shape discrimination reached a criterion of a maximum of five errors per board by the fourth board (100 reinforced trials). The three birds trained with black shapes took 12, 17 and 18 boards to achieve this criterion (maximum 450 reinforced trials). Each bird in group C and Group E 500 made a significant choice of the red circle over the blue circle on the generalization test ($P < 0{\cdot}01$, preferred stimulus chosen 9 or 10 times out of 10) but there were no significant differences in responses to the black cross and black square (choice of one stimulus five or six times out of 10). In Group E 1000 each bird made a significant choice of the red circle over the blue circle ($P < 0{\cdot}01$), and of the black cross over the black square ($P < 0{\cdot}01$). The P values for generalization responses were calculated with regard to the fact that sampling was without replacement.

DISCUSSION

Previous work (Towe, 1954; Williams, 1966) suggests that pigeons find colour cues easier than shape cues. This is borne out in this study, the shape cue taking at least three times as long to master as the colour cue. There is apparently a suppression of learning about a hard (shape) cue by the presence of an easy (colour) cue; no learning was demonstrated of the shape cue in Group E 500 after 600 reinforced trials on the colour–shape problem, whereas the maximum time taken to learn the shape problem alone was 450 reinforced trials. This was a suppression effect and not a complete elimination of learning about the hard cue, for the shape cue was learned eventually (group E 1000), albeit after 1100 reinforced trials.

This result is then consistent with work with animals (Sutherland & Holgate, 1966)

and humans (James & Greeno, 1967), and suggests that the interpretation put on the cue suppression effect by Lovejoy & Russell (1967) is too extreme. The problem such results pose for attention theory are discussed fully by Trabasso & Bower (1968), who while unable to offer a complete solution suggest possible directions for theoretical analysis.

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(*Manuscript received* 13 *September* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 411–420
Printed in Great Britain

CONFLICT RESOLUTION BY SEPTAL, DORSAL HIPPOCAMPAL OR VENTRAL HIPPOCAMPAL LESIONED RATS WITH PRE- OR POST-OPERATIVE APPROACH TRAINING

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Rats with septal, dorsal hippocampal or ventral hippocampal lesions were observed during the development of approach behaviour followed by the introduction of an approach–avoidance (food–shock) conflict which was eventually resolved in favour of approach. Surgery was performed either prior to or after the acquisition of the initial approach response. Septal lesions made before approach training resulted in a loss of inhibition during the early stages of conflict, whereas lesions made just prior to the conflict resulted in increased inhibition. Irrespective of the timing of surgery, rats with dorsal hippocampal lesions demonstrated 'normal' avoidance behaviour on the trials immediately after shock, but overcame the fear sooner than the control subjects. Ventral hippocampal lesions produced less avoidance behaviour when made before approach training than when made just prior to the conflict. Possible functions of the various structures are discussed.

There is an increasing body of evidence suggesting that the rat's hippocampus is anatomically (Raisman *et al.*, 1966; Nauta, 1956) and functionally (Grant & Jarrard, 1968; Jackson, 1968; Kimura, 1958) a heterogeneous structure. One possible function of the dorsal and ventral areas has been proposed by Nadel (1968), who hypothesized that the dorsal hippocampus functions in the attenuation of responses to motivational stimuli whereas the ventral hippocampus is involved in behavioural flexibility. Fried (1971) examined these hypotheses by observing lesioned animals during the development of an approach–avoidance conflict and a subsequent resolution of that conflict in favour of approach behaviour. The effect of dorsal hippocampal lesions apparently supported Nadel's (1968) proposal of heightened responsiveness to motivational stimuli as the development of avoidance responding to footshock was as fast as the experimental design permitted and the subsequent learning of an approach response using food reinforcement was significantly faster than the control animals. On the other hand, although the ventral hippocampal group did differ from the dorsal hippocampal group, the results did not confirm Nadel's hypothesis of loss of response flexibility. In the same task, rats with septal lesions were impaired in their ability to change approach responding into avoidance with the dysfunction attenuating during the course of the experiment.

Extensive hippocampal and septal lesions in rats are typically reported to result in behavioural changes characterized as a loss of some sort of inhibiting mechanism (Douglas, 1967; Kimble, 1968; McCleary, 1966). However, several reports have indicated that, under particular experimental conditions, a response inhibition deficit is not manifested in rats with hippocampal damage (Fried, 1970; Fried & Goddard, 1967; Middaugh & Lubar, 1970; Wishart & Mogenson, 1970) or in rats with septal damage (Fallon & Donovick, 1970; Fried, 1969, 1970; Gittelson *et al.*, 1969; Middaugh & Lubar, 1970). It has been proposed that the disinhibition following

hippocampal lesions is dependent, in part, upon a pre-operatively established approach 'set' (Fried & Goddard, 1967), whereas the loss of a response inhibition mechanism in rats with septal lesions is dependent upon a post-operative approach (appetitive) training period (Fried, 1969).

The present studies are designed to investigate, in a straight alley approach–avoidance conflict task, the effects of dorsal hippocampal, ventral hippocampal, or septal lesions made either before or after approach training. In addition to latencies being recorded, the experimental procedures were such that it was possible to quantify the degree of approach behaviour to a source having both negative and positive properties, i.e. anthropomorphically, a measurement of boldness (Thomas *et al.*, 1968).

GENERAL PROCEDURE

Subjects

Sixty-one male albino rats of the Wistar strain averaging 60 days of age and 200 g. in weight at the beginning of the experiment were used as subjects.

Surgery and histological method

Operations were performed under sodium pentobarbital anaesthesia (60 mg/kg). Electrolytic lesions in the experimental animals, made with a stainless steel needle insulated except for 0·5 mm at the tip, were aimed bilaterally at either the anterodorsal hippocampus, the postero-ventral hippocampus, or the lateral edge of the medial septal nucleus. The coordinates and current parameters used have been reported elsewhere (Fried, 1971). The control animals were anaesthetized but surgically intact.

Following the experiments, animals were sacrificed with an overdose of sodium pentobarbital, perfused through the heart with normal saline followed by 10 per cent formol saline. The brains were removed from the skull, frozen, sectioned at 60 μ and stained with thionin.

Apparatus

The apparatus was a 1·5 m black alley containing a 17 cm start box and a 27 cm goal box with a concave, circular plastic food cup located 2·5 cm from the end wall of the goal compartment. The start and goal boxes were separated from the rest of the alley by guillotine doors. When the start box door was raised, a timer was activated by means of a snap-action switch. This timer was stopped when a photocell beam, located above the food cup, was broken. In addition to the timer, 16 photocells were connected to an electronic counter. The photocells were spaced 8·5 cm apart, commencing 23 cm in front of the start box door and ending with the photocell over the food cup. As the animal moved towards the food cup, the successive photocell beams broken were recorded. Retracing by the animal did not activate the counter. With this arrangement, the point to which the animal had proceeded towards the food cup at any given time could be monitored.

The breaking of the final photocell beam, in addition to stopping the timer and being recorded on the electronic counter as 16, delivered, on appropriate trials, an electric shock of 0·40 mA, 60 cycle a.c. for 0·1 sec. to the floor of the goal box.

Method

Upon their arrival in the laboratory, all animals were placed upon a 23 hr. food deprivation schedule, housed individually, with water available *ad lib.* This feeding regimen was followed throughout the experiments with the exceptions noted below.

In the two studies, groups were operated on at different times but, during the 8-day recovery periods, all animals were treated in the same manner, whether or not they had undergone surgery. All animals had food available *ad lib.* in their home cages for 5 days and then were returned to the 23 hr. deprivation schedule 2 days before testing was resumed. During the recovery period, the animals were rated for hyper-irritability in response to normal handling.

Experiment I

The first study was a replication of earlier work (Fried, 1971) designed to examine the effects of extensive post-operative approach training prior to the introduction of an approach–avoidance conflict. In the present study the addition of the photocells and the electric counter allowed a more detailed characterization of the effects of the various lesions.

Method

Groups

After 4 days of handling, dorsal hippocampal lesions were made in 10 animals, ventral hippocampal lesions in nine animals, septal lesions in eight subjects, and 10 animals served as anaesthetized controls.

Procedure

After the 8-day post-operative recovery period, animals were given three familiarization sessions consisting of placing each rat in the alley with both guillotine doors raised and allowing 10 min. free access to 45 mg food pellets (P. J. Noyes Company) placed in and near the food cup. Following the familiarization phase, the rats received 6 days of acquisition training consisting of 10 reinforced trials daily with a 10 min. intertrial interval. Two 45 mg food pellets were used as positive reinforcement on each trial throughout the study. At the beginning of each trial, the rat was placed in the start box, the guillotine door raised, and the animal allowed to enter the alley. As the rat progressed down the alley, the electronic counters were activated sequentially.

The 6-day acquisition period was followed by an 8-day rest period (equivalent to the post-operative recovery period in the second experiment) and then an approach–avoidance conflict was introduced. On the first three trials of the seventh day of testing and the first and tenth trial of the eighth day, the rats received a footshock in the goal box when they took the food. During the 10 trials of the last test day, no shock was administered. If, at any point during the study, a subject took longer than 120 sec. to take the food, it was placed in the goal box for 20 sec. and, if scheduled, was given a footshock. The latency on these trials was recorded as 120 sec., and the photocell beam closest to the food cup which was broken within 120 sec. was noted.

Results

The latencies were recorded for each subject on each trial, and were analysed with a Lindquist Type I analysis of variance, with the Newman–Keuls test being used to examine differences between pairs of groups. The between-subject variable was the four groups and the within-subject variable was the individual trials during the acquisition or during the conflict phase.

The four groups were quite similar by the end of the acquisition phase with all subjects consistently exhibiting a latency of less than 3 sec. However, in the approach–avoidance portion of the study, the groups varied during different portions of the conflict ($F = 1{\cdot}56$; d.f. = 87, 957; $P < 0{\cdot}002$). The mean latencies during the 30 trials of approach–avoidance training shown in Fig. 1 were quite similar to those reported previously (Fried, 1971).

A day-by-day analysis of this phase revealed that the septal lesioned animals ran significantly faster than controls, with the difference decreasing over days. In fact, by the end of the last day, the septal group was running slightly slower than the control subjects.

In contrast to the septal animals, the dorsal hippocampal lesioned subjects did

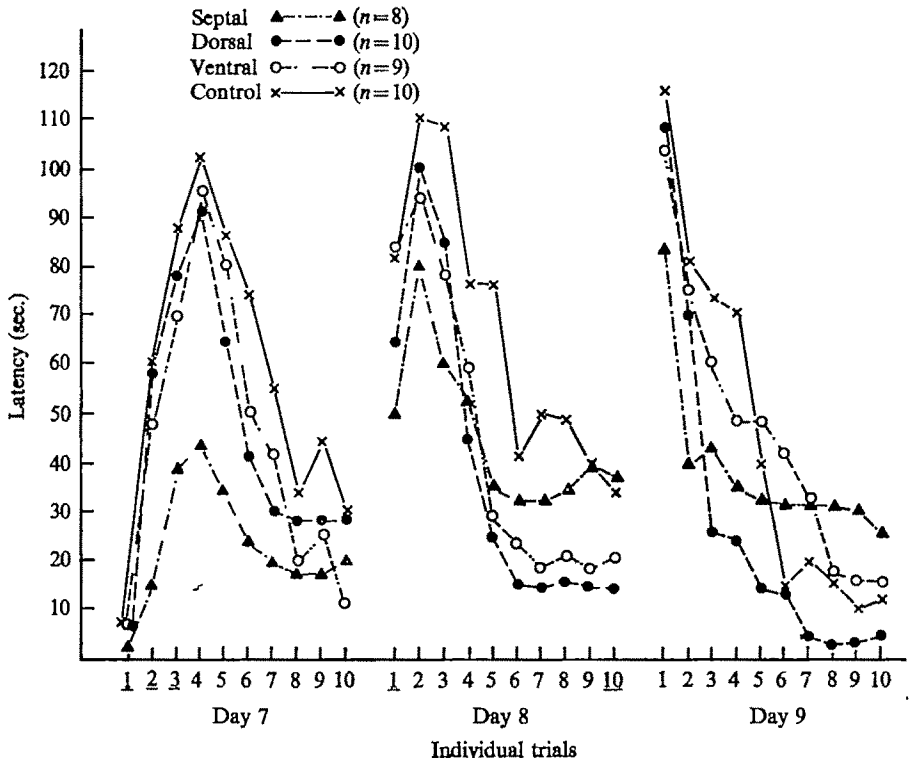

Fig. 1. Mean latencies of individual trials during conflict in Expt. I. The underlined trials are ones in which shock occurred.

not differ from controls in their latencies following shock. However, an examination of the development of running speeds after the punished trials indicated that the dorsal hippocampal group returned to a rapid speed at a significantly faster rate than controls ($F = 19{\cdot}13$; d.f. = 29, 522; $P < 0{\cdot}001$) and to a significantly greater degree ($F = 9{\cdot}07$; d.f. = 1, 18; $P < 0{\cdot}01$). The ventral hippocampal group were quite similar to the control and dorsal hippocampal animals with respect to response to shock. However, unlike the dorsal hippocampal group, the ventral lesioned animals did not return to a significantly faster running speed than the controls.

An overall analysis of the photocell data indicated that the groups differed with respect to how close they approached the goal box in the approach–avoidance phase ($F = 1{\cdot}73$; d.f. = 87, 957; $P < 0{\cdot}001$). The data are presented in Fig. 2. During the three days of this phase, the control animals, on the trials immediately after shock, demonstrated a rather broad avoidance gradient for, as a group, they did not progress further than approximately a third of the way towards the goal box. The rate of overcoming this avoidance behaviour increased in rapidity during each successive day of this phase. The septal group, on the other hand, as suggested by the latency data, maintained a strong approach tendency during the seventh day of testing but, by the end of the eighth and during the ninth day, the control subjects were approaching closer to the goal than the septal lesioned group.

The ventral hippocampal subjects displayed less avoidance than the animals in

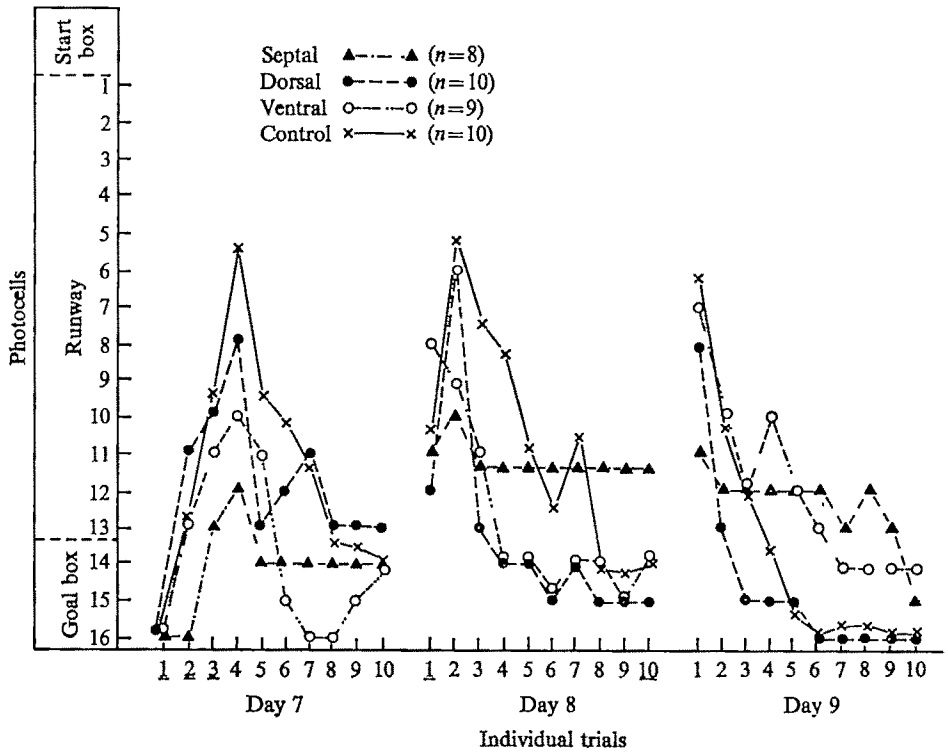

Fig. 2. Mean points of advancement towards the goal of each trial during conflict in Expt. I. The underlined trials are ones in which shock occurred.

the control or dorsal hippocampal group ($P < 0{\cdot}05$) on the seventh day of testing. However, on the eighth day, the two hippocampal groups were quite similar, with both approaching significantly ($P < 0{\cdot}05$) closer to the goal than the control subjects. On the last ten trials, there was a reversal of the seventh day pattern as the dorsal hippocampal group approached significantly closer ($P < 0{\cdot}05$) to the goal than the ventral group.

None of the animals demonstrated hyper-irritability during the course of the study.

Experiment II

In this study, the effect of pre-operative approach training was examined on subsequent approach–avoidance conflicts in animals with dorsal hippocampal, ventral hippocampal or septal lesions.

Method

The procedure and apparatus in this experiment were identical to the first with the exception of the timing of surgery. In the present study, rats underwent surgery following the 6-day acquisition phase. In each of the four groups, there were six animals.

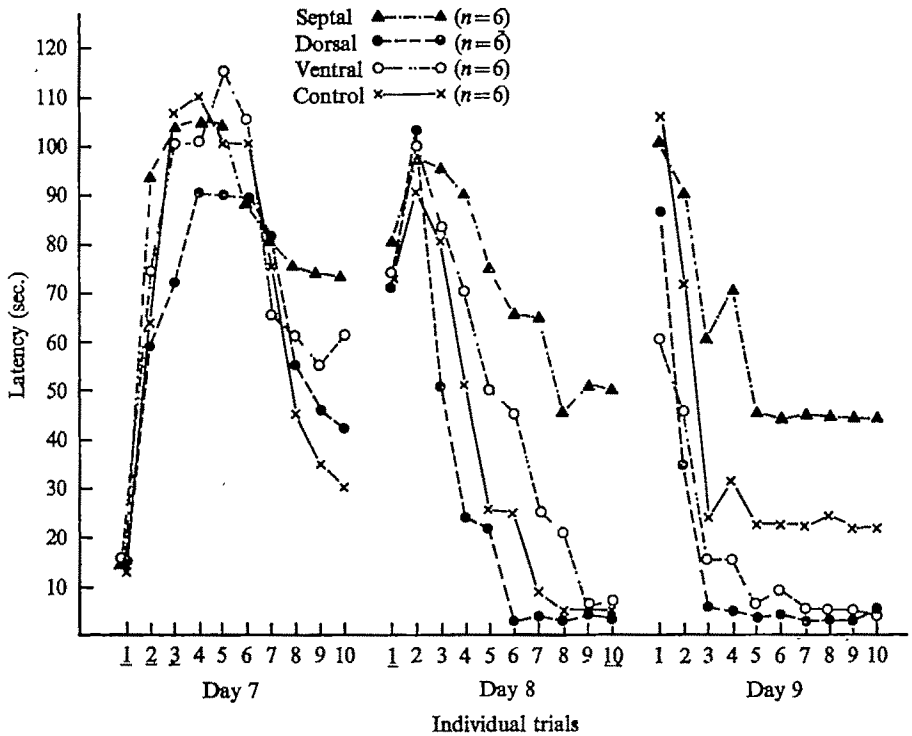

Fig. 3. Mean latencies of individual trials during conflict in Expt. II. The underlined trials are ones in which shock occurred.

Results

During the acquisition phase all groups were quite similar and therefore only the latencies of the last three days will be discussed. These data are graphically presented in Fig. 3.

An analysis of the latency data indicated that there was no overall significant difference between groups in the last three days of testing. However, analysing each day of the conflict phase separately revealed a significant difference between groups on day 8 ($F = 3{\cdot}94$; d.f. = 3, 20; $P < 0{\cdot}05$) and day 9 ($F = 3{\cdot}21$; d.f. = 3, 20; $P < 0{\cdot}05$). A separate analysis of pairs of groups using the Newman–Keuls test was performed in order to examine the results in detail.

The septal lesioned animals ran at a slower speed than the controls during the conflict phase of the study, with the differences between the groups reaching statistical significance on the eighth ($P < 0{\cdot}01$) and ninth ($P < 0{\cdot}05$) day. Both groups were similar in response latencies on trials immediately after shock, but the septal subjects did not return to fast approach speeds to the degree that control animals did. The dorsal hippocampal animals were quite similar to the control and septal groups in their latencies on trials immediately after shock. However, the return to rapid running speeds was quite different between the groups. Unlike the septal group, those animals with dorsal hippocampal lesions recovered their rapid approach behaviour faster than the controls on the last two days of testing ($P < 0{\cdot}05$). The ventral hippocampal

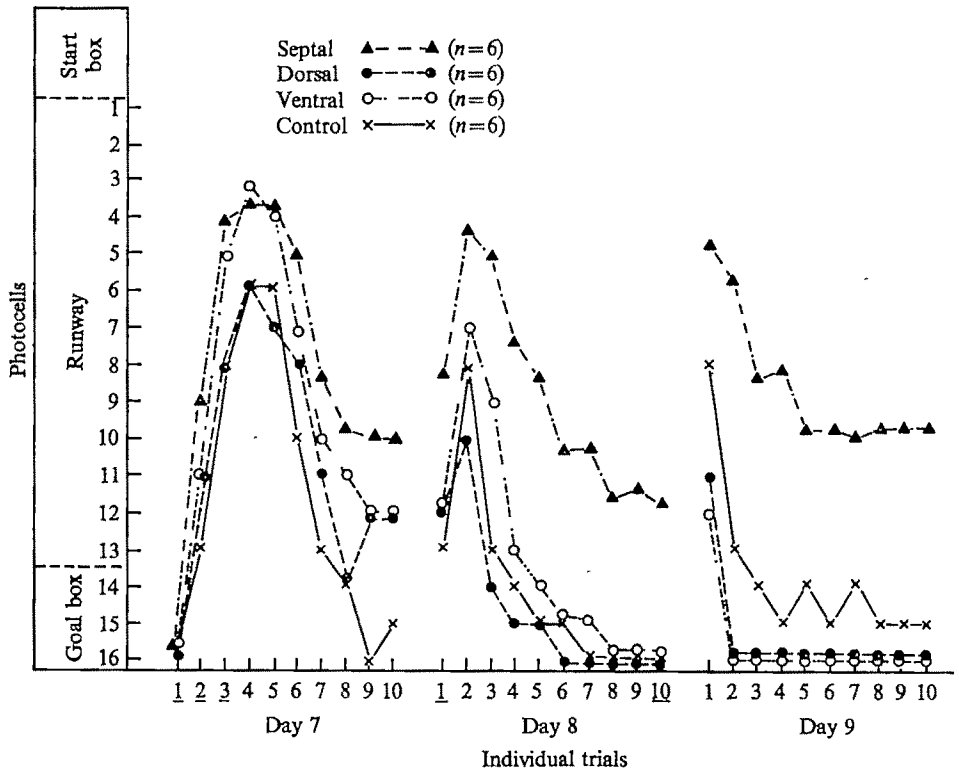

Fig. 4. Mean point of advancement towards the goal of each trial during conflict in Expt. II. The underlined trials are ones in which shock occurred.

group were quite similar to the dorsal hippocampal group on day 7, displaying slightly longer latencies during the last six trials. On the eighth day of testing, the ventral hippocampal subjects ran significantly slower than the dorsal hippocampal group. However, on the last day of testing, the two hippocampal groups were quite similar, both exhibiting speeds which were significantly faster than the controls ($P < 0{\cdot}05$).

An overall analysis of the photocell data indicated that the four groups differed significantly with respect to the degree of approach towards the goal ($F = 4{\cdot}61$; d.f. = 3, 20; $P < 0{\cdot}02$). These data are presented graphically in Fig. 4. As in the first study, the control subjects exhibited a rather broad avoidance gradient on the trials immediately after shock. The number of trials required to overcome the fear decreased over the three days of conflict training. Those animals with septal lesions had an avoidance gradient which was broader than the control subjects. The lesioned group also maintained the fear level (as measured by distance from goal after 120 sec.) significantly longer than the control animals ($P < 0{\cdot}01$).

Animals with dorsal hippocampal lesions approached the goal to a significantly greater degree ($P < 0{\cdot}05$) than the ventral hippocampal group on the trials immediately after shock on days 7 and 8, but this difference was not present after several non-shock trials on the last day of testing.

None of the animals demonstrated hyper-irritability in response to handling during the post-operative recovery period.

Anatomical findings

Microscopic examination of the lesions revealed that the damage in the three experimental groups was very similar in size and extent to that reported in earlier studies (Fried, 1970, 1971). Animals in the septal group sustained extensive damage to the medial and lateral septal nuclei. Other structures which usually suffered damage were the anterior columns of the fornix and the diagonal band of Broca. These subjects had corpus callosum damage and two subjects sustained injury to the accumbens nuclei. Animals in the dorsal hippocampal group had extensive bilateral damage to that area (anterodorsal CA1, anterior CA3–CA4). Non-hippocampal structures which were damaged in a few subjects were the corpus callosum, fimbria and overlying cortex. Animals in the ventral hippocampal group received bilateral damage to that portion of the hippocampus (primarily CA3 and CA4). Occasional unilateral damage was sustained in the entorhinal cortex and medial geniculate nucleus.

There were no significant behavioural differences within any group as a function of lesion size or number of non-septal or non-hippocampal structures damaged.

Discussion

The most striking finding of these studies was the differential effect of pre- and post-operative approach training in rats with septal lesions. Both the latency and the photocell data provide support for the hypothesis that the loss of a response-inhibiting mechanism following septal damage in an approach–avoidance conflict situation is largely contingent upon post-operative approach training (Expt. I). These and other data (Fried, 1969, 1970, 1971; Middaugh & Lubar, 1970; Zucker, 1965) suggest that septal damage results in an increase in response strength which normally follows a reinforcement. Although there is no apparent explanation for the finding that the loss of inhibition in the septal lesioned animals attenuated during the course of the study, it is consistent with the earlier reports (Fried, 1970, 1971; McCleary, 1966). The finding that animals with septal lesions receiving no immediate post-operative approach training retained their avoidance behaviour longer and to a greater degree than control subjects (Expt. II) indicates that, as with positive reinforcement, negative stimuli are over-reacted to by septal lesioned rats under certain conditions. These results clarify somewhat the role of hyper-reactivity generally ascribed to animals with septal lesions. It appears that animals with septal damage over-react to either positive or negative stimuli, depending upon which is first encountered post-operatively, with the result that the ensuing behaviour appears to be one of response perseveration (McCleary, 1966). The septum is hypothesized to be involved in attenuating habit strength of responses and thereby facilitating the establishment of new responses. This inhibitory system in which the septum is postulated to play a role is thought to be a complex one operating at the level of organized, goal-directed responses rather than at the level of individual muscle tonicity (Gerbrandt, 1965; McCleary, 1966).

It is quite evident from the present experiments that dorsal and ventral hippocampal lesions result in behaviour that differs considerably from that following septal lesions, but it is not at all obvious what the roles are of the two hippocampal areas.

Whether surgery occurred before or after approach training did not produce differential effects in the dorsal hippocampal groups. This finding differed from the data obtained from the septal lesioned animals in this study and from rats with lesions of the entire hippocampus in a similar situation reported elsewhere (Fried, 1970). In the latter case, the rats showed a greater loss of inhibition when pre-operatively trained to approach. In both of the present experiments, the latency data of the dorsal hippocampal group apparently support Nadel's (1968) hypothesis of increased reactivity to motivational stimuli, as these experimental animals manifested long latency responses on the trials immediately after shock (as did the control animals) and returned to rapid running speeds faster than the control subjects. However, the photocell data are not in accord with this proposal, as the rats with dorsal hippocampal damage did not demonstrate any greater 'fear' than the control subjects on the trials after shock. The difference between the interpretation of the latency and photocell data can be attributed to the fact that the present studies did not permit a differentiation of the control and experimental groups with respect to the development of long latency responses, as both groups exhibited slow running speeds as soon as the experimental paradigm permitted. On the other hand, the photocell data, which indicated that both groups did proceed some distance from the start box on all trials, potentially could have demonstrated differences in degree of reactivity or 'boldness' towards the negative stimulus. The failure to do so, coupled with the fact that the dorsal hippocampal lesions resulted in an abnormally fast resolution of the approach–avoidance conflict in favour of approach, makes it apparent that although injury to that structure results in a behavioural dysfunction, the behaviour cannot be attributed to either a general loss in inhibition or a general increase in reactivity to motivational stimuli. These findings indicate the caution needed in the interpretation of latency data when it is the sole dependent variable considered in approach–avoidance tasks. Although the present work does not provide sufficient information to ascribe a definite role to the dorsal hippocampus, it does seem probable that its function is related to response strength following positive reinforcement.

It is apparent that the ventral hippocampal group differed in many respects from both the control animals and the dorsal hippocampal group, yet the behavioural dysfunction following the ventral hippocampal lesions is difficult to characterize. In neither study can the deficit be described as one resulting from a loss of response flexibility (Nadel, 1968) as the animals in the experimental group changed from approach behaviour to avoidance behaviour and vice versa as rapidly as the control subjects. Whether ventral hippocampal lesions were made before (Expt. I) or after (Expt. II) approach training did have a slight, and somewhat surprising, effect. Whereas combined dorsal and ventral hippocampal lesions made before the establishment of an approach–avoidance 'set' were less effective in producing passive avoidance deficits than the same lesions made after approach training (Fried, 1970), the reverse was found in the ventral hippocampal groups of the present studies.

The data obtained from the dorsal and ventral hippocampal groups emphasize the importance of considering the hippocampus as a heterogeneous structure whose sum of its parts does not equal, in the behavioural sense, the whole.

I thank G. Gailey and I. Rodrigues for experimental assistance and C. Stark for histological assistance. This work was supported in part by Medical Research Council of Canada grant MA-3430 and Defense Research Board of Canada, grant 9425-24.

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(*Manuscript received* 16 *June* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 421–428
Printed in Great Britain

THE RELATIONSHIP OF WORD-ASSOCIATION COMMONALITY RESPONSE SET TO COGNITIVE AND PERSONALITY VARIABLES

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Department of Psychology, University of Birmingham

A form of the Kent–Rosanoff Word-Association Test was given to a sample of male and female undergraduate students, together with tests of Ideational and Associational Fluency, Verbal Ability and of Neuroticism and Extraversion. Analysis of the data indicates little relationship between various measures of associative commonality and originality and the tests of fluency and ability. A significant correlation between Neuroticism and giving common responses to non-opposite evoking stimuli was of interest in the light of the previous paucity of results in this area. The chaotic nature of research in this field is indicated by demonstrating the way in which investigators claimed support for their hypotheses through the mislabelling of the tests they have used. The manipulation of instructions and of the social conditions of the test situation is necessary for understanding of this behaviour to advance.

Many studies have attempted to relate word-association commonality – the tendency on a word-association task to give popular responses – to various personality and cognitive factors. This research has shown little consistency in findings and while there have been attempts at integration (e.g. Cramer, 1968; Jenkins, 1960), these have not been entirely successful. Studies have generally been of a piecemeal nature, performance on word-association tasks being related to a few personality scales, and these scales have been of a varying nature. Without sufficient replication of research, attempts to integrate the results have been somewhat in the nature of re-labelling exercises, with the hope that some degree of commonality underlies the many different scales which have been used to measure personality.

The present paper reports an investigation which attempts to relate associative commonality to various measures of ability and personality in a further attempt to identify some meaningful relations of commonality to other psychological variables.

Method

Subjects

The subjects were 126 undergraduates (53 males and 73 females) in the age range 18–22 years.

Materials

The tests which the subjects completed were:

1. A 50-word version of the Kent–Rosanoff Word-Association Test (K–R WAT). The words were: table, *dark*, *sickness*, *man*, *soft*, eating, *mountain*, house, *black*, mutton, comfort, chair, whistle, *woman*, *slow*, wise, foot, spider, needle, sleep, anger, carpet, *girl*, *high*, *sour*, *hard*, eagle, stem, lamp, dream, *boy*, *light*, *health*, *swift*, blue, *long*, religion, hammer, butter, doctor, *loud*, *heavy*, tobacco, scissors, quiet, green, salt, street, king, blossom. All subjects associated to the words in the order presented above and were instructed to give the first word they could think of (Palermo & Jenkins, 1964).

2. The Unusual Uses Test (Guilford, 1967). Subjects were presented with a three-page booklet containing the Unusual Uses Test and also the Word-Association Test (Getzels & Jackson, 1962). The first page of the booklet gave general orienting instructions. On the second page the subjects were instructed to write down as many different uses as possible for a Brick and a Pencil.

3. Word-Association Test (Getzels & Jackson, 1962). The 10 words, in a constant order, were:

arm, bolt, file, grave, hard, pitch, port, mortar, fast, sack, and the subjects had to give as many different meanings for these words as possible.

Subjects completed the Unusual Uses and Word-Association Test first and then the K–R WAT. They were allowed 10 min. for the Unusual Uses and Word-Association tests, and unlimited time for the K–R WAT. This order was used to minimize pressure of timing on the K–R WAT performance, although some contamination of associations may have occurred as a result.

Data were also available on two other measures.

4. * Measures of verbal intelligence from the Morrisby General Ability Test. The verbal test comprised three parts, measuring speed of apprehension of verbal concepts, perception of relationships between verbal concepts, and word analysis; 3 min. each was allowed for parts 1 and 2, and 6 min. for part 3. Scores were available for 48 males and 65 females.

5. † Personality measures were taken from the Eysenck Personality Inventory (Form B) which gives scores on dimensions of Extraversion and Neuroticism.

Scores were available for 46 males and 69 females.

RESULTS

Several scores were taken from the K–R WAT.

1. *Measure of associative commonality* (*Co*). This was measured by the number of primary responses taken from Miller's norms‡ for English undergraduates (Rosenzweig & Miller, 1966). Maximum score was 50.

2. *Number of opposites to Opposite-Evoking Stimuli* (*OES*). In the list of 50 words there were 20 words identified by Carroll *et al.* (1962) as Opposite-Evoking Stimuli. These words are italicized in the previous list. The number of opposites given to these 20 words was scored.

3. *Number of primary responses to non-OES* ($Co(\overline{OES})$). This score was the number of primary responses (as given in the norms) to the 30 words in the list not appearing as OES.

4. *Originality*. This was scored as the number of responses given by subjects which failed to appear in the English norms.

The Unusual Uses Test was scored for the total number of uses suggested by the subjects, the totals for Brick and Pencil being summed. This score is indexed by Guilford (1967) as a measure of:

5. *Ideational Fluency*. The production of ideas under conditions of low restriction and with a low level of quality demanded.

The Word-Association Test was scored for the total number of *different* meanings, for the 10 words being summed. This score, following Guilford (1967) and Getzels & Jackson (1962), was taken as a measure of:

6. *Association Fluency*. The multiple production of related words.

7. *Verbal Ability*. A single measure was taken from the Morrisby, the sum of correct answers to the three parts of the tests. Raw scores were used.

Two scores were taken from the EPI: (8) Extraversion and (9) Neuroticism.

Means and standard deviations for males and females and the correlation matrices are presented in Table 1.

* I would like to thank Dr P. M. Levy for making these data available to me. These data were collected by Dr Levy from the same group at the same hour one week after the other tests.

† I am indebted to Mr J. A. Wankowski of the Educational Survey Unit, University of Birmingham, for making these data available to me.

‡ I am indebted to Dr K. R. Miller of the University of London for making available his unpublished norms for 400 English undergraduates.

Table 1. *Means, standard deviations and correlation matrices of word association, ability and personality measures for male and female subjects*

(Coefficients for males are above the diagonal, those for females below the diagonal.)

	1	2	3	4	5	6	7	8	9	Mean (males)	S.D.
1. Commonality†	—	0·92**	0·79**	−0·68**	−0·17	−0·29*	0·18	−0·18	−0·30*	17·08	6·44
2. Opposite-Evoking Stimuli†	0·92**	—	0·52**	−0·53**	−0·14	−0·19	0·14	−0·09	−0·20	8·89	4·76
3. Co($\overline{\text{OES}}$)†‖	0·83**	0·57**	—	−0·68**	−0·18	−0·32*	0·22	−0·20	−0·32*	8·47	3·16
4. Originality†	−0·71**	−0·59**	−0·69**	—	0·17	0·17	−0·02	−0·13	0·23	5·77	4·47
5. Ideational fluency†	−0·04	−0·06	−0·02	0·18	—	0·58**	−0·03	0·10	−0·04	19·59	6·48
6. Associational fluency†	−0·18	−0·13	−0·21	0·35**	0·29*	—	−0·09	0·27	−0·07	32·87	4·67
7. Intelligence‡	0·20	0·18	0·16	−0·20	0·18	0·06	—	−0·02	−0·17	112·15	12·08
8. Extraversion§	0·07	0·04	0·05	0·00	0·04	0·02	0·20	—	−0·13	14·33	3·80
9. Neuroticism§	0·02	0·02	0·09	0·03	−0·11	−0·20	−0·13	−0·13	—	10·80	4·07
Mean (females)	19·89	9·97	10·33	5·25	18·95	31·97	107·80	14·22	12·29	—	—
S.D.	6·60	4·68	3·07	4·23	5·06	4·90	12·05	3·58	3·95	—	—

* $P < 0·05$. ** $P < 0·01$.

† For males, $n = 53$; for females, $n = 73$. ‡ For males, $n = 48$; for females, $n = 65$. § For males, $n = 46$; for females, $n = 69$. Correlation between Intelligence and Extraversion or Neuroticism is based on $n = 43$ (males) and $n = 61$ (females). ‖ Number of primary responses on non-Opposite-Evoking Stimuli.

Comparison of means

Female subjects produced significantly more primary responses than males ($P < 0{\cdot}05$), the over-all effect being due to the emission of more primaries to non-OES words ($P < 0{\cdot}01$). No sex difference was found for the number of idiosyncratic responses.

The women tend to have lower scores on verbal ability, the difference tending towards a conventional level of significance ($0{\cdot}10 > P > 0{\cdot}05$). No sex differences were found on the measures of associational or ideational fluency.

No differences were found on the extraversion score but females did tend to be more unstable than males ($0{\cdot}10 > P > 0{\cdot}05$).

Correlation matrices

There were small reductions in the number of subjects for whom ability and personality measures were available. No differences were found between the total and reduced samples for either means or standard deviations on any variables.

The measures from the WAT were all significantly intercorrelated in both male and female samples. The correlation between OES scores and Co($\overline{\text{OES}}$) scores were, however, substantially less than the other correlations from the WAT.

Interrelationship of WAT scores, intelligence and fluency. The different measures of fluency did not seem related to verbal ability. Inspection of the scatterplots gave no indication of a wedge-shaped distribution which does occur in relationships between 'creativity' and 'intelligence' tests. It must be borne in mind that the population used here had a high mean value of ability (the mean of the present sample was between the 70th and 75th percentile for a population of university students). The correlations found here may not hold true over a wider range of intelligence.

Fluency tests were significantly correlated with each other. Associational fluency was significantly and negatively correlated with Commonality and Co($\overline{\text{OES}}$) for males, and positively with WAT Originality for females.

Verbal ability did not significantly correlate with any measure from the WAT.

Interrelationship of personality tests with other variables. The only relationship

Table 2. *Principal components analysis: male and female subjects*

(Decimal points omitted. See Table 1 for explanatory notes.)

	Males Components			Females Components		
Variable	1	2	3	1	2	3
1. Commonality	9530	1178	−0208	9594	0976	1367
2. OES	8147	1434	−0731	8522	0950	1518
3. Co($\overline{\text{OES}}$)	8636	0576	0663	8591	0213	1202
4. Originality	−7652	−2277	3622	−8497	0774	0788
5. Ideational fluency	−3402	6762	1757	−1353	6273	4598
6. Associational fluency	−4430	7347	−0024	−3421	5803	3967
7. Verbal ability	2485	0745	8130	2654	5801	−2186
8. Extraversion	−1764	5194	−3927	0767	4178	−7075
9. Neuroticism	−3594	−4886	−2414	0547	−5447	2436
Variance	3·4373	1·6007	1·0457	3·3212	1·5631	1·0387
Percentage	38·19	17·79	11·62	36·90	17·37	11·54

between scores on the EPI and any other measure was that between Neuroticism and Commonality, which appears to be mainly due to the correlation with Co($\overline{\text{OES}}$). This relationship held for males only.

Principal component analysis

A principal components analysis was performed on the correlation matrices and the results for these components with latent roots greater than unity are presented in Table 2.

The first component for both males and females is a Word Association (Commonality) one, defined by Co($\overline{\text{OES}}$) at the positive and originality at the negative poles. The WAT scores appear to have no other significant loadings on other components.

The second component for males seems to be a Fluency component with substantial loadings of Associational and Ideational Fluency. Extraversion also seems to be loaded on this component. Verbal Ability has a near zero loading. For female subjects the second component has substantial loadings of the Fluency scores together with Extraversion and Intelligence and the negative pole is defined by Neuroticism.

The third component for males appears to be defined by Intelligence. Interestingly there is a substantial loading of WAT originality also on this component. For females the third component has a high loading of Extraversion and the Fluency scores appear to have negative loadings on the component.

Apparently for male subjects Extraversion seems to be associated with Fluency, while for females Extraversion is associated with Verbal Ability.

Discussion

The result showing higher commonality scores for females than for males supports the findings of Palermo & Jenkins (1965). Failure to find differences in the number of opposite responses, however, conflicts with a result of Palermo (1963), although supporting Peak *et al.* (1960).

The reduced correlation between the OES and Co($\overline{\text{OES}}$) scores is similar to that found by Carroll *et al.* (1962), although in the latter study the correlation was not statistically significant. Van der Made-Van Bekkum & Prevo-Molenaar (1968) reported a significant correlation of magnitude slightly lower than in the present study.

The significant correlations between the fluency tests and the lack of relationship of these to the measure of verbal ability is expected on the basis of previous work, e.g. Horn & Cattell (1966) and Guilford & Christensen (1956). To the extent that Associational Fluency is a predictor of creativity, and in view of the high correlations between scores on the K–R WAT produced by different scoring methods (Jenkins & Palermo, 1964), the correlations between Associational Fluency and K–R WAT measures supports some previous work, e.g. by Mackinnon (1962).

Intelligence, in the present sample, did not relate significantly to Commonality. Previous work with high-school students found a positive relationship of intelligence to the number of non-opposite primaries (Kjeldergaard & Carroll, 1963). In their study correlations between OES and verbal ability measures were all virtually zero, whereas Co($\overline{\text{OES}}$) scores correlated significantly in 22 out of the 26 cases. A measure of reasoning ability, for example, correlated 0·21 ($P < 0{\cdot}01$) with Co($\overline{\text{OES}}$), close to

the values found in the present study. Kjeldergaard & Carroll's values were based on far higher n's (231). In the present study, however, OES correlates as highly as Co($\overline{\text{OES}}$).

The significant negative relationship between Neuroticism and Commonality is interesting in view of the general paucity of studies finding any significant relationship between personality and Commonality, e.g. Block (1960). Kjeldergaard & Carroll (1963) found a lack of any relationship between personality measures and their Commonality measures. From 72 correlations between K–R WAT scores and personality measures reported, only six were significant at $P < 0{\cdot}05$ and all of these were for female subjects. They found, for example, coefficients of $-0{\cdot}19$ and $-0{\cdot}23$ for males and females respectively between Co($\overline{\text{OES}}$) and thoughtfulness on the Guilford–Zimmerman Temperament Survey (GZTS). This latter measure may be a correlate of EPI Introversion (Carrigan, 1960). In the present study there appears to be a relationship of similar magnitude between Co($\overline{\text{OES}}$) and Extraversion ($r = 0{\cdot}20$), but it is for male subjects, and in the opposite direction. Sociability on the GZTS correlated virtually zero with both OES and Co($\overline{\text{OES}}$). In the present study Co($\overline{\text{OES}}$) was negatively correlated with Extraversion.

Dunn *et al.* (1958), in a sample of female subjects, found that low scorers on the Guilford R scale, i.e. impulsive subjects, gave more contrast responses. The R scale correlates positively with E, thus it appears that the Dunn *et al.* results are not supported by the present data, which show essentially zero correlations.

In the Dunn *et al.* study high scorers on the Guilford T (thoughtfulness) scale tended to give more contrast responses (although the difference was not statistically significant). T has a fairly high loading on Neuroticism, but in the present study, for male subjects, high Neuroticism scorers tend to give fewer contrast responses. No relationship appears to be replicated.

Cramer (1968) has suggested that response commonality will be related to the degree of interaction a subject has had with the verbal community. With frequent exposure to words a person comes to form associations which reflect the relationships between words in the environment. Factors which affect the amount of verbal intercourse a person will have include intelligence, persons of high ability being better able to give the socially expected associations when required to do so, and personality, with extraverted persons having more social contacts and more common associations. In adults the fact that most associations are over-learned, and these are likely to short-circuit any conceptual mediation processes, will mask any relationship between intelligence and commonality.

Most studies of word-association behaviour have used so-called 'speed' instructions in which subjects are told to give the first word that comes into their head. Siipola *et al.* (1955) have demonstrated that under speed compared to more relaxed instructions, there is a greater frequency of contrast responses. This suggests that if instructions are more relaxed, with the subject being encouraged to consider his response, then individual differences related to the giving of speed-type responses, i.e. the predilection to give contrast responses, should emerge. The findings of Dunn *et al.* (1958) in which relaxed instructions were used suggest that in fact a relationship between extraversion and antonym response may emerge under relaxed conditions.

Nakamura & Wright (1965) have suggested that a factor which may moderate any relationship between associative commonality and personality is that a social stimulus (an observer) may facilitate the production of common responses in inhibited, introvert(-like) subjects, and facilitate the emission of uncommon responses in extravert-type subjects.

In the present study, introverts tend to respond with common associations which are not, however, antonym responses, at least for male subjects. There may in fact be some support here for a form of the original hypothesis.

There is a large literature on the effect on an individual's behaviour of the presence of another person (Zajonc, 1965). This literature suggests that the presence of another person increases drive or arousal in a subject. This should have the effect of increasing the probability of the production of responses dominant in that subject's response hierarchy. Matlin & Zajonc (1968) have produced some supportive evidence in one of their experimental conditions, using a word-association task. Subjects with an observer present produced more common associations, with shorter latencies, than when alone.

The Nakamura & Wright (1965) study would seem to be contrary to this evidence, at least for their 'extravert' group, if 'Neurotic under-control' can be equated with Extraversion. If it is postulated that extraverts have overlearned the common associations available to them, then the presence of an audience should facilitate the production of associations and increase commonality. In fact, if introverts have non-common associations dominant in their response repertoire then the presence of an observer should reduce their commonality score even further and this effect also fails to occur in the Nakamura & Wright (1965) study.

Cramer (1968) suggests that the presence of an observer forces the subject to produce more uncommon responses by making the subject 'take the task seriously' and produce conceptually mediated responses (this effect for extraverts). Presumably introverts are encouraged to appear more common under observation than they would normally allow themselves to be.

The present study was carried out in a large group, under speed instructions, both of which are likely to produce antonym responses. It is not possible to demonstrate any interaction between intelligence, personality and commonality response style.

The data from the present study do suggest the existence of some relationships between associative commonality responses and measures of associational fluency and neuroticism. The present state of the literature relating commonality to personality and ability measures, however, does not enable one to unequivocally replicate any previous studies. There is a need for studies manipulating the nature of the instructions and the social nature of the testing situation before a picture may emerge which has some clarity and interest. Work is at present in progress to assess the role of instructions and social conditions in word-association performance.

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(*Manuscript received* 10 *June* 1971; *revised manuscript received* 18 *August* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 429–440
Printed in Great Britain

CONTINUITIES AND INDIVIDUAL DIFFERENCES IN CONSERVATION

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An analysis of present conceptions of conservation capacity suggested that they might be unduly influenced by developmental stage theory, to the extent that continuities and individual differences at one particular stage of development are insufficiently stressed. In order to achieve a finer discrimination between individual differences in conservation of continuous quantity (solid and liquid), a new test instrument was devised. This departs from traditional experimental procedures by being presented largely in pictorial form, and by progressively increasing the number of standards, variables and distributions and redistributions of material. The results indicate that ordinal scaling of conservation at one particular developmental level is possible. It is suggested that the demonstrated individual differences may reflect differences in syllogistic reasoning and information-processing capacity.

The present study, and the development of the instrument for the measurement of conservation described below, arose in the context of wishing to demonstrate finely discriminating individual differences in this cognitive capacity. With rare exceptions, individual differences in conservation tend to be expressed by gross nominal scale categories, especially when one particular developmental level of this cognitive skill is assessed. Thus subjects' responses have been commonly classified as *non-conserving*, *pre-conserving* or *transitional*, and *conserving*, with additional categories of *almost conserving* or *undecided* (e.g. Lovell & Ogilvie, 1960). Alternatively, subjects' actual verbalized reasons for a conserving or non-conserving response have been used to differentiate them (e.g. Elkind, 1961). Apart from the customary difficulties attending the interpretation of verbal responses, this approach is usually confined to a limited number of test problems (at a specified level of conservation) which restricts the demonstration of the full range of individual differences. One of the main factors in this orientation was a perhaps unnecessarily rigid adherence to Piaget's concept of stages. Stages and levels of cognitive development inferred from verbal responses may be considered suspect from the point of view of validity, and more objective indices of cognitive performance may be said to be desirable. The type of study reported by Wohlwill (1960) partly overcomes this problem by extending the investigation to a wider range of difficulty embodied in conservation of number problems. This study is of additional merit by virtue of the application of scalogram analysis to the difficulty hierarchy, thus permitting rank order statistics and more discriminating demonstrations of individual differences to be carried out. Similar approaches to the scaling of cognitive development based on the stage concept have been attempted by Nassefat (1963) and Laurendeau & Pinard (1963). In both instances the level of cognitive development is derived from performance on a test hierarchy representative of stages of hypothesized progressively higher order developmental structures as demanded by Piaget's conception of stages. While this type of approach permits the empirical validation of problem difficulty, it is by no means certain that it is the most appropriate test of the

stage theory. This continues to throw up many philosophical and physiological difficulties (Pinard & Laurendeau, 1969).

There is little doubt that, for example, conservations of weight or displaced or occupied volume are more difficult than conservation of substance or quantity. What may be questioned is the largely tacit assumption that conservation at *one particular stage of development* is an all or none skill, and that we are dealing with discontinuities in cognition. That is to say, a child is either non-conserving, transitional or conserving. Piaget illustrates his conception of an incomplete conserving operation by reference to changes in the response when a child is faced with the situation $A_1 \rightarrow C_1+C_2+C_3$ compared with $A_1 \rightarrow B_1+B_2$ (Piaget, 1952). The transitional child conserves in the latter, but not in the former situation. An 'equal' response in the first situation, however, makes the child a conserver. The approach to the differential difficulty of conservation problems at one particular level of development presented here would hold that a child's conserving response in the situation $A_1 \rightarrow C_1+C_2+C_3$ does not guarantee that he will be able to conserve in situations containing multiple, heterogeneous deformations and redistributions. Supposing $C_1 \rightarrow D_1+D_2$ and $C_3 \rightarrow D_3+D_4+D_5$, so that the comparison is between A_1 and $C_2+D_1+D_2+D_3+D_4+D_5$. This extended conservation problem is predicted to be more difficult because a purely perceptual strategy will be approximate and inefficient, because more equations have to be handled, and because the inclusion of subsidiary conserving steps increases the information processing demands on the cognitive system. In a multistage problem, the application of the principle of dimensional compensation, for example, would be relatively ineffective because of the lengthy sequence of deformations or redistributions.

To demonstrate individual differences in conservation at one level of development optimally as well as reliably, it is obviously preferable to manipulate the difficulty of the task rather than be restricted to the interpretation of subjects' responses. The manipulation of the difficulty level of individual problems should be possible to achieve by gradually increasing the number of deductive syllogistic steps incorporated in conservation problems, so that neither reversibility nor dimensional compensation can reliably lead to correct solutions because of the complexities of the perceptual displays. In the study reported here an attempt was made to devise a conservation test problem series which fulfilled these requirements, and as a result permitted a finer discrimination of individual differences in conservation than had been possible before.

METHOD

Design

There are at least three major difficulties in designing tests of conservation permitting a finer differentiation of this skill than existing techniques. First, the number of problem situations must be increased without departing from the developmental level of conservation that is being assessed. That means, for example, that the difficulty level of conservation of 'volume', which may be defined at its simplest level by the conservation of a continuous liquid quantity, cannot be manipulated by proceeding from the distribution and subdivision method to testing for conservation of internal volume, followed by displacement volume. Secondly, any modification of the Piagetian procedure must not become experimentally cumbersome, and must remain comparable if not identical with the traditional method. Thirdly, the new techniques must incorporate some scale of progressively increasing test difficulty, so that any existing individual differences can appear in diminishing problem-solving success.

The traditional method of testing for conservation of continuous solid quantity involves two equal amounts of plasticine, one of which may be called the *standard*. The second quantity is usually deformed into a shape different from that of the standard, or is divided up into several portions. This may be called the *variable*. The task for the subject is to decide whether standard and variable are equal in amount, with the prior knowledge that they had been equal before deformation or subdivision. The procedure for testing for conservation of continuous liquid quantity is identical in conception, except that *standard* and *variable* quantities of water in a variety of glass jars are manipulated.

The extensive literature on the study of conservation contains no evidence that truly standard amounts, volumes, shapes or containers have been employed in constructing problems, or that the difficulty level of the traditional conservation problems has been controlled or manipulated in any systematic way. Any study requiring to demonstrate individual differences in conservation would need to develop a series of problems in which level of difficulty is variable. Since the basic task of testing for a logical understanding of identity, equivalence or invariance of amounts compared remains unchanged (representing different levels of conservation) problem difficulty level can be varied only by increasing the complexity of the presentation or display of the problem. The method chosen here was to present in successive conservation problems increasing amounts of perceptual information on the assumption that conservation may be affected by the number of relevant and irrelevant cues that have to be processed.

Problem difficulty level can be manipulated in several ways. The original equal quantities or standards can be gradually increased from the usual two, the original plasticine shapes of standard and variable need not be homogeneous, nor is it necessary to start out in a conservation of liquid quantity problem with the same shape or same size vessels as long as it was made clear to subjects that equal amounts of water were poured into both. Since understanding this procedure itself requires the presence of conserving operations, any manipulations of the test display introducing perceptually misleading information may be considered additive from the point of view of informational complexity, and thus for the overall difficulty of the conservation problems.

For the present purpose it was thus assumed that the traditional plasticine and water jar problems could be extended along a gradient of difficulty such that the necessary operations of reversibility and equilibration would be elicited in the context of a display making increasing cognitive demands. It was postulated that this aim might be achieved by manipulating and/or gradually adding to the test problems the following parameters:

(*a*) Number of different shapes of stimulus objects (1–23).
(*b*) Homogeneity or heterogeneity of standards (0–1).
(*c*) Homogeneity or heterogeneity of variables (0–1).
(*d*) Homogeneity or heterogeneity *between* standards and variables (0–1).
(*e*) Orientation of stimulus objects (0–1).
(*f*) Shape 'complexity' of stimulus objects (0–1).
(*g*) Number of standard objects (1 standard – 1, 2–3, 3–6).
(*h*) Comparison with a 'near' or 'far' standard (0–1).
(*i*) Comparison of 'near' or 'far' distributed variables (1–2).
(*j*) Number of redistributions of a standard (0–13).
(*k*) Number of summations of variables between stages of distributions, e.g. Fig. 6 (0–5).
(*l*) Total number of distributions of a solid or liquid quantity (1–19).

These parameters are further explained below, and are illustrated in Figs. 1–6. The figures in parentheses give the range of the scores for each parameter.

In a pilot study up to 21 different shapes of plasticine and glass vessels were used, some of them irregular and unsymmetrical (in particular, some of the chemical flasks); the number of standard objects – the original undeformed or undistributed quantities – ranged from one to three; in the case of two or three standards, the comparison could be between the distributed quantities and spatially either the nearest or the furthest standard; all standards could be deformed or redistributed either wholly or partially into a varying number of parts, where the total number of redistributions could range from 0 to 19. Two series with 27 test problems each were designed using for conservation of continuous liquid quantity the required large number of glass jars, and for conservation of continuous solid quantity preparing a correspondingly extensive range of plaster moulds by means of which standard amounts of plasticine could be re-formed for distri-

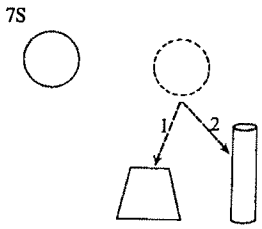


Fig. 1

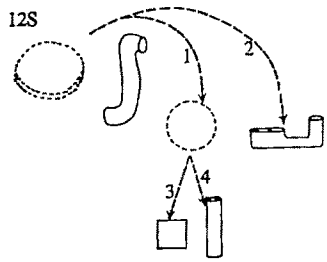


Fig. 2

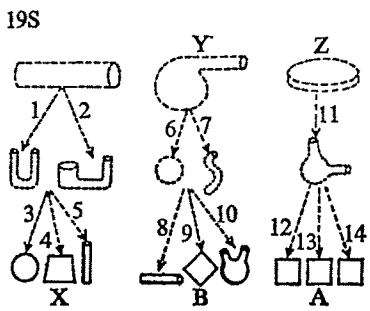
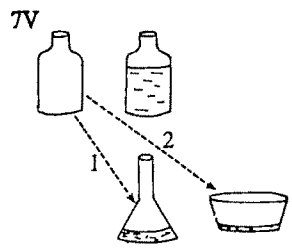


Fig. 3



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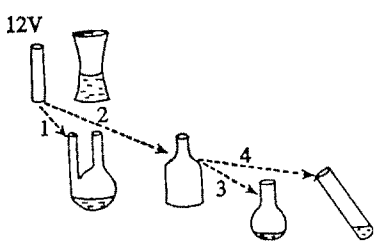

Fig. 5

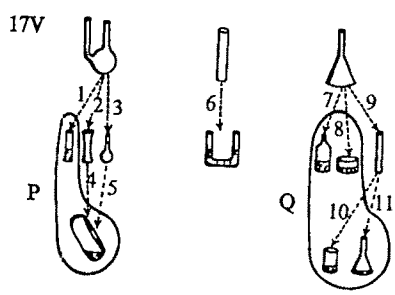

Fig. 6

Figs. 1–3. Conservation of solid quantity.

Figs. 4–6. Conservation of liquid quantity.

bution and redistribution in repeatable shapes and amounts. The test results obtained from 20 boys aged 4½ to 8½ showed, as predicted (1) a larger number of correct solutions for the older children, and (2) that the more difficult the problem (as defined), the lower the number of conserving responses in each age group.

Not unexpectedly, the traditional manipulation of concrete materials in its present extended form proved to be excessively cumbersome and time-consuming. It was decided, therefore, to shorten the problem series to 21, and to change all but the first three problems into pictorial form, i.e. standard size isometric drawings of solid shapes and glass jars were prepared showing for each problem the first and final stages of the distribution of the material. On the last card there were either the dotted outlines of already distributed solid quantities and the final distribution (viz. Figs. 1–3), or drawings of arrays of jars with the pictorial representation of the final distribution of water (Figs. 4–6). The arrows and numbers in the figures indicate the direction and the sequence of the distribution of material. Testing with each series was discontinued after subjects had given two consecutive non-conserving answers.

Figs. 1–3 are nos. 7, 12 and 19 of the conservation of continuous solid quantity series, Figs. 4–6 are nos. 7, 12 and 17 of the series testing for conservation of liquid quantity. The sequence of test

Table 1. *Manipulation of item difficulty: conservation of substance and volume*

Problem no.	'Complexity' score	Problem no.	'Complexity' score
1	3	12	19
2	4	13	21
3	5	14	23
4	6	15	28
5	7	16	30
6	8	17	40
7	9	18	43
8	10	19	46
9	11	20	62
10	12	21	75
11	13		

items was determined by calculating an additive 'complexity' score which depended on the presence or absence of the display characteristic *a–l* described above. Thus the complexity (or gross scale) score of Figs. 1 and 4 is 9, of Figs. 2 and 5 is 18, and of Figs. 3 and 6 is 46 and 40 respectively. Table 1 gives the 'complexity' scores of all test items. The greatest contribution to these scores derives from the manipulation of parameters *a*, *g*, *j* and *l*.

The additive score was increased gradually in the early problems to facilitate the differentiation of the youngest subjects who had barely acquired the conserving operations. Wider between-problem gaps became necessary at a later stage to delimit the total length of the test series. Equal interval scales, possibly for different age groups, would be the ideal to aim for in subsequent studies.

The scaling procedures, and how the manipulated display characteristics *a–l* contributed to them, require further comment. The number and differential shapes of stimuli (parameter *a*) increase from Fig. 1 to 3 and from Fig. 4 to 6. Figs. 1 and 4 show homogeneous standards, while in Figs, 2, 3, 5 and 6 the standards are heterogeneous with respect to shape (parameter *b*). The distributed substances or quantities are heterogeneous (parameter *c*), except in one of the last redistributions in Fig. 3, and in all the examples given here, comparing standards and variables, the shapes and sizes of quantities and/or their containers are heterogeneous (parameter *d*). Orientation (parameter *e*) departs from the upright in Figs. 3, 5 and 6. The complexity of individual stimuli (parameter *f*) is varied by introducing shapes and jars with more than one protusion or orifice as in Figs. 2, 3, 5 and 6. The number of standards is seen to increase from one to two in the illustrations, and was raised to three in the most difficult problems not shown here (parameter *g*). When there was more than one standard, the test question could either refer to a comparison between a distribution *X* in Fig. 3 and standard *Y*, or standard *Z* if these had been left undistributed (parameter *h*). Because of the greater spatial distance, the *X–Z* comparison was ranked as more difficult than the *X–Y* comparison. With all amounts of substance or volume distributed as in Fig. 3, the test question could be aimed at the comparison of 'near' variables, i.e. comparing *X* and *B* or *A* and *B*, or the comparison of 'far' variables *X* and *A* (parameter *i*) where the latter, because of their spatial separation, were considered more difficult. The number of redistributions of a standard (parameter *j*) is given by the number of shapes or jars containing liquid derived from redistributing the original distribution of the standards. Thus the number of redistributions is two in Figs. 2 and 5, three in Fig. 6 and nine in Fig. 3. Summations between first and subsequent distributions are necessary in Figs. 2, 5 and 6 for phrasing the test question as well as for the conserving response (parameter *k*). Thus the amounts compared are P and Q in Fig. 6, giving two summations. Finally, the total number of distributions is defined by the total number of subdivisions contained in a problem, summing all distributions and redistributions. Thus parameter *l* contributes scores of four for Figs. 2 and 5, ten for Fig. 6, and 14 for Fig. 3 to the 'complexity' score. A worked example of how the complexity score was obtained may be desirable. The score of *19* of Fig. 5 (no. 12 conservation of a continuous liquid quantity), derived from the 12 display parameters, is made up as follows: $a = 6$, $b = 1$, $c = 1$ (the variables are heterogeneous), $d = 1$, $e = 1$ (one distribution tilted), $f = 1$ (complex shape as defined), $g = 1$, $h = 0$ (not applicable because of a single standard), $i = 0$ (not applicable), $j = 2$, $k = 1$, $l = 4$.

Parallel series were constructed for the two types of conservation of continuous quantity partly for reliability testing and partly to serve the design of an associated study (Hamilton, 1971). The reliability coefficients were 0·80 (solid) and 0·84 (liquid). A long sequence of similar problems may elicit perseveration and/or response set. To counteract and minimize these biasing factors 'dummy' and 'misleading' problems were placed at intervals throughout all series. Dummy items were problems testing for class inclusion and exclusion. Misleading items were conservation problems in which the variables had not been fully distributed, yet the test question, as in other problems, asked for a comparison between an undistributed standard and a distributed variable. Awareness of the 'catch' in the problem therefore required the presence of the conserving operation and awareness of the non-conserving determinant in the test item, and thus the maintenance of alertness and the need to proceed from first principle with each problem. It could be predicted that the better established the conserving operation, the more frequently subjects would spot the 'catch' in the problem. This prediction was confirmed (rho = 0·6 to 0·9 for the various conservation indices).

Two scores were obtained from each subject for each conservation series: a raw score – the number of correct conserving responses (a score of 1 for a conserving response), and a *weighted score* – determined by the logical level of subjects' verbal justification in support of the conserving response. Although ratings of verbal responses in conservation tasks have always presented methodological and grading problems, in the present case they would permit a further gross test of the scaling method employed, since the more difficult the conservation problem, the greater the number of suboptimal verbalized reasons subjects are likely to give.

Subjects' answers were therefore recorded *verbatim* to permit the weighing of correct responses according to the logical power of the reasons given in support of an equality response. To achieve a reliable as well as relatively valid classification of the verbal responses, all answers given by each age group to all problems were typed on cards and a *Q* sort was performed yielding seven weighting categories defined by the following criteria:

1. 'Equal because they look the same'.
2. Description of the procedure.
3. 'They can be put together to make the same'.
4. 'Equal because they started the same and ended the same'.
5. 'Equal because you never added or took any away'.
6. 'Equal because you only changed the shape'.
7. Equality answers containing statements illustrating the logical inevitability of invariance, often syllogistically.

(Both sets of scores, raw and weighted, were in addition angularly transformed, but the present report is largely limited to considerations of only the first two indices of performance.)

Procedure

Instructions

The test instructions to subjects stressed the need to attend carefully to what the experimenter was doing or saying. Since the first three problems in each series were presented concretely, and since each successive problem involved the introduction of additional parameters, the commentary was standard only in a general sense, with the experimenter describing verbally the distribution and redistribution of quantities shown graphically on the test cards. If subjects asked questions, the instructions and commentary were repeated. The concrete items gave an opportunity to check that they generally understood the meaning of the terms '*more*', '*less*' and '*equal*', because they were invited to help in the task of equating the standard quantities of water. For the plasticine problems, the total contents of two containers were extracted in front of them, and they were informed that the shop had sold equal amounts in each container. No subject availed himself of the opportunity to carry out corrections on the plasticine balls by adding to one or taking away from the other. Responses, particularly from the younger children, frequently referred in concrete terms to the original equality of the standards. At the end of the manipulation or the description of the manipulation of materials, subjects were asked: 'Does this one have *more* (*less*) than this one, or is there *as much* here as there?' The order of the critical phrases and the quantities pointed to first was counterbalanced. Subjects' responses were followed by the question: 'Why are they the same (equal)?' or 'Why is there more (less) here than there?'

Table 2. *Basic characteristics of experimental groups*

School grade	*n*	Mean age	Mean socio-economic group†	Wechsler IQs: Verbal	Wechsler IQs: Performance	Wechsler IQs: F Scale	Educational attainment‡ (percentiles): Arithmetic	Educational attainment‡ (percentiles): Reading	Mean digit span (weighted scores)
KG	15 (7)*	5–11	2·55	101·5	119·7	111·2	62·4	43·6	10·1
1	26 (10)	6–8	2·75	102·8	109·6	106·6	43·8	47·8	10·4
2	22 (11)	7–9	2·70	108·9	108·7	109·6	39·7	59·8	10·7
3	22 (9)	9–0	2·63	111·0	113·8	113·6	45·1	70·1	10·3

* The numbers in parentheses refer to the number of girls in each group.
† Registrar-General's five-point classification.
‡ Wide Range Achievement Test. Jastak & Jastak (1965), Delaware.

Table 3. *Individual differences in conservation of quantity*

Group...	Kindergarten		Grade 1		Grade 2		Grade 3	
Scores...	Raw	Weighted	Raw	Weighted	Raw	Weighted	Raw	Weighted
Solid								
Mean	2·9	—	6·4	25·6	11·2	45·1	17·0	83·4
S.D.	7·4	—	8·3	32·8	8·2	34·8	5·5	32·3
Range	0–21	—	0–21	0–95	0–21	0–100	3–21	21–131
Liquid								
Mean	2·8	—	5·6	20·5	13·5	57·9	15·5	73·1
S.D.	7·0	—	7·6	30·3	8·3	38·5	7·2	41·0
Range	0–20	—	0–21	0–100	0–21	0–128	3–21	10–127
n	15		26		22		22	

Maximum scores: raw, 21; weighted, 147.

Subjects

These were members of Kindergarten and Grades 1–3 classes in two North Toronto schools who were selected for study because they fulfilled the requirements of three different types of maternal attitudes towards their children. Details of the larger study are reported elsewhere (Hamilton, 1970, 1971). Table 2 gives the social and intellectual data of the four age groups. No attempt was made to achieve between-age group identity on the parameters shown in Table 2, because comparability between subgroups within each age had priority for the associated study. There were, furthermore, no grounds for predicting that the individual differences tested for here in terms of gradients of success or failure in problem-solving would be affected by relatively small age group differences in educational or intellectual status. The only really relevant prediction would be concerned with the upward displacement of the curve of successful problem-solving with increasing age.

Hypotheses

Two predictions were tested: (1) that conservations of solid and liquid quantity are best described as continuous dimensions of cognitive ability which depend on the individual's capacity to handle progressively more complex information in displays of increasing complexity; (2) that there is a significant positive relationship between age and solving conservation problems of increasing order of difficulty, as defined.

Results

A summary of the main distribution parameters is given in Table 3. Only two subjects of the youngest age group gave correct answers to more than one problem, and ten subjects gave not one correct conserving response. This group is omitted, therefore, from subsequent data analyses.

Table 4. *Pass rates for individual problems of conservation of solid quantity for three school grades*

Problem no....	1	2	3	4	5	6	7	8	9	10	11
Raw scores											
Grade 1	46·2	23·1	57·7	42·3	42·3	34·6	38·5	26·9	30·8	30·8	30·8
Grade 2	63·6	50·0	77·3	63·6	72·7	63·6	54·5	68·2	59·1	68·2	54·5
Grade 3	95·5	90·9	100·0	90·9	95·5	95·5	90·9	90·9	90·9	90·9	90·9
Weighted scores											
Grade 1	24·7	16·5	45·6	30·8	24·2	23·1	24·7	15·4	19·2	15·9	14·3
Grade 2	42·2	33·1	56·5	46·1	50·6	37·7	30·5	36·4	27·3	33·8	32·5
Grade 3	68·8	69·5	75·3	64·3	68·2	70·1	64·9	62·3	62·3	65·6	57·8

Problem no....	12	13	14	15	16	17	18	19	20	21
Raw scores										
Grade 1	23·1	26·9	26·9	19·2	23·1	23·1	23·1	23·1	23·1	19·2
Grade 2	50·0	45·5	45·5	40·9	45·5	40·9	40·9	40·9	36·4	36·4
Grade 3	68·2	77·3	81·8	77·3	63·6	63·6	63·6	68·2	59·1	50·0
Weighted scores										
Grade 1	14·3	15·4	11·0	8·8	9·9	11·0	9·9	12·1	12·1	12·6
Grade 2	22·7	24·0	25·3	23·4	23·4	18·8	22·1	22·7	15·6	19·5
Grade 3	48·1	50·0	53·2	46·1	37·7	42·2	43·5	44·8	42·2	29·9

Table 5. *Pass rates for individual problems of conservation of liquid quantity for three school grades*

Problem no....	1	2	3	4	5	6	7	8	9	10	11
Raw scores											
Grade 1	65·4	23·1	38·5	34·6	38·5	38·5	34·6	30·8	30·8	26·9	26·9
Grade 2	72·7	63·6	77·3	77·3	77·3	72·7	72·7	72·7	68·2	72·7	68·2
Grade 3	95·5	86·4	100·0	81·8	90·9	86·4	81·8	81·8	81·8	77·3	72·7
Weighted scores											
Grade 1	32·4	13·7	20·3	19·8	20·9	18·7	21·4	17·0	17·6	13·7	13·7
Grade 2	42·2	35·7	42·2	46·1	56·5	46·8	44·8	40·9	45·5	48·1	44·8
Grade 3	60·4	57·8	64·9	59·1	57·1	55·8	53·9	50·6	54·5	46·8	44·8

Problem no....	12	13	14	15	16	17	18	19	20	21
Raw scores										
Grade 1	19·2	19·2	26·9	15·4	15·4	15·4	15·4	15·4	15·4	15·4
Grade 2	63·6	68·2	59·1	63·6	63·6	59·1	54·5	59·1	59·1	50·0
Grade 3	72·7	68·2	68·2	63·6	63·6	63·6	63·6	63·6	63·6	59·1
Weighted scores										
Grade 1	13·2	11·0	16·5	7·7	5·5	5·5	7·1	5·5	5·5	6·6
Grade 2	43·5	38·3	32·5	34·4	31·8	32·5	32·5	36·4	27·9	24·7
Grade 3	44·8	44·8	47·4	42·9	43·5	44·8	45·5	42·2	40·3	39·0

As shown by the standard deviations, considerable individual differences in pass rates and in the quality of the conserving responses were obtained. These individual differences are shown in Tables 4 and 5 for the two conservation tasks for all 21 problems. The raw score values are the *percentage* of subjects giving a conserving response to each item of the series. The weighted scores in the tables give the ratio of the sum of all weighted answers for individual problems and the maximum weighted answer possible ($21 \times 7 \times$ no. of subjects in group), expressed as a *percentage*. The relationship between performance and problem difficulty, and between these and age, is illustrated in Fig. 7 for the raw scores of conservation of solid and liquid quantity. (The graphs for the weighted scores are similar, but steeper.)

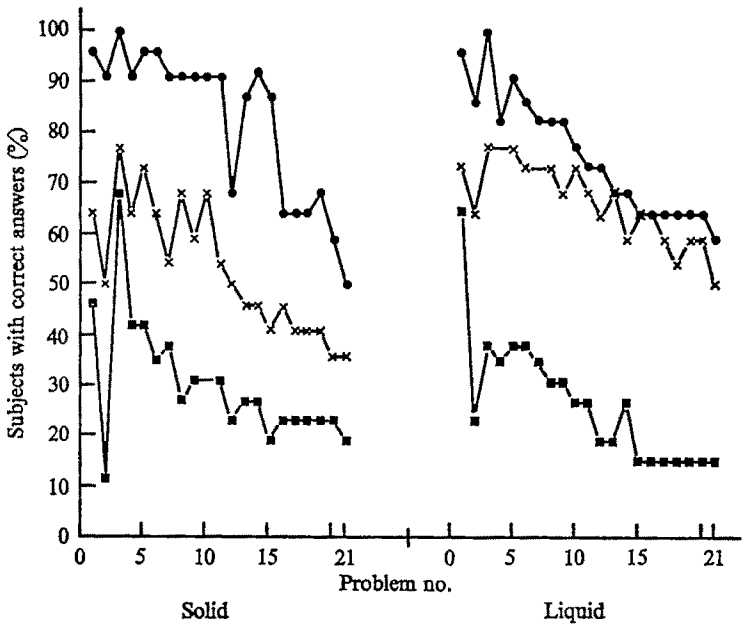

Fig. 7. Distribution of scores on conservation of continuous quantity. ■—■, Grade 1; ×—×, Grade 2; ●—●, Grade 3.

The correlations between assumed order of difficulty and the conserving skill ranged from rho = −0·694 to −0·936, while the correlations between age and performance on the two conservation tests were, respectively, 0·446 and 0·441 (d.f. = 68; $P < 0{\cdot}01$).

Discussion

The results give considerable support to the hypotheses. There are no abrupt discontinuities in the ability to conserve solids or liquids for the groups tested, and correct responses to the problems assumed to be easier were no guarantee that the later problems, considered to be more difficult, would be solved. Similarly, for many individual subjects there was no sudden break in conserving responses as the problems increased in difficulty. As in many intelligence test protocols, there was often a recurring pattern of single failures and subsequent successes before the criterion of two consecutive failures was reached. For two of the three groups conservation of solid quantity was somewhat better established than conservation of liquid (viz. Table 3) suggesting that the latter task may be more difficult, and the possibility that at least partial conservation of internal volume may facilitate the conservation of continuous liquid quantity. The results also confirm the prediction of a regression of age on difficulty of conservation problems.

While the method of testing for conservation used here constitutes a marked departure from the traditional method, it is unlikely that the cognitive operation elicited by the problems is not in fact that of conservation. In each problem originally physically equal amounts are re-formed, deformed, distributed and/or redistributed. The ability to conclude that the manipulated and the unmanipulated substances are physically the same depends on the ability to conserve the original situation despite

the perceptual cues of figurative dissimilarity. This situation is not altered by changing from concrete test displays to the representational setting of drawings presented with a running commentary on what the drawings signify. It is perhaps worth remembering that very similar decelerating curves were obtained in the pilot study with the traditional, concrete method of manipulation. Nevertheless, there is a possibility that the new method makes greater demands on verbal storage and retrieval, especially with the long commentary that is necessary for the problems shown in Figs. 3 and 6. If short-term memory were an important mediator for correctly solving these and the easier problems, we would expect at least a significant if not a substantial correlation with a memory test. The correlations between conservation scores and the Digit Span test are non-significant, however, for all comparisons. Since school children are introduced at an early stage to representational and verbal symbolism, it is in any case somewhat implausible to suggest that there may be very different rates of development for reading and diagrammatic arithmetic compared with understanding the sequence of manipulations of substances represented in the test used here. Quite the contrary seems to be the case, to judge from the correlations in this study between the conservation scores and reading (rho = 0·267; $P < 0{\cdot}05$) and arithmetical ability (rho = 0·324; $P < 0{\cdot}01$). The ultimate test, however, of a direct comparison of the traditional and the present method of testing for conservation *on the same subjects* remains to be done.

To account for the individual differences in conservation at *one particular developmental level of the operation* presented here, we may have to turn to two additional explanatory principles to support the role played in it by what is termed equilibration by Piagetians. In the first instance – and possibly of lesser importance – there is the difference between conserving responses occupying different levels in a hierarchy of logical power. These different levels are likely to be occupied by responses which, respectively, indicate the operation of conservation through the *perception of identity*; through the *perception of compensating relationships* in the manipulation of the materials, and the *indication as well as the deduction of invariance or equivalence*. Other investigators have also referred to the usefulness of such an operational and response hierarchy (e.g. Wallach & Kogan, 1965; Elkind, 1967). In the present study, these different logical approaches were taken into account when deciding on the rating criteria applied to subjects' verbalized conserving responses. If it is assumed that responses based on invariance operations represent the highest operational level, two predictions follow: (1) that subjects employing it with the simpler problems will be more successful with the more difficult problems than subjects using the lower logical level strategy; (2) the weighted score index used here should show a steeper regression of conserving ability on problem difficulty than the raw score index. The individual test protocols lend some support to the first prediction even for some of the 5- and 6-year-olds. In support of the second prediction, the correlation between problem-solving ability and problem difficulty is greater, using the weighted score index, compared with raw scores, in four out of six possible comparisons (three school grades, two types of conservation). These findings suggest that the number of subsidiary and intermediate conserving steps built into the progressively more difficult problems make the operations of invariance type of solutions progressively more difficult. Since invariance solutions in the difficult problems essentially require the

integration of successive syllogistic statements or equations, the drop in performance at this level may be due to the increasing information-processing requirements of the problem series.

This role of the information-processing requirements of successive problems is the second principle that may need to be put forward to account for the demonstrated progressive reduction of the conserving capacity with test problems of increasing order of difficulty. The more complex the problem display as defined by the greater number of intermediate conserving operations, the more inadequate will be these operations if they are at the level of perceived identities or spatial compensations. The invariance or equivalence principle will also become more difficult to apply successfully if it is assumed that there is an increase in the number of additive syllogistic steps. Furthermore, the more complex problems contain a larger number of relevant as well as irrelevant equations. The cognitive processes required for correct solutions will have to extract the relevant information, and reject the irrelevant. The differences in information load between simple and complex problems could be assessed from Figs. 1–6 by applying the rules for combinations and permutations. If information-processing capacity may determine performance in conservation situations, conservation at any developmental level is as likely to be susceptible to emotional stressors as other cognitive processes have been shown to be. Some evidence that this is a plausible inference has been produced recently (Hamilton, 1971, 1972).

Most of the irregularities in the performance curves are probably the result of two factors: (1) the absence of equal between-item intervals of difficulty, and (2) the operation – despite precautions – of a response set that would be particularly marked if adjacent items are not substantially different. The present series of pilot and main studies has indicated that it will not be possible to reduce setting factors to zero (they must operate in any experiment involving progressions of difficulty), but that they may be reduced by lifting the test ceiling, by reducing the length of the problem series and by lengthening the between-item difficulty steps.

In view of the results presented here, it is concluded that individual differences in conservation of continuous quantity are not fully described by the limited standard procedures. Problems aimed at one specific level of cognitive capacity may contain so many variations on the basic theme of dimensional change that neither multiplication of relations nor reversibility operations may be adequate for the task. We seem to be dealing with a scalable skill dependent on the progressive development of the capacity for handling and processing amounts of information which is required for a 'necessary' deduction of invariance.

The study was supported by Canada Council Grant no. 67-0285 and a supplementary grant from the British Social Science Research Council. Mrs Soya Ward was the Research Assistant, and Mrs Marjorie Whitney carried out the pilot study and contributed on points of methodology. I am indebted to them, the subjects and the Richmond Hill School Board, Ontario.

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(*Manuscript received* 28 *June* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 441–443
Printed in Great Britain

ART PREFERENCE AND PERSONALITY

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The neuroticism factor of personality was compared with the romantic/classical dimension of art. Twenty-five male and 25 female college students between 19 and 24 years of age participated. Subjects chose their preferred selection of each of 11 pairs of paintings, which had been rated by art specialists on a romanticism scale, and completed the Maudsley Personality Inventory as an index of neuroticism. Subjects choosing Romantic art tended to score higher in neuroticism than did those choosing classical paintings. A negative correlation between extraversion and choice of romantic pamtings was also found.

The basis of all aesthetic experience is perception; the apprehension of perceptual materials as they are concerned with structural and qualitative relations. William James (Lee, 1938) held that rationality rests on a feeling of satisfaction in the apprehension of the data of experience. Eysenck (1940, 1941) investigated the nature of aesthetic judgement in search of a universal value (his T factor). In isolating the T factor, Eysenck also obtained two bipolars relating to the type of art appreciated: (1) the complex *v.* the simple in content, and (2) the highly emotional *v.* the more restrained. The work of Elsie Payne (1967), based on Eysenck's second bipolar, and several of the 12 factors reported from the inquiry of Cattell & Saunders (1954), suggested that personality is an important basic factor in the motivation of taste in music preference. Payne's study was limited to the area of classical *v.* romantic taste and the neuroticism factor of personality, showing a positive correlation between stability and classical taste, and between neuroticism and romantic taste in music.

Considering that 'feeling' is the object of art, the 'creative enjoyment' is more closely associated with a pure form, and the 'specific emotional subject-matter' is associated with art somewhat devoid of pure form. An aesthetic experience, though a compound of elements, is usually dominated by a single form as is a musical composition by a Leitmotiv, or a picture by some compositional theme. To approach the ultimate form, this compositional theme must be so exactly structured as to satisfy an immediate comprehension with minimum ambiguity.

By her definitions of form and feeling, Payne sought to extend her terms 'classical' and 'romantic', beyond the 18th and 19th centuries, to the music of any period. It was proposed in the present study to extend this dichotomy, not only through the ages of artistic expression, but across the media of expression into the visual arts of painting and again correlate these concepts with the neuroticism factor of personality on a plane comparable to that of Payne's correlations.

Method

Subjects

Twenty-five male and 25 female undergraduates from various majors at Bellarmine College participated in the experiment.

Table 1. *Eleven pairs of classical and romantic paintings*

Classical	Romantic
1. *Voyage of Life and Youth* Thomas Cole	*Stonehenge* John Constable
2. *Portrait of Charles VII* Jean Foquet	*Portrait of an Officer* Franz Hals
3. *On a Lee Shore*, 1900 Homer Winslow	*The Coming Storm*, 1901 Homer Winslow
4. *A View on a High Road* Meindert Hobbema	*The Swing* Jean Fragonard
5. *Mount St. Victorie* Paul Cézanne	*Landscape with Cypress* Vincent van Gogh
6. *The Poor House on the Hill* Paul Cézanne	*Rock Formation* Paul Cézanne
7. *Portrait of a Young Man* Angilo Bronzino	*The Watchmaker* Paul Cézanne
8. *The Vale of Dedham* John Constable	*Poppyfield* Vincent van Gogh
9. *Magenta and Blue* Hans Hofman	*Listen to Living* Matta
10. *An Old Lady with a Book* Hermanzoon Rembrandt	*Madame Henroit* Auguste Renoir
11. *Maja Clad* Lucientes Goya	*Jose de Francisco* Lucientes Goya

Materials

Twenty-two paintings were chosen in terms of relative classicism (C) and romanticism (R) and assimilated in 11 pairs, controlling for content of subject-matter within each pairing. The 11 groups spanned a wide range of subjects, periods of painting, and artists (see Table 1). Selection and assessment of the paintings were made by three professors of art at the Louisville School of Art, according to the accepted definitions of romanticism and classicism (*Webster's New International Dictionary*, 2nd edition):

'*Romanticism*. Of or pertaining to the style of the Christian and popular literature and art of the Middle Ages. It is characterized by freedom of fancy in conception and treatment.

'*Classicism*. The principles and characteristics of a literature or art established as a formal standard, originally those of Greek and Roman literature and art, embodying lucidity, simplicity, dignity, and correctness of style.'

In keeping with Payne (1967), and due to a pilot study which found the Bendig adaptation of Eysenck's test to differ insignificantly from Eysenck's own test, the Maudsley Personality Inventory (Eysenck, 1959) was used as a test of neuroticism.

Procedure

Paintings were presented on colour transparencies. Subjects were instructed to 'indicate which picture you are most sympathetic with by circling "left" or "right" on the answer sheet'. Subjects worked on the personality inventory during this time.

RESULTS AND DISCUSSION

Overall, significant correlations were found associating romanticism and neuroticism scores, thus supporting the hypothesis.

More specifically, by using the analysis of variance, it was found that choosers of Romantic pictures made significantly more neurotic responses ($F = 12{\cdot}16$; d.f. $= 1,46$; $P < 0{\cdot}1$) than did choosers of classical pictures (see Table 2). The sex and interaction factors did not reach significance. Chi squares, run on the neuroticism

Table 2. *Comparison of neuroticism scores and romantic art preference*

	Mean neuroticism score	*n*
Classical choice		
Male	22·60	5
Female	22·67	9
Total	22·64	14
Romantic choice		
Male	24·95	20
Female	26·75	16
Total	22·75	36

Table 3. *Chi square scores of romantic choices*

N = neurotic high scorer.
S = stable (neurotic low scorer).

	χ^2	D.F.	*P*
Male, Female	4·58	1	0·05
Male N, Male S	63·87	1	0·001
Female N, Female S	54·56	1	0·001
Neurotic, Stable	118·25	1	0·001

scale, found that males chose more romantic pictures than did females ($\chi^2 = 4{\cdot}58$; d.f. = 1; $P < 0{\cdot}5$); male neurotics chose more romantic pictures than did male stables ($\chi^2 = 63{\cdot}87$; d.f. = 1; $P < 0{\cdot}001$); female neurotics chose more romantic pictures than did female stables ($\chi^2 = 54{\cdot}55$; d.f. = 1; $P < 0{\cdot}001$). It was also found that, overall, neurotics chose more romantic pictures than did stables (see Table 3). The chi squares run on the extraversion scale supported the above sex difference finding.

Using the Pearson *r*, a negative correlation ($r = -0{\cdot}284$; d.f. = 48; $P < 0{\cdot}05$) was found to exist between romantic pictures chosen and extraversion. A negative correlation was also found between the females' choices of romantic pictures and their scores on the extraversion scale ($r = -0{\cdot}435$; d.f. = 23; $P < 0{\cdot}05$). While the direction for males was the same, significance was not achieved. No correlations between romanticism and neuroticism proved significant.

In view of the new findings, and those of Payne (1967), it can be concluded that the neuroticism factor of personality is positively related to the romantic dimension of art and music. Further work needs to be done on why this relation exists. Perhaps the definitions of classicism and romanticism hint at some characteristics of stables and neurotics.

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(*Manuscript received* 30 *June* 1971)

Br. J. Psychol. (1972), 63, 3, *pp.* 445–450
Printed in Great Britain

PERSONALITY AND THE SUCCESS OF CARD-PUNCH OPERATORS IN TRAINING

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A short battery of tests consisting of the Eysenck Personality Inventory, a test of clerical aptitude and a coding test was given to a group of 100 young female card-punch operators during their first day in training. Test results were related to supervisors' ratings of output made at the end of each month of a three-month training period. There were significant negative correlations between extraversion and output ratings during the first two stages of training ($r = -0{\cdot}29$, $P < 0{\cdot}01$; $r = -0{\cdot}33$, $P < 0{\cdot}01$), but the relationship at the third stage fell short of significance. Neuroticism and coding scores were not related to training performance, but clerical aptitude became significant during the last month.

The report presents information on the relationship between the personality dimensions of Introversion–Extraversion and Neuroticism as measured by the Eysenck Personality Inventory (EPI) and the success achieved by young female card-punch operators in training. Klemmer & Lockhead (1962), in their analysis of productivity and error rates among IBM card-punch and bank proof machine operators, found a large gap between the performance of the best and poorest operators. Among experienced workers, the fastest tended to produce about twice as much as the slowest. They chose not to speculate on the factors involved in this discrepancy, but their results show that a need exists for further attempts to identify the range of factors involved in success or failure in these jobs. Although there exists a considerable literature on the aptitudes needed for success in clerical work, the number of studies utilizing quantitative personality measurements are limited. However, in the EPI Manual, Eysenck & Eysenck (1964*a*) present normative data on the personality characteristics of a mixed group of clerical workers which show them to be somewhat less extraverted (more introverted) than the average, but average with respect to neuroticism. This finding also has a certain amount of face validity in that one might expect clerical workers to be generally less socially driven and less impulsive than other work groups.

Theoretically, one could predict that on many clerical tests, particularly those of a routine nature, extraverts would do less well than introverts because of their tendency to build up relatively quickly higher levels of reaction and conditioned inhibition than introverts. Several laboratory studies have tended to support this prediction as far as simple motor tasks are concerned (Eysenck, 1959). In addition, Lynn (1960), using an inverted alphabet printing task, found that extraverts showed greater reminiscence than introverts, and gave some suggestion of greater work decrement under conditions of massed practice.

Tapping tasks also provide a convenient form of behaviour for studying the interaction of personality variables and simple motor skills. Jensen (1966) used Morse telegraph keys weighted by different amounts in studying reactive inhibition. His

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results were consistent with the view that the build up of reactive inhibition should be greater under massed than under distributed practice, greater under fast than under slow rates of tapping and least when tapping was self-paced. In the occupational field, card-punch operating may well be regarded as sufficiently related to tapping for a similar conceptual framework to apply. Work activity is sustained for periods long enough for inhibition effects to be expected to exert an influence on performance and, if Eysenck (1959) is right, the performance of extraverts will suffer most. Self-regulated punching will, of course, go on within the working period, and this will allow some reactive inhibition to dissipate. Overall output level should, however, be affected more in the case of the extraverts than introverts and show itself in reduced speed, if not in reduced accuracy, of performance.

Cooper & Payne (1967) have carried out a study among female packers in a tobacco factory which showed that extraversion, as measured by the EPI, was linked with several measures of job success. The more extraverted subjects had worse 'job adjustment' as rated by supervisors, shorter periods of service and a higher rate of absenteeism. A similar finding with respect to extraversion and absenteeism was reported by Taylor (1966).

The aim of the present study is to extend investigations into the field of training to see if similar relationships emerge. In addition to the EPI, scores on a clerical aptitude test and a visuo-motor coordination test were obtained and examined in relation to supervisors' ratings of a group of trainee card-punch operators. This information is considered as important for selection and may influence turnover and failure rates.

METHOD

Assessments

The measures used were the EPI (Eysenck & Eysenck, 1964*a*), tests of name and number checking taken from the Minnesota Clerical Test (Andrew & Paterson, 1959), and a coding test adapted from the Digit Substitution subtests of the Wechsler Adult Intelligence Scale (Wechsler, 1955).

The EPI is a self-administered questionnaire and consists of two parallel forms, A and B. Here, both forms, comprising 114 items, were used and total scores for both scales are reported. The test gives measures of Introversion–Extraversion (E scale), Neuroticism (N scale), and includes a measure of the subject's tendency to give socially desirable responses, called the Lie scale (L scale).

The names and number comparison tests were 5-min. timed versions of the Minnesota Clerical test, with a 3-min. rest-pause between them. A composite score (names plus numbers) was also obtained.

The coding test was used as a measure of visuo-motor coordination which also shows a moderately high correlation with general intelligence. The WAIS coding subtest was given by group administration.

Finally, the *criterion measures* of training success were made by supervisors at the end of each month of the three-month training period. The supervisors were asked to rate the efficiency of the trainee with respect to the speed and accuracy of her punching. Each set of ratings was made by the supervisors independently of any information previously obtained for the purpose of the study, such as initial test scores or earlier rating information. One exception to this was when termination of employment arose. Supervisors were then given access to previous ratings, but not to test scores. The operators were rated on a five-point scale, five being the best rating category, at the end of each month. Rating 1 was carried out by a single supervisor responsible for training all the girls to operate a conventional typewriter keyboard. The basis of judgement at this point was relatively objective, as speed and accuracy tests were given throughout the

Table 1. *Personality and the success of card-punch operators in training: means, standard deviations and ranges for all variables*

Variable	Mean	S.D.	Range
Age	15·75	0·97	15–19
E scale	28·97	6·25	8–44
N scale	25·65	8·62	8–43
L scale	5·57	2·71	1–13
Coding	55·15	10·69	25–86
Names	41·20	10·51	16–66
Numbers	41·70	9·65	14–61
Total names and numbers	82·50	19·60	30–125
Rating 1	3·38	1·20	1–5
Rating 2	2·74	0·92	1–4
Rating 3	2·74	1·02	1–4

month. In the case of ratings 2 and 3, however, a uniform criterion of output was not available as the operators had split up to work on different types of machines. Their ratings were made by two different but highly experienced supervisors who shared the training responsibility.

Subjects

The subjects were the total group of trainee card-punch operators selected by officers of the Ministry of Health and Social Security for the clerical branch of the British Civil Service during 1966. They were recruited in groups of about 25 over a four-month period and were tested in groups during their first day in training. The selection requirements for entry were not stringent and no previous testing had taken place. The sample was not, therefore, likely to be biased as to intelligence and/or personality adjustment. A total of 100 girls was tested with an average age of 15·8 years, S.D. = 0·98 years.

Before testing began, the girls were reassured that the tests were not part of the selection process and that the people carrying them out were not Civil Service employees, but researchers from the local university. They were told that individual results would not be made known to their employers, although they would, in the course of events, be told how the group as a whole performed. On that basis, all the subjects were cooperative in the group testing.

Results

The means and standard deviations for each of the measures employed are shown in Table 1. Table 2 presents the product-moment correlations between the psychological test measures and supervisors' ratings of output at each of the three stages of training. The hypothesis of a significant relationship between the EPI E scale and performance in training is confirmed at the 1 per cent level for the first two stages of training, but falls below an acceptable level at the third stage. Conversely, the measures of clerical aptitude do not show an association with success until the final month.

Neither the EPI N scale nor the coding test are correlated with the supervisor ratings. There is a correlation between the L scale and rating 2, which is significant at the 5 per cent level. This correlation does not yield to easy interpretation and, in the absence of additional evidence or theoretical position, may be regarded as a chance phenomenon.

An incidental, but interesting finding is that extraversion is not related to scores on the clerical or visuo-motor tests. This suggests that during these tests, at least within the time intervals given here, reactive inhibition has not built up sufficiently to handicap extraverts on these measures.

Table 2. *Personality and the success of card-punch operators in training: product-moment correlations between all main variables*

	E scale	N scale	L scale	Coding	Names	Numbers	Total names and numbers	Rating 1	Rating 2	Rating 3
E scale	—	+0·09	−0·30**	+0·02	+0·14	+0·11	+0·15	−0·29**	−0·34**	−0·14
N scale		—	−0·29**	−0·06	+0·14	+0·08	+0·10	−0·13	−0·06	−0·10
L scale			—	+0·16	−0·02	−0·01	−0·02	+0·15	+0·25**	+0·10
Coding				—	+0·23*	+0·17	+0·19*	−0·02	+0·10	+0·12
Names					—	+0·59**	+0·78**	+0·12	+0·08	+0·25**
Total names and numbers						—	+0·93**	+0·02	+0·13	+0·27**
Rating 1							—	+0·05	+0·06	+0·27**
Rating 2								—	+0·50**	+0·45**
Rating 3									—	+0·68**

* $P < 0·05$. ** $P < 0·01$.

Discussion

The means and standard deviations for the measures used are consistent with the previous reports of Andrew & Paterson (1959), Cooper & Payne (1967), Eysenck & Eysenck (1964*a*) and Klemmer & Lockhead (1962). The present results complement the findings of Cooper & Payne that extraversion is an important personality dimension in the performance of simple, repetitive skills, by identifying its influence during training. In the card-punch operating training described here, it seems that extraversion is more important in job success in the early stages of training and clerical aptitude less so, with the relative importance of these attributes being reversed at a later stage. This may well relate to the differential build-up of inhibition in learning between extraverts and introverts. The period of observation is, of course, very short and these trends in the data might not necessarily continue outside the period of training. The role of extraversion, however, may follow a fluctuating course and also relate to other job factors such as absenteeism and turnover at later stages. Within the training period, 12 out of the 100 trainees had their employment terminated because of poor work: their average E score is 32·00 compared to the average E of 28·56 for the 'survivors'. The t value does not reach the 5 per cent significance level, but is suggestive of a tendency for the extraverts to drop out from the job.

Eysenck & Eysenck (1964*b*) have pointed out that E and N scores are contaminated by social desirability bias, when obtained for personnel selection purposes. In spite of the fact that our subjects were given reassurances before testing that the results were 'off the record', distortion towards less neuroticism and less extraversion occurred. The tendency to play down extraverted characteristics is interesting and possibly resulted from a job stereotype that the girls shared. A liking for travel and social stimulation and excitement of various kinds was not likely to be satisfied by this job, and the girls gave more introverted responses than if they had been, say, trainee airline hostesses. Fortunately, the extent of the distortion has not been sufficient to mask the link between extraversion and rated output, which appears, in the circumstances, rather more impressive. A question one might ask is how much more social desirability and 'job' desirability bias will appear when the test is used under selection conditions and how this can best be controlled. One procedure would be to reject individuals whose lie scores were above a cut-off value. If the cut-off point were set on the basis of this investigation, it would, in any event, be a safety-first measure as respondents appear to under- rather than over-estimate their extraversion when their lie scores are high. A very high lie score in itself might also be regarded as a negative indicator for selection.

As the extraversion dimension seems to be important in the early training phase of card-punch operators, it suggests that training methods should reflect the current theoretical and practical views on speed versus massed practice.

We are grateful to The Training Section, Department of Health and Social Security, Longbenton, Newcastle upon Tyne, for their cooperation in this project.

Br. J. Psychol. (1972), 63, 3, *pp.* 451–462
Printed in Great Britain

SELECTING CREATIVITY TESTS FOR USE IN RESEARCH

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This study looks at the problem of creativity test selection by examining the relationships between 15 divergent and non-divergent tests. This practical problem raises two main theoretical issues: whether 'creativity' is a unitary dimension across and within tests, and what relationship this range of abilities bears to IQ. A secondary aim is to evaluate four new tests by comparison with the 11 others. The tests were administered, along with two IQ reference measures, to 117 10- to 11-year-old school children. Factor analysis of the resulting intercorrelations led to the conclusion that 'creativity' implies an integrated range of abilities represented by the divergent tests which, although related to general intelligence in subjects of average IQ, remains factorially distinct from it; within tests, Fluency, Flexibility, Originality and Elaboration subscores were highly intercorrelated. Thus, providing that all modes of response and types of test material are included (drawing/writing; verbal/non-verbal content), the actual divergent tests chosen are not of vital importance, and calculation of anything other than Fluency scores adds little information. Analysis of the four new tests confirmed this conclusion, and led to suggestions for their further development.

Research workers in psychology and education frequently encounter the problem of selecting a set of creativity tests which will provide an accurate and comprehensive measure without unnecessary duplication. There exist certain well-known standard test batteries such as that of J. P. Guilford and his associates (e.g. Guilford & Hoepfner, 1966), the Minnesota Tests of Creative Thinking (Torrance, 1962; Yamamoto, 1964*b*), those of Wallach & Kogan (1965) and of Getzels & Jackson (1962), but these batteries are not often used in their original form for various reasons. Time would not permit, for example, the administration of the full Guilford battery in the case of many research projects. Particular theoretical orientations are often implied by standard test batteries (especially in the case of non-divergent tests such as the Barron–Welsh Art Scale (1952)); it is desirable in many cases to use a selection of tests which samples more broadly the full spectrum of approaches to creativity research. Most frequently, workers select individual tests from one or more of the standard batteries, but the reasons for their selection are rarely obvious and hardly ever stated.

The primary aim of this study is to look at this practical problem of test selection by assessing the factorial structure of a range of divergent and non-divergent creativity tests which is as comprehensive as possible. Previous factorial studies (e.g. Mackler & Spotts, 1965; Yamamoto & Frengel, 1966; Cropley, 1968; Cropley & Maslany, 1969; Dacey *et al.*, 1969) comparing creativity tests have restricted themselves to a limited range of approaches; no study has as yet made a general comparison of tests from different batteries.

This aim raises two main, interrelated theoretical issues which have concerned many previous workers (for a comprehensive review see Wallach, 1970) – whether 'creativity' can be regarded as a unitary trait across and within tests, and what relationship this range of abilities bears to conventionally measured intelligence.

J. P. Guilford, who was largely responsible for the resurgence of interest in creativity with his pioneering factorial studies, has proposed a 'Structure of Intellect

Model' (e.g. Guilford, 1967) which proposes that 'creativity' is essentially multifactorial both across and within tests. Some authors have supported the notion of distinct inter-test 'creativity' factors (McGuire *et al.*, 1961; Sultan, 1962; Yamamoto & Frengel, 1966) whereas others have argued that 'creativity' is a unitary trait, with high inter-test correlations (Wallach & Kogan, 1965; Cropley, 1966, 1968; Cropley & Maslany, 1969; Wallach & Wing, 1969). Guilford's proposition of distinct intra-test factors (divergent tests are usually scored for Fluency, Flexibility, Originality and Elaboration, as described in the next section) has not received much empirical support, most workers finding high intra-task correlations (Torrance, 1962; Mackler & Spotts, 1965; Wallach & Wing, 1969).

Wallach (1970), however, concluded that only ideational fluency (as distinct from word fluency) measures, and measures of originality which place no emphasis on evaluation or appropriateness of responses, truly represent divergent thinking, and that Flexibility and Elaboration scores have much more in common with convergent thought. To argue that 'creativity' and intelligence are independent domains presupposes a higher degree of intercorrelation amongst the measures of creativity than between those of creativity and intelligence. Wallach suggests, therefore, that workers such as Getzels & Jackson (1962) and Torrance (1962), whose test batteries did not meet this requirement, were mistaken in basing their conclusions on 'creativity index' scores, which had been formed by combining inadequately related measures.

It was also the failure to meet this requirement that formed Burt's (1962) and Vernon's (1964) main criticism of Getzels & Jackson's widely discussed study. Thorndike (1963*a*) and Marsh (1964) followed up this criticism by factorizing Getzels & Jackson's correlation matrix, and failed to find a 'creativity' factor free of IQ test loadings. Although Thorndike found a factor on which the creativity tests loaded highly, a substantial amount of this factor's variance was accounted for by the IQ tests, and he concluded (1963*b*) that there was no evidence for 'creativity' as a separate, distinct factor. Yamamoto (1964*a*, 1965) investigated this relationship further, and suggested that creativity and intelligence become independent only above a 'threshold' level of about IQ 120.

Wallach & Kogan (1965) suggested that previous studies had one common failing; creativity tests should be administered in a playful, anxiety-free atmosphere rather than in the more usual 'test' context. Under these conditions their ten creativity measures intercorrelated highly amongst themselves, and showed extremely low correlations with IQ. The importance of a game-like context has been confirmed by Boersma & O'Brien (1968), Kogan & Morgan (1969) and by Van Mondfrans *et al.* (1971), who suggest that this variable is of vital importance to the dimensionality issue. Subsequent factor analyses of Wallach & Kogan's data by Ward (1967) and Fee (1968) supported their conclusion that creativity and intelligence become separate domains under permissive testing conditions; it was further validated in pre-school children by Ward (1968) and in college students by Wallach & Wing (1969). Although reporting a high degree of consistency among the Wallach–Kogan measures, however, Cropley (1968) and Cropley & Maslany (1969) found significant correlations between 'intelligence' and 'creativity' factors in subsequent replications. Kogan (1971) replied that this had arisen from Cropley & Maslany's failure to rotate their

principal-components solution; Promax rotation yielded a pure 'creativity' factor and two 'intelligence' factors.

The present study attempts to be as general as possible in its consideration of these issues by administering tests from a wide range of standard batteries along with four new tests; by selecting a sample with a very wide IQ range; by adopting a playful, game-like context, and by considering both unrotated and rotated factor solutions.

Data collection

Subjects

The subjects were 117 10- to 11-year-old Durham school children (49 boys and 68 girls) drawn from four primary schools. The schools were chosen to provide as wide a range of IQ as possible; 'IQ index' scores were calculated by standardizing and combining General Ability Test–Verbal and Perceptual scores from the Morrisby Differential Test Battery (1955). These ranged from 71 to 138 with an overall mean of 102 (S.D. 13·5).

Procedure

In each school the tests were administered in two separate sessions on different days (about one week between sessions). Every effort was made to maintain a playful, anxiety-free atmosphere as specified by Wallach & Kogan (1965). The order of administration of the tests was chosen to reduce effects of boredom and fatigue by maximizing the contrast between one test and the next.

Brief description of the tests and scoring procedures

The 17 tests (two IQ measures, 11 divergent and four non-divergent tests) consist of nine presently existing tests in their original form, four tests adapted for use with 10- to 11-year-old English school children as subjects, and four new tests. All the tests were administered in group form, using pencil-and-paper responding.

Scores on the divergent tests were calculated according to the Minnesota protocols (Yamamoto, 1964*b*). Fluency scores are obtained by counting the total number of responses given to all items of the test; Flexibility scores by calculating the number of shifts amongst predetermined response categories for each item of the test (the Minnesota response categorization schemes were adapted for the tests used in this study). Originality scores are calculated by weighting each response given according to its frequency of occurrence in the whole sample, and by differentially assigning higher scores to more infrequent responses. The Minnesota weighting scheme was modified to fit more accurately the distribution of scores on each test; in general there were five possible scores for each response. Elaboration scores are obtained simply by assigning one mark if a response is specific (e.g. 'use a box for storing bananas') and 0 marks if the response is a general one ('use a box to keep things in').

IQ tests

(1) and (2) Morrisby's General Ability Test–Verbal and General Ability Test–Perceptual were administered as reference measures.

Non-divergent tests

(3) *Remote Associates Test.* Fourteen newly devised items were used with one original item from Mednick's (1962) R.A.T. so as to eliminate potentially unfamiliar American words (e.g. 'railroad') and to make items generally easier for 10- to 11-year-olds. Subjects are given three words (e.g. 'rat', 'blue', 'cottage') and are required to find a fourth word which links all three ('cheese'). The new items were devised such that there was no reasonable possibility of more than one correct response to each item; scores are simply the number of correct answers given.

(4) *Questionnaire.* A collection of items taken from various inventories designed to measure the personality characteristics associated with creativity. The 30 items, selected to enable 10- to 11-year-olds to understand and answer them, consisted of 10 items from Holland & Baird's

Preconscious Activity Scale (1968); 10 items from Barron's Attitude Questionnaire (see Barron, 1963) which measure complexity (seven items) and independence of judgement (three items); and 10 items from Child's (1965) scales, which measure tolerance for complexity (two items), tolerance for ambiguity (four items) and scanning (four items). The items, which were set out in random order, were all in the form of statements of opinion to be endorsed 'true' or 'false'. The wording of the statements was altered slightly in some cases to render them more meaningful to 10- to 11-year-olds; scores were simply the number of 'correct' answers as laid down by the various test authors.

(5) *Images.* A new test which is the application of a well-tried technique from the field of paired-associate learning, in a new context. It has been shown by Paivio and his associates (e.g. Paivio, 1969) that the ability to form images is the crucial mediating process in certain paired-associate learning situations, and that such situations provide an excellent means of measuring this ability. Many psychologists have linked imagination with creativity in the past; it is suggested that the imaging ability which mediates paired-associate learning processes may also be of importance in creative thinking.

The details of the technique are to be found in Paivio (1965); these were adapted to the classroom situation. Pairs of specially chosen words are read to subjects after instructing them to link the pairs by forming images of them (e.g. 'shoes-tree; imagine a tree wearing a pair of shoes on its roots'). The first word of each pair is then read out, and subjects are asked to write down the second word of each pair by remembering the image. An individual's score is simply the number of word-pairs correctly recalled. Three lists of six pairs each were constructed and used according to Paivio's findings; in this initial adaptation to the classroom situation, however, several of the experimental conditions were not adequately fulfilled. More stringent control would obviously be required in further similar applications.

(6) *Picture Preferences.* An adaptation of the Barron–Welsh Art Scale (1952) to the group testing situation; briefly, it has been shown that preference for complex rather than simple pictures is related to some of the variables important in creativity. In the present study, pictures were presented one at a time (by slide projector) and subjects replied 'like' or 'don't like' to each stimulus. Six of the original stimuli (three complex, three simple; 'BW' in Table 2) were used alongside six polygons (three complex, three simple) taken from Vanderplas & Garvin (1959; 'VG' in Table 2). Scores were calculated as

$$\frac{\text{number of 'like' responses to complex stimuli}}{\text{total number of 'like' responses}} \times 100\,\%.$$

Divergent tests

(7) *Consequences.* A widely used test which derives from the Guilford battery. Subjects are asked to provide as many answers as they can to questions like, 'What would happen if all the water in the world suddenly dried up?' Three items were used; Fluency, Flexibility, Originality and Elaboration scores were calculated.

(8) *Uses for Things.* Perhaps the best-known creativity test, this involves subjects providing as many uses as they can think of for four common objects, such as a brick. Fluency, Flexibility, Originality and Elaboration scores were calculated.

(9) *Groupings.* This new test, similar in some ways to Guilford & Hoepfner's (1966) 'Multiple Groupings' test, is designed to measure the ability to associate and reassociate cognitive elements in different ways. Wallach & Kogan (1965) see this ability as a fundamental part of creativity, and 'Groupings' is in a sense an extension of their 'Similarities' test.

Subjects are presented with a list of six common objects, and asked to form groups amongst them, stating why each particular group has been formed. The size and number of groups and number of objects left ungrouped are unimportant. It is emphasized to subjects that they are free to use each object over and over again in as many groups as possible – and it is in this important respect that 'Groupings' differs from object sorting tasks such as those of Gardner & Schoen (1962). Four items were used, the first two containing obvious possibilities for groups, the second two containing more unrelated objects. It was hoped that varying the level of difficulty of the task in this way would provide a more effective means of controlling discrimination between high- and low-scoring subjects than is usual in divergent thinking tests. Fluency, Flexibility and Originality scores were calculated; it is suggested that by the adoption of scoring schemes such as

those of Gardner & Schoen (1962), this test is also of potential use as a measure of the variables involved in concept formation.

(10) *What Kind Is It?* Another new test, designed to accompany 'Groupings', in which subjects are supplied with the name of a common object (e.g. 'penny') and asked to generate as many class-names for the object as they can ('money, disc, coin...'). Six items were used; Fluency, Flexibility and Originality scores were calculated.

(11) *Picture Meanings.* In this test, taken from Wallach & Kogan (1965), subjects are shown ambiguous figures (by slide projector for group administration) and asked to write as many answers as they can as to what the figure might be. Two 'line meanings' and two 'pattern meanings' stimuli from the original test were used alongside two Rorschach inkblots and two 'complex' figures from the Barron–Welsh Art Scale (1952). Responses were scored for Fluency, Flexibility, Originality and Elaboration.

(12) *Stories.* This is simply the application of the TAT as a 'Picture Meanings' test; subjects are asked to invent an unusual story about what is happening in the picture. Two stimuli from the Children's TAT were used (Bellak, 1950); responses were scored for Fluency and Originality (based on the distinct ideas expressed in stories), and Construction. Construction is a bipolar score like Elaboration; one point is assigned if the story follows a coherent, comprehensive sequence, 0 points if not.

(13) *Picture Completion.* Taken from the Minnesota Tests (Torrance, 1962), this consists of a series of four incomplete pictures (consisting, at most, of three lines) which are to be filled in to make as unusual a picture as possible in each case. Flexibility, Originality and Elaboration scores were calculated.

(14) *Drawing.* Another Minnesota test, very similar to 'Picture Completion'. Subjects are asked to fill in a series of empty circles with unusual drawings, using as many of the circles as possible. Both forms of the test were used ('Circles' and 'Lines'), making up 16 stimuli in all; Fluency, Flexibility, Originality and Elaboration scores were calculated.

(15) *Word Meanings.* Taken from Getzels & Jackson (1962). Subjects are asked to write down as many meanings as they can for eight words. Fluency and Originality scores were calculated.

(16) *Nonsense Words.* An adaptation of 'Word Meanings', in which statistical approximations to English words, taken from Miller & Selfridge (1950), are used instead of actual words – in a sense the verbal equivalent of 'Picture Meanings'. Subjects are presented with the words (e.g. 'grocid') and asked to provide as many possible meanings for each as they can. Four items were used; Fluency, Flexibility, Originality and Elaboration scores were calculated.

(17) *Similarities.* Taken from Wallach & Kogan (1965). Subjects are asked to provide as many unusual answers as they can to questions like, 'In what ways are a potato and a carrot alike?' Four original items were used; Fluency, Flexibility and Originality scores were calculated.

Results

To investigate possible sex differences, *t*-tests for uncorrelated means were carried out on each of the 17 tests; only one (the R.A.T.) showed that girls score significantly more highly than boys ($P < 0{\cdot}01$). All subsequent analyses were therefore carried out on the whole sample.

Product-moment correlation coefficients were calculated between each of the 44 scores; correlations between the IQ tests, the non-divergent tests and the Fluency scores on the divergent tests are shown in Table 1, and the intra-test correlations on the divergent tests appear in Table 3. The total correlation matrix was subjected to factor analysis by the principal components method, and a Varimax rotation was performed. Nine factors, which account for 76·8 per cent of the total variance, were extracted by the adoption of Kaiser's criterion (see Harman, 1960). The Varimax solution appears in Table 2, along with the loadings of the first principal components factor.

A high IQ group, comprising the 20 highest scorers on the IQ index measure, was

Table 1. *Inter-test correlations* ($n = 117$)

(Decimal points omitted; correlations of 0·18, 0·24 and 0·30 are significant at the 0·05, 0·01 and 0·001 levels respectively.)

	IQ		Non-divergent					Divergent										
Score*	1	2	3	4	5	6	7	8	12	16	19	22	26	30	32	36	38	42
1		62	58	−06	62	17	01	67	56	59	62	42	37	28	39	67	35	53
2			47	11	49	10	05	53	42	49	45	36	25	23	45	56	34	45
3				−10	73	24	10	49	45	47	56	42	39	23	38	58	26	42
4					−11	−17	02	−03	−05	−09	−07	00	00	04	07	00	−11	−03
5						28	11	54	51	56	58	41	36	30	31	54	27	49
6							−10	06	04	11	12	02	03	13	03	06	00	07
7								−09	−05	02	−13	−16	−06	03	−02	07	−07	−14
8									71	66	72	61	51	20	54	63	56	71
12										58	70	53	48	34	51	62	59	64
16											71	71	60	36	43	60	54	67
19												73	64	27	53	71	62	73
22													79	23	50	62	66	70
26														21	54	60	53	61
30															40	27	19	27
32																56	48	52
36																	59	63
38																		63

* For score labels, see Table 2.

Table 2. *Varimax factor loadings and variances; principal components factor* 1 (n = 117)

(Decimal points omitted.)

	Test score	1	2	3	4	5	6	7	8	9	P.C. 1
1	Verbal IQ	16	61	16	−52	10	04	19	04	06	70
2	Perceptual IQ	00	57	21	−32	26	30	18	−01	15	57
3	R.A.T.	28	78	15	−17	03	−13	02	04	05	57
4	Questionnaire	03	−05	11	05	−11	82	03	−02	−02	−03
5	Images	26	66	13	−33	−06	−20	19	06	10	61
6	Picture Preferences VG	−10	35	23	10	−04	−38	23	−51	−06	10
7	Picture Preferences BW	−14	19	16	14	−01	−09	11	82	−10	−05
	Consequences										
8	Fluency	28	31	14	−71	26	08	18	−05	07	83
9	Flexibility	30	23	17	−65	16	13	05	19	10	73
10	Originality	18	25	20	−70	25	11	24	−01	07	78
11	Elaboration	19	12	11	−79	13	−03	03	−04	11	68
	Uses for Things										
12	Fluency	28	21	19	−68	21	−07	−01	04	38	79
13	Flexibility	16	32	22	−65	16	−10	23	−10	14	75
14	Originality	13	26	31	−71	18	03	22	−02	06	76
15	Elaboration	33	−10	05	−43	30	−01	−29	19	52	53
	Groupings										
16	Fluency	47	26	13	−38	23	−05	61	06	14	82
17	Flexibility	45	19	11	−37	19	−02	62	09	16	76
18	Originality	38	17	−02	−34	10	09	68	−01	08	63
	What Kind Is It?										
19	Fluency	57	37	12	−50	25	−12	09	−14	18	88
20	Flexibility	47	34	09	−49	20	−12	03	−11	15	77
21	Originality	56	28	13	−49	16	−16	09	−15	14	81
	Picture Meanings										
22	Fluency	78	17	07	−21	39	06	22	−11	05	81
23	Flexibility	80	19	08	−21	34	05	18	−07	04	81
24	Originality	70	12	18	−33	34	10	27	−08	07	84
25	Elaboration	69	06	13	−33	37	04	19	12	05	80
	Stories										
26	Fluency	83	16	23	−13	23	03	02	01	03	74
27	Originality	77	17	23	−27	12	02	10	04	10	78
28	Construction	50	29	48	−21	03	−03	11	06	00	66
	Picture Completion										
29	Flexibility	−01	24	19	−15	06	05	11	−16	75	37
30	Originality	13	04	46	−02	−02	−09	33	02	54	38
31	Elaboration	09	10	57	−27	−14	−21	15	18	36	44
	Drawing										
32	Fluency	32	17	68	−24	28	17	−06	−06	18	68
33	Flexibility	13	10	76	−22	18	08	−03	09	05	52
34	Originality	15	25	70	−23	21	03	04	−12	05	59
35	Elaboration	40	07	55	−30	22	15	12	11	21	71
	Word Meanings										
36	Fluency	40	55	19	−34	38	08	02	11	14	81
37	Originality	41	54	20	−34	39	09	09	10	13	82
	Nonsense Words										
38	Fluency	32	03	16	−32	82	−13	05	00	07	73
39	Flexibility	35	09	15	−25	81	−09	13	00	02	73
40	Originality	35	06	22	−26	81	−07	17	−02	−02	74
41	Elaboration	33	16	07	−15	56	06	−03	00	09	54
	Similarities										
42	Fluency	51	10	23	−64	23	−05	14	−10	−05	84
43	Flexibility	52	07	20	−62	28	−07	11	−04	−08	82
44	Originality	52	−01	20	−64	15	−10	14	−06	00	78
	Total variance (%)	17·6	8·8	8·3	17·6	9·7	2·9	4·9	2·9	4·1	48·0

Table 3. *Intra-test correlations on the divergent tests* ($n = 117$)

(Decimal points omitted.)

Test score	Flexibility	Originality	Elaboration
Consequences			
Fluency	72	85	75
Flexibility	—	68	62
Originality	—	—	68
Uses for Things			
Fluency	80	74	68
Flexibility	—	84	28*
Originality	—	—	35
Groupings			
Fluency	88	82	—
Flexibility	—	72	—
What Kind Is It?			
Fluency	91	92	—
Flexibility	—	83	—
Picture Meanings			
Fluency	95	93	81
Flexibility	—	92	78
Originality	—	—	77
Stories			
Fluency	—	89	Fluency–construction
Construction	—	63	59
Picture Completion			
Flexibility	—	40	31
Originality	—	—	50
Drawing			
Fluency	63	67	77
Flexibility	—	61	52
Originality	—	—	54
Word Meanings			
Fluency	—	94	—
Nonsense Words			
Fluency	93	94	55
Flexibility	—	94	55
Originality	—	—	51
Similarities			
Fluency	93	93	—
Flexibility	—	86	—

* $P < 0{\cdot}01$; all other correlations $P < 0{\cdot}001$.

found to have a mean IQ of 122 (S.D. 7·8). The mean correlations amongst its divergent test scores, and between its divergent and IQ test scores, were calculated as 0·44 ($P < 0{\cdot}05$) and 0·09 (n.s.) respectively. To check that restriction of IQ range was not a major factor in producing this low divergent–IQ test correlation, the equivalent figure was calculated for a low IQ group (20 lowest scorers on IQ index) and for the remaining 77 middle IQ scorers. These were 0·31 (n.s.) and 0·46 ($P < 0{\cdot}05$) respectively.

Discussion and conclusions

Test selection

Factors 1, 2 and 4 in Table 2, along with the first unrotated factor, provide our main finding: 'creativity' implies an integrated range of abilities, represented by the divergent tests, which although related to general intelligence in subjects of average IQ, remains factorially distinct from it. The results of Table 1 also illustrate this: the divergent tests intercorrelating more highly amongst themselves than with the IQ tests (mean coefficients 0·55 ($P < 0{\cdot}001$) and 0·45 ($P < 0{\cdot}001$) respectively).

Factor 1 is a pure 'creativity' factor, reflecting the 'verbal response' bias of most divergent tests, and accounts for 17·6 per cent of the total variance; factor 2 represents the IQ tests. Factor 4, like the first unrotated factor, accounts for the variance shared by both domains and reflects the differences in content between tests (verbal/pictorial). Factor 3 represents the differences between the divergent tests in terms of mode of response, the two tests involving drawing loading more highly than all the others. This might be interpreted as the divergent equivalent of a bipolar group factor of general intelligence; the relatively small amount of variance it accounts for, however, illustrates its narrowness as compared with the broad group factors of intelligence. Factor 5 appears to be a task-specific 'Nonsense Words' factor, which probably emerged since many subjects had difficulty in understanding the task. The remaining four factors are also probably task-specific; these, however, account for minimal amounts of the total variance and are therefore unimportant.

The non-divergent tests form a much less coherent pattern of results than the divergent tests. The R.A.T., although correlating highly with IQ (mean coefficient 0·53; $P < 0{\cdot}001$) and loading 0·78 on the IQ factor, as Cropley (1966) would predict, also shows positive correlations with the divergent tests (mean coefficient 0·42; $P < 0{\cdot}001$). 'Images' produces a similar pattern of results, showing significant correlations with both IQ and the divergent tests (mean coefficients 0·44 ($P < 0{\cdot}001$) and 0·56 ($P < 0{\cdot}001$) respectively), and loading 0·66 on the IQ factor. Both 'Picture Preferences' and the 'Questionnaire' appear to be unrelated to all the other tests; the personality characteristics associated with creativity which these two tests measure, however, could well be insufficiently developed in 10- to 11-year-olds. Several studies using older subjects (often undergraduates) have reported positive correlations between picture preference and questionnaire measures, and divergent thinking (e.g. Barron, 1953, 1963; Eisenman, 1969; Eisenman & Schussel, 1970); the age variable is possibly crucial in these relationships. It is also worth noting that Wallach's (1970) contention that word fluency, as distinct from ideational fluency, tests are closer to convergent than to divergent abilities is supported by the correlations and loadings of the 'Word Meanings' test.

Great care must therefore be taken in interpretation when non-divergent and word fluency tests are used as measures of 'creativity'. Selecting divergent creativity tests, however, is rather like selecting IQ tests. Providing all modes of response and types of test material are included (this might also cover numerical material, not encountered in most standard batteries) the actual tests chosen are not of vital importance. Obviously it is preferable to select tests which load most highly on a general 'creativity' factor, and which have minimal loadings on others – thus of the

tests covered here, for example, a battery comprising 'Drawing', 'Picture Meanings' and any of the 'verbal' tests (e.g. 'Uses', 'Consequences', 'Similarities') would be adequate.

Creativity and intelligence

The results of the factor analysis support the notion that a principal components solution yields a broad general factor (P.C.1 in Table 2, with IQ test loadings of 0·70 and 0·57), whereas rotation of this solution gives rise to pure 'creativity' and 'intelligence' factors in addition to a much narrower general factor (Varimax factor 4). It is the relationship between the two domains, however, that still provokes disagreement. The high mean correlation between the IQ and divergent tests (0·45 ($P < 0{\cdot}001$), from Table 1) would be predicted on the 'threshold' hypothesis since the mean IQ of the sample, 102, is well below the proposed 'threshold' of 120 (Yamamoto, 1964*a*, 1965). This relationship is made more clear by the results from the high IQ group: when the mean IQ of the sample is raised to 122, the average correlation between IQ and the divergent tests drops to 0·09 (n.s.) whilst the average correlation amongst the divergent tests remains significant at 0·44 ($P < 0{\cdot}05$). The mean IQ–divergent test correlations from the low and middle IQ groups are both much higher than the corresponding figure from the high IQ group, confirming that restriction of IQ range is not a primary factor in producing this low correlation.

Scoring methods

Every correlation but one in Table 3 is significant at the 0·001 level; the exception is still significant at the 0·01 level. The generally lower correlations of the Elaboration scores are to be expected from the relatively crude method of assigning scores. These findings suggest that 'creativity' is unidimensional within divergent tests; in more practical terms, the time and effort required to calculate anything more than Fluency scores on divergent tests do not justify the small amount of extra information gained. Furthermore, to calculate Fluency scores only would render divergent tests much more amenable to automated scoring, and eventually result in their more widespread use.

The new tests

'Images', the only non-divergent new test, emerges from the analyses with encouraging results in correlating highly with the divergent tests (Table 1); it would be interesting to follow up this test under more precisely controlled conditions. 'Nonsense Words', although correlating as a typical divergent test (Table 1), produced its own task-specific factor (Table 2); this was probably because many subjects had difficulty in understanding the task. If this problem could be overcome, however (e.g. by using older subjects), the test's potential for variation and control of stimulus material seems considerable. 'Groupings' appears to be the most promising, loading as a typical divergent test (Table 2) and offering the possibilities of greater control of task difficulty, and of use as a measure of conceptualization. Its counterpart, 'What Kind Is It?', also emerges as a typical divergent test, but its possibilities are not as great.

The results obtained from the new tests suggest further directions for the study of

creative thinking; the fact that the analysis of as yet undeveloped tests supports the idea of a unitary 'creativity' dimension confirms and strengthens our earlier conclusions with established measures.

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(*Manuscript received* 3 *May* 1971; *revised manuscript received* 26 *September* 1971)

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Printed in Great Britain

PAPERS TO APPEAR IN FORTHCOMING ISSUES

(Not previously listed)

FREEMAN, N. H. & PARKER, D. M., Department of Psychology, University of Durham. Affective preference and misclassification in a novel/familiar identification task.

HARVEY, N., Department of Experimental Psychology, University of Oxford. The semantics of high–low continua: a note.

HOLDING, D. H., Department of Psychology, University of Louisville, Louisville, Kentucky, U.S.A. Recognition tests of visual information storage.

MACKAY, C. K., Department of Psychology, University of Aberdeen. Ikonic imagery in the severely subnormal.

MARCER, D., Department of Psychology, University of Southampton. Interference and short-term retention.

MARKS, D. F., Department of Psychology, University of Otago, Dunedin, New Zealand. Visual imagery differences in the recall of pictures.

MUMFORD, J. M., NEWTON, A. V. & LEY, P., Departments of Dental Surgery and Psychiatry, University of Liverpool. Personality, pain perception and pain tolerance.

REMINGTON, R. E. & STRONGMAN, K. T., Department of Psychology, University of Exeter. Baseline dependent facilitation: effects of a pre-reward stimulus.

SEYMOUR, P. H. K., Department of Psychology, University of Dundee. A model for reading, naming and comparison.

SMITH, S. & LEACH, C., MRC Social and Applied Psychology Unit, Department of Psychology, University of Sheffield. A hierarchical measure of cognitive complexity.

WETHERICK, N. E. & DAVIES, P., School of Studies in Social Sciences, University of Bradford. Inductive thinking in children of primary school age: the 'door and key' test.

WHITE, M. J., Department of Psychology, Victoria University of Wellington, Wellington, New Zealand. Hemispheric asymmetries in tachistoscopic information-processing.

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Printed in Great Britain

BOOK REVIEWS

Realism and the Explanation of Behavior. By MERLE B. TURNER. New York: Appleton-Century-Crofts. 1971. Pp. viii + 257. $11.00.

The aim of this book is to examine, and in fact to defend, the 'early Hobbesian' thesis of 'biomechanics'. In Professor Turner's eyes the Hobbesian thesis as applied to psychology seems to embrace, first, reductionism, in the sense of physiological reductionism, and, secondly, methodological behaviourism, which she looks upon as a step on the road to reductionism.

The first two chapters give a general account of reductionism in science, and attempt to establish it as the appropriate tactic for psychological theorizing and research. Professor Turner admits that examples of successful neurophysiological reduction are exceedingly uncommon; but she shows little patience with those who use this fact as an argument against the reductionist programme. Reductionism, she holds (for reasons which remain largely obscure to me), simply *is* the key-note of science: 'Method alone does not dictate our seeking reductive explanations, but the broad scope of our scientific tradition does coalesce to produce a rare sense of unity, the realization that all scientists share in a common discipline and a definable hierarchy of phenomena. Yet what is perhaps more significant is the realization that there is something natural about reduction. In an indubitable sense, mediation just is microreductive' (pp. 31–2). It is not enough for critics of reductionism simply to point to its present empirical shortcomings; rather they must show that there is a logical incompatibility between the concepts and languages of psychology and those of the physical sciences.

Professor Turner accordingly devotes much of the rest of her book to an examination of various arguments purporting to demonstrate such a logical incompatibility. She begins by examining the so-called 'argument from Gödel', which she presents in the context of a general discussion of automata theory and of 'computer simulation'. This argument alleges that since within certain mathematical systems there are theorems which no mechanical computing device could compute from the axioms of the system, but which a human being by turning to a 'metasystem' can show to be true, a human being can do something which a mechanical system cannot, viz. compute the theorems concerned. Turner seems disposed to accept the counter-proposals of J. J. C. Smart, which are to the effect that we might programme a computing machine to observe its own behaviour and induce therefrom the rules of its own operation. This would enable it to construct a 'metasystem' within which to prove the missing theorems. However, it is far from clear in what sense one could attribute to a computing machine the power to induce, prove theorems, or 'do' mathematics at all. From a psychological point of view, doing mathematics can only be regarded as the process of exercising certain complex and very abstract concepts in a systematic and directed manner. The mathematicians who programme computers and interpret their print-outs undoubtedly possess and exercise these concepts. The question which needs investigating is whether we have, or ever could have, grounds for attributing the possession and exercise of such concepts to the computers.

A substantial section is devoted to what Professor Turner calls 'the argument from intention'. Of the many arguments which might be thus labelled, the one which she has in mind seems to be this. It has often been suggested that there is a class of behavioural events, to wit 'actions', which we can only mark off by reference to intentions, and which we are only able to explain in terms of intentions, or of some other related class of things. Now 'intention' is traditionally a 'mentalistic' term, and as such might be thought to lie outside the scope of the physical sciences. In that case the reductionist approach to behavioural science will never completely achieve its goal. Turner's solution is to proffer a behavioural-dispositional account of intentions as lists of hypothetical S–R concomitances (she seems to regard a behaviourist analysis as a half-way house to reductionism proper), whilst at the same time admitting what she chooses to call 'phenomenological intentions' as the sources of such lists. The 'phenomenological intentions' are as it were step-ladders to the lists; step-ladders which are kicked away afterwards and are not mentioned in the propositions of the developed science.

It is impossible here to do even minimal justice to the implausibility of this idea. The situation

with which psychology is confronted in the matter seems to me to have the following elements: (*a*) It is impossible to dispense with the notion of intention, (*b*) Intentions cannot be resolved or even partly resolved into behavioural dispositions, for the only pieces of behaviour which 'express' intentions are 'actions', and actions have to be defined in terms of intention, and (*c*) Intentions do not consist in the holding in consciousness of some characteristic kind of 'phenomenological' datum. It is hard not to feel that here, as elsewhere in her book, Professor Turner has not merely failed to resolve the problems she has approached, but has failed to come squarely to grips with them.

It is, however, something to have approached the problems at all. Professor Turner must be one of the first psychologists to have tackled them at any length, and she writes with considerable erudition and in a very moderate tone. A little more care over matters of presentation could have made this a valuable book. As it is, the reader has to hack his way through an undergrowth of unelucidated symbolism and technical terms, clichés, neologisms and misuses of existing words, and tautologies or incomprehensible phrases and sentences. A lot of this arises, one suspects, because Professor Turner has soaked herself in the jargon of the philosophy of science. Indeed the single most powerful impression which the book leaves behind is of the generally harmful influence of the philosophy of science upon psychology. Today's philosophy of science tends to be based upon yesterday's physics, and tomorrow's psychology will only be fitted into that mould by the extruding of some central problems and the trimming of others beyond all recognition.

ALAN GAULD

Homo Psychologicus: Bridging the Arts and Sciences by a Study of Man as Worker, Player and Idler. By JOHN COHEN. London: Allen & Unwin. 1970. Pp. 192. £2.50 and £1.40.

This is a timely, imaginative and pungent book. It may be strongly recommended to all who feel a need to widen their horizons and re-examine their assumptions; and even more strongly to any who do not.

'Many psychologists', writes Cohen, 'have drifted away or exiled themselves from their native terrain, and are ready to explore anything so long as it has nothing to do with the history and geography of their own minds'. Sometimes this drift has been a consequence of insisting upon particular methods; although, as Cohen remarks, 'a comprehensive psychology cannot be delimited by particular methods, experimental or statistical for example, any more than a cartographer can omit oceans or mountain ranges merely because he cannot traverse the former or scale the latter'. Sometimes it has been a consequence of looking to other disciplines – technology, physics, chemistry, mathematics – for explanatory models, rather than to the study of the mind itself. Again, attempts have often been made to reduce psychology to some other allegedly more developed science. There is a sociological reductionism for which man is nothing but the product of culture, but zoological and physiological reductions have been most popular. Such views have encouraged the belief that psychology 'deals with provisional explanations which will surely be superseded when other sciences are in a position to take over the study of psychological questions in more rigorous fashion'.

As Cohen comments, 'the psychologist's task is not to cut his own throat by writing himself off'. In contrast to all such limitations and deviations, he boldly asserts that 'the subject-matter of psychology is distinctively human; it is not the mere "lining of physiology"... Our starting-point must be the phenomena of experience from the 'inside".' Thus once again the analysis of experience becomes the psychologist's primary task. Having thus set the stage, Cohen then discusses a number of themes of central human significance in both individual psychology (creativity; psychology and literature; and the experience of time) and social psychology (the role of secrets; work, play and leisure; and the malaise of culture). Perhaps the best way of indicating briefly the flavour of the discussion is to note that of some three hundred names recorded in the index only about one-fifth are from the psychological literature. The remainder are philosophers, scientists, theologians, playwrights, and so on, who have at one time or another made pertinent observations on human nature. This is a most effective way of awakening us to the true nature and magnitude of the task which psychology confronts, and to the catholic sympathies which that task demands.

Cohen's standpoint raises many questions which call for fuller treatment, especially in relation

to the views of other psychologists past and present. His distinction between introspection and phenomenological analysis is a case in point. However, he specifically states that he is not here attempting a systematic exposition of the subject, and it is not the least of the virtues of this pioneering work that it brings into prominence so many fundamental but frequently neglected problems. This book, it may be hoped, foreshadows the psychology of the future.

R. B. JOYNSON

Patterning of Time. By LEONARD W. DOOB. Newhaven and London: Yale University Press. 1971. Pp. xi + 472. £6.95.

There is something odd about the way psychologists, as a rule, treat the psychology of time, in so far as they deign to take any notice of it at all. They are apt to discuss it in splendid isolation, as though it had nothing to do with memory or emotion, with thought or action, or with social phenomena. Professor Doob's massive work is a salutary corrective to this blinkered view, for he endeavours, not without some success, to place the experience of time in the psychological foreground, where it has some claim to belong.

Early in the book (p. 13) he formulates his 'principal thesis', which, it must be admitted, is not breath-taking in novelty. It is this: we experience in the psychological present, just as we recall the past and anticipate the future, and we sometimes have to renounce one of these orientations as a result of our devotion to the other(s). A temporal judgement (we are told) is 'patterned by beliefs and attitudes' which are recollected from the past and which affect anticipations of the future. Nearly always we face a choice in time and, in choosing, we must renounce. In the light of this thesis, the text is directed to answer three main questions: (i) When and why do we make temporal judgements; (ii) What is the nature of these judgements; and (iii) What are our feelings about them?

In attempting to answer these questions, Professor Doob draws on a vast bibliography of some 900 items on psychology, psychiatry, social science and, to some extent, *belles lettres*. Most of the chapters are followed by a concise summary of the investigations to which reference is made.

The book falls into three parts. The first, entitled 'Processes', is elaborated in seven chapters: 'Focus', 'Potentials', 'Behaviour', 'Temporal Motives', 'Perception and Scope', 'Drives' and 'Secondary Judgement'. The second part deals with 'Functioning' under 'Development', 'Deviations', 'Groups' and 'Work and Risk-taking'. Part III is called 'Manipulation', and is devoted to the 'Arts' followed by chapters on 'Changing' and 'Envoi'. The choice of titles for his chapters is not, perhaps, the author's strong point.

The 'foundation for the entire book', says the author, and 'the guide to the analysis' of Chapters 3 to 8 is a chart described as 'The Taxonomy of Time'. Unfortunately, this chart is far from clear, and it will not greatly help the average reader to find his way through the book.

The work concludes with an inventory of some fifty 'principles', of which the following are typical examples: 'each society provides appropriate information for passing temporal judgement'; 'temporal information is likely to have an appropriate effect on behaviour'. The reader cannot help wondering how much 'appropriate information' is transmitted by these principles. They are followed by a set of hypotheses, for example, 'people are likely to accept and execute a plan when they have confidence in their own ability to carry it out', which are not terribly exciting.

Occasionally there is a looseness of expression as, for instance, on p. 351, where we read that the feeling (of frustration) 'increases even more than the apparent duration of the interval'. But a comparison between two such disparate variables seems devoid of meaning, for they share no common unit of measurement. We are also told that 'a challenging non-monotonous task demands the full attention of the person if he is to succeed', an assertion which is open to question if, say, driving a vehicle on the road is representative of the tasks the author has in mind.

If any further criticism is to be made of this book, one feels obliged to say that it is unnecessarily long-winded. Apart from this, the author, out of the democratic kindness of his heart, fails to discriminate between his sources. All are treated as though they were on a par. Yet some are trivial and insignificant while others are far-reaching. This makes it hard for the reader to discern any hierarchy of significant properties in the experience of time or, indeed, any *essential* structure in such experience.

Yet, when all that is adverse is said, the author is to be commended for having written a scholarly work which might find a respectable place in psychological libraries, and students of psychology could do worse than browse through this book.

JOHN COHEN

Psycholinguistics: Biological and Social Factors. Edited by JOHN MORTON. London: Logos Press. 1971. Pp. 215. £3.50.

This volume grew out of a symposium convened by the editor at the last International Congress of Psychology in 1969. It contains contributions from nine authors of whom six are psychologists, two are linguists and one is a physical anthropologist.

In the opening chapter, Bernard Campbell, the anthropologist, examines human language from an evolutionary and comparative perspective. After outlining the differences between primate call-systems and human language, he makes the point that, although man has produced, in language, an entirely new cultural tool, this has not replaced the original primate communication system which still survives more or less intact and remains the principal means of expression of the subjective emotions. From comparative psychology, Campbell moves on to comparative anatomy. He cites evidence, both fossil and contemporary, for the claim that the development of language required the evolution of novel structures in the brain and argues that this evolutionary process must have occurred gradually over a considerable period of time. Clues as to how it occurred, however, seem rather sparse.

In the second chapter, John Marshall asks 'Can Humans Talk?' His scholarly essay deals with problems of a philosophical or meta-psychological nature. After considering some of the elaborate stratagems which behaviourists have used to avoid admitting, at any rate in plain English, that man can speak, Marshall examines certain ideas of Bühler in order to clarify exactly what language behaviour consists of. He appears fairly confident that, even if we adhere strictly to behaviourist rules of evidence, it will eventually be possible to convict the adult human race not only of talking but of 'symbolization' also.

In the next chapter, David McNeill, one of the pioneers of developmental psycholinguistics, tackles the controversial topic of linguistic universals; the hypothesized common characteristics of all languages whose origin is claimed to lie in the innate structures of the human brain. He distinguishes between 'weak linguistic universals', which have their cause in some universal characteristics of perception or cognition, and a 'strong' variety whose origin is purely linguistic. These ideas are then briefly exemplified through comments, first, on the reversal of the customary relationship between actor and object in sentential complement constructions which occurs for the verb 'promise' and secondly, on the fact the verbs are typically inflected to agree with their accompanying nouns while the converse rarely if ever occurs.

In the fourth chapter, Roger Wales examines the processes of 'Comparing and Contrasting'. According to his own excellent synopsis, 'This chapter is concerned with the biological characteristics of man which seem to make him unique in his possession of language and with the ways in which the structure of sentences affects the relative ease of processing them. The first is considered from the vantage point of a study of the acquisition of comparison; the second is considered in the light of studies of the relevance of definiteness, topicalization and presupposition in processing sentences'. The two principal ideas which Wales explores are the linguistic notion that comparative sentences may be regarded as the adjectival equivalents of sentences containing transitive verbs and the psychological hypothesis that the capacities needed to learn basic relational sentence forms, such as the comparative and the transitive verbal, are no different from those needed to perform relationally in a discrimination task. Thus the aim of the chapter is to try to relate psychological (relating to non-linguistic behaviours) structures to linguistic ones.

We return directly to the topic of linguistic universals in the following chapter in which John Morton asks 'What could possibly be innate?' His aim is to show that this question is in no sense a simple one and he succeeds admirably in doing this. He first questions the common assumption that language is a uniquely complex form of behaviour which is acquired with particular rapidity. This he does by analysing the process of inferring the three-dimensional shape of an object from its two-dimensional outline, following the principles of the 'picture grammars' of Guzman and Clowes. When compared with this, transformational grammar seems

quite simple! Which is precisely the point that Morton wishes to make. He suggests that if there is a specific innate ability which underlies language and distinguishes man from other animals, then it may well be cognitive rather than linguistic: simply the extent which he can represent and organize knowledge of environmental regularities.

The next contributor, Schlesinger, continues the discussion of linguistic universals with a study of the sign language used by the deaf in Israel. This is an independent language, not parasitic on any spoken form, which appears to serve as an adequate means of communication although its users follow no consistent word order and have no unambiguous means of indicating basic semantic roles such as action or goal. Schlesinger takes this as evidence against any conception of linguistic universals as a set of basic grammatical relations such as 'subject of' or 'object of'.

A. A. Leontiev, of the Institute of Linguistics of the Soviet Academy of Sciences, discusses the 'Social and Natural in Semiotics' in a chapter that will prove very difficult, at any rate for the Western reader. By 'semiotics' he understands that part of psychology which examines behaviour dealing with or governed by symbols. The main explanatory concept is that of a 'functional organ' which develops 'as a result of specific activities of various physiological mechanisms united in a unified functional system'. Unfortunately, the whole discussion is pitched on such a level of abstraction and generality that it is nearly incomprehensible, except perhaps to the initiated.

The following chapter, 'Syntax and Semantics' by Ruqaiya Hasan will also prove tough going. She examines the relationship between the semantic level of language and the levels of grammar and lexis. She sees this as being hierarchical, with the semantic level providing the interface between the linguistic system and the extra-linguistic world. The framework for the discussion is not the usual one of transformational grammar but is derived from the viewpoint of Lamb and Halliday. The latter part of the chapter contains a number of important observations. In particular, it is pointed out, using the example of the passive in English and in Urdu, that although, for reasons of economy of description, syntactic structures in different languages may be labelled with the same name this cannot be taken to mean that they will function in the same way. Some implications of this fact for the discussion of linguistic universals are noted.

The last chapter, 'The Integrated Study of Language Behaviour', by Tom Bever is the longest and most wide-ranging of the book. It opens with some quotations from Wundt sounding remarkably like Chomsky. Unfortunately Wundt's acute theoretical insights did not lead to experiments due to his view that language fell within the domain of social psychology and was therefore susceptible only to introspective and not empirical investigation. Bever next outlines some of the evidence that language acquisition is a process of discontinuous and sometimes radical restructuring of knowledge rather than its piecemeal accretion. After noting that it is this evidence which has led many to adopt a nativistic view of linguistic abilities he proceeds to his central theme, the nature of the relation between syntactic structure, as described by the linguist, and language behaviour, as investigated by the psychologist. He makes the important point that intuitions about language are never purely grammatical in origin. Thus a sentence may be judged unacceptable for its semantic implausibility or the sheer ungainliness of its syntactic structure besides any violation of syntactic rules. Bever then presents detailed discussion based on the results of experiments on sentence perception and memory, of how syntactic knowledge is used in speech processing. His central thesis is that it is no longer good enough for us to talk glibly of the speaker-listener 'using' his knowledge of grammar without examining in detail how he employs it in performing different tasks since the behavioural results of linguistic knowledge will never be task-independent.

Three themes run through this book. First, there is the continuing interest in linguistic universals and the innate linguistic abilities of the human species. Secondly, there is a realization that the psychological investigation of language will draw on many sources of data and, unlike for instance the study of verbal memory, it will not rely almost exclusively on a narrow range of paradigms performed by consenting adults within the privacy of the laboratory. Finally, there is an appreciation of the fact that language behaviour is not just talking but communication within a context and for a purpose, and neither context nor communicative purpose can be ignored.

The book offers a stimulating sample of current research. It is, as I hope this review indicates, full of ideas and the editor deserves special credit for the breadth of its scope. It is not a textbook

but will be very worthwhile reading for researchers and for students aspiring to a serious knowledge of psycholinguistics. It is recommended both to them and to those less dedicated persons who would like to see what psycholinguistics is about and will not be put off by the book's periodic linguistic and philosophical episodes.

R. Q. GOODWIN

Language Acquisition: Models and Methods. Edited by RENIRA HUXLEY and ELISABETH INGRAM. London: Academic Press. 1971. Pp. xiii + 311. £5.00.

This volume of papers and discussions from the 1968 conference presents a diverse range of theoretical positions and methodological approaches. Some familiarity with contemporary issues in psycholinguistics is assumed and, though not essential to the reader, this would be useful since a recurrent theme is the difficulty of relating a Chomskian linguistic model of language acquisition to current research. The papers range over four main areas: sociolinguistics (five papers), grammar (five papers), cognition and language (three papers), and clinical (one paper). Each paper is followed by comment and discussion except for the brief, clinical résumé by Hécaen on sentence production in normal children and patients with diffuse and unilateral cerebral disease. This report stands rather apart in its orientation from other sections. Separate discussions are devoted to the topics: 'Limitation of the word "Language"', 'Early utterances', 'Comprehension and grammaticality judgements', 'Learning strategies', 'Critical periods in language learning', and 'The word and its referent'. These discussions are short, usually some four sides of print, and therefore the treatment is somewhat cursory.

By way of introduction, the reader is recommended to the paper by Ingram on 'The requirements of model users' for it focuses issues to which many of the papers refer. Ingram proposes four attributes which must characterize any model if it is to be of value. They are: stability, plausibility, relevance and compatibility. The then current linguistic model appears to encounter difficulties in all these respects. For example, it is pointed out that notions like 'the homogeneous speech community' and 'the idealized native speaker-hearer' have little *relevance* for studies in communication across indrviduals and social groups. The idealized speaker-hearer notion is also dismissed as 'not helpful' in relation to cognitive development (p. 177). Similarly very little *compatibility* is seen as existing between the concepts underlying the linguistic model and the more general concepts in social and cognitive research. In the opening paper on 'Sociolinguistics', Professor Hymes spells out some of the drawbacks of a linguistic theory of competence in the context of American cross-cultural studies of the acquisition of communicative competence. But it is a moot point whether more or less confusion will result from such an enlargement of the existing definition of linguistic competence to cover knowledge of the rules of style and social interaction, bearing in mind the difficulties this term has already encountered. This theoretical analysis is followed by two papers which emphasize methodological aspects. Ervin-Tripp distinguishes between competence for the basic rules of language and competence involving ranked skills of communication. Her discussion of dependent variables in research makes explicit the major role of social features in the generation of performance and underlines the previous view that much more than grammar and phonology is involved in language development. In the second paper, Cazden considers independent variables. The argument is developed that language universals and language specifics may be viewed as a continuum. Variation in the child's experience will affect acquisition along this continuum in various ways or to different degrees; language-specific components require more exposure and show greater variability across children. A matrix of social factors and the setting out of degrees of certainty in the knowledge obtained from them together provide a useful framework for research to be done.

An interim report of current research on social factors in England is also given by Robinson, who describes a 'Use of Language' programme aimed at improving and expanding the range and type of grammatical and lexical categories available to children. The research is specifically oriented to the theory of 'restricted' and 'elaborated' codes of Bernstein, who also contributes a short paper on 'Language and Roles'.

In the section on 'Grammar' much of the questioning of a Chomskian model of language acquisition is directed at its basic postulates: the centrality of the syntactic component, the inviolable position of deep structure, and the distinction between deep and surface structure. The possibility that the underlying structure can be acquired by inductive reference from an already acquired stock of surface structures is developed by Schlesinger, who maintains that a

nativist explanation is no explanation at all and, though he rules out neither approach, inclines to the view that grammar can be learnt. Input to the language acquisition model, he suggests, contains not only utterances but utterances coupled with their meanings – rules being derived from these meaning-utterance pairs. These rules the child must learn. Yet another approach, a Piagetian one, is developed by Sinclair who concurs with Schlesinger in stressing the primary importance of relationships rather than categories (see also Church, p. 176). The contention is that the infant brings to language acquisition innate cognitive functions which result in universal structures of thought, and that these cognitive structures should be used to explain acquisition rather than the reverse (p. 123). Similarities are drawn between the types of sensorimotor co-ordinations and pre-operational structures and the base rules of deep structure; the similarities are suggestive but appear rather fragile. Bellugi's paper emphasizes the systematic, rule-governed and non-imitative behaviour of children's language. Data are presented on the auxiliary inversion in questions, on the relationship of negative and indefinite, and on the development of case marking in personal pronouns. Much is made of the view that utterance is characterized by an order of emergence of certain types of structures and the same types of 'immature' forms or transient hypotheses across children. Yet it is well to remember that the data are restricted to three children before we conclude too readily that children develop similar automonous systems for language.

In the papers on 'Cognition and Language' the emphasis shifts from linguistic structure to cognitive process, and the relationship with a linguistic model becomes even more tenuous. Mehler makes the point that we cannot claim that the notion even exists as to what a competence theory means for cognition in general because we have no idea as to the limitations imposed upon the mode and possibility of thought (p. 223). In similar vein Church states flatly that 'the notion of linguistic universals is useless' (p. 177). The central problem is how language relates to perceiving, thinking, learning, feeling, judging and development – the child's total grasp of the world about him. His paper describes a variety of techniques for studying these aspects.

Cognitive process rather than linguistic structure is the theme of both Mehler and Bever. Memory development is conceived by Mehler as the increase in the number of corrections made on an invariant core such that qualitative as well as quantitative changes occur. In a separate context, he states that it is not the frequency but the *way* a child observes some correlation in nature that is important. Bever separates language performance from sensitivity to language structure with its connotation of exceptionless rules. His thesis is that non-grammatical strategies of immediate speech processing are learned processes of the utilization of language in comprehension; the locus of these acquired strategies of speech perception is in the dominant hemisphere, though Bever does not claim that the data demonstrate a direct association between the development of auditory dominance and the development of an *actor–action–object* strategy in speech perception.

It is evident from the following extract (pp. 154–5) that a satisfactory relationship with a linguistic model (see also pp. 157, 225) has yet to be resolved: 'That's right; it's a big mess. – It is a bit of faith too; you hope that a principle will sort it out. – We are waiting for a principle to come along to help us do better. – It sounds to me as though people are saying that you can't keep psychology and linguistics apart, but you can't find any real way of getting them together either. – The problem is that they are inextricably together. – But we are trying to sort some things out.'

This conference report helps 'to sort some things out'. Perhaps the main interest for the reader is the survey of areas of current research, and the ideas and incidental information generated among its participants, even if the stability and plausibility, to quote two of Ingram's criteria, suffer from the lapse of three years. One wonders therefore why this volume could not have been produced less attractively but more cheaply as a working edition.

DAVID SALTER

Language and Thinking in Human Development. By D. G. BOYLE. London: Hutchinson. 1971. Pp. 191. £2.25 and 90 p.

Although the nature of the relationship between language and thought is hardly a new issue in psychology, and few would want to deny that their role in the development of human behaviour is crucial, if complex, it would be quite reasonable to attribute much of the current interest in

this problem to the exciting and expanding research of psycholinguists both in this country and in America. But while the past year or so has seen the appearance of several excellent undergraduate texts presenting either accounts of the underlying linguistic theory with a broad review of research, or a more detailed exposition in some selected area, say development of language, seldom has there been more than a brief chapter on language and cognitive development. An earlier student's guide to Piaget by the author contained just such an introductory chapter, and it is a disappointment that his current book does not fit too comfortably into what is a clear gap in undergraduate texts. While it contains a range of clearly presented material, the balance and relevance are often uneven, and there are important omissions. The book is in three parts: two main sections entitled 'Language and Imagery' and 'Language and Thinking', with a final 'Philosophical Perspective'.

The first chapter begins by outlining semiotics and briefly considers some animal studies of reasoning or symbol use (von Frisch, Köhler, Maier), concluding that these are of a lower order than man's, but ends with three illustrative problems where man's use of visual imagery interferes with his symbolic powers. The following chapter outlines the development of vocabulary and simple morphology. McNeill is mentioned for a discussion of holophrase and further grammatical development, but the reference is not given. After a passing mention of Piaget, Bruner, Luria and Furth, the main issues between Chomsky and Skinner are cogently summarized. Chapter 3, 'Communication', has some six pages on information theory, and a page or so outlining a Chomsky-type grammar, which the author concedes is impressive but leaves the reader with the impression that it has explained nothing about the child's ability to master the complexities of grammar. But what is surely surprising is the complete omission of any mention of the considerable experimental work on children's language acquisition which has been catalysed by the theory. 'Culture and Confusion' covers Whorf and linguistic relativity, Ryle's notion of category mistakes, and Bernstein's identification of social language codes. Chapter 5, 'Mental Imagery', is mainly devoted to an interesting account of the breakdown and recovery of a psychotic, but mentions Galton's questionnaire and McKellar's classification of imagery, as well as Freud's studies of dreams and the use of drugs. The author concludes that the origins of imagery are a superstitious fear of a strange world, and primitive attempts to make sense of that world; the study of imagery may lead us to wisdom because it shows us the dark places to avoid. Many psychologists (e.g. Piaget, Bruner, Paivio) would, of course, consider that there are more optimistic and constructive uses of imagery in studying children's conceptual activity. Much the same kind of point might be made about the following chapter on 'Symbolism and Play', which is replete with entertaining anecdotes ranging from the Egg Marketing Board to McLuhan, as well as the revelation that the institutional form of the pursuit of vertigo (one of four types of sociological game) is best manifested in steeplejacks. The reader is at least referred to Millar's recent survey of the psychology of play.

Part 2 is on firmer ground, beginning with a chapter covering the behaviourist theories of thinking, and the experiments on problem-solving by Wertheimer, Duncker, Maier, Luchins and Bartlett. (Note that the final problem on p. 83 is misprinted, and should read 'every digit from 1 to 10 is represented'.) This straightforward and clear presentation of admittedly well-established material continues with a chapter on 'Intelligence and Creativity', covering the work of Getzels and Jackson, and of Wallach and Kogan, and adds a sensible discussion of the relevance of creativity studies to educational practice. 'Conceptualizing' has a brief philosophical introduction, then presents the studies of concept attainment of Heidbreder and Bruner, Goodnow and Austin, ending with a brief outline of Luria's studies of the directive function of language in the child. The last two chapters are more technical: one on the epistemological and logico-mathematical emphases in Piaget's theory, and one covering the role of the brain, particularly the effects of lesions and aphasia. The third, and briefest section, provides a useful philosophical perspective to the empiricist–rationalist issue, some comments on the status of explanations of linguistic ability, and of meaning but here it is mediation theories and the semantic differential technique which are described, componential analysis is dismissed in two lines, and there is no reference to any studies of semantic development, irrespective of their theoretical origins.

The preface indicates that points are often made briefly rather than at length, in the belief that a nod is as good as a wink, not only to the blind, but to the far-sighted too. But is this so? While the general or lay reader may find the book interesting and highly readable, even the mildly myopic may emerge somewhat blinkered with respect to current research. What is

really needed is a text which provides a much fuller account of both recent and longer established material than is readily available, but which is less formidable to the undergraduate that the research originals.

GEORGE BALFOUR

The Acquisition and Development of Language. By PAULA MENYUK. Englewood Cliffs, N.J.: Prentice-Hall. 1971. Pp. xvi + 285. £3.50.

Despite the attention paid to theoretical approaches in its first and last chapters, this book is a perfect example of the wide gap between 'rationalist' and 'empirical' treatments of language development. On the one hand are attempts by writers such as McNeill to explain the child's ability to learn language on the basis of a unitary and universal theory of transformational grammar. This rests on the assumption that, granted the validity of transformational concepts such as linguistic universals, deep and surface structure relations and so on, it is possible to 'deduce' the what, how and why of language acquisition.

Compared with such theoretical superstructures, often built up on the basis of the recorded utterances of a few middle-class children, the book under review gives a wide-ranging survey of empirical results dealing with all kinds of child speech. The material reported ranges from the earliest reaction of infants to speech noises through 'normal' acquisition of phonology, syntax and semantics to a discussion of abnormal development and dialect variations due to racial and social factors. The author's own extensive research in the area enables her to make many sharp comments on possible weaknesses in experimental design. Perhaps because of this awareness of the immense complexity of children's performance, all conclusions put forward are tentative; demonstrating a becoming humility about how much further research needs to be done before anything can be said about most aspects of language development.

It must be admitted, however, that this emphasis on empirical investigation consorts somewhat uneasily with the theoretical framework in which the research is introduced. Although stressing that the availability of linguistic descriptions has opened up a new way of looking at the structure of children's language, the author's exposition of transformational grammar and other theoretical approaches is simpliste, sometimes misleading, and, in any case, not strictly adhered to in discussion of experiments. For instance, it is not always clear whether investigations are concerned with the child's competence or performance, or whether postulated rule systems are meant to refer to deep or surface structure.

In general, then, to those reared on the heady excitements of the new psycholinguistics, this book offers no provocative theoretical insights into the underlying causes of the overall structure of child language. Against these weaknesses of empirical fragmentation, Menyuk provides a down-to-earth compendium of research in this field. While in itself adding little to the final aim of an explanatory theory of language acquisition, any such theory will have to digest the accumulating evidence reported here concerning the multiplicity of factors affecting linguistic development.

JUDITH GREENE

Picture Language Machines. Edited by S. KANEFF. London and New York: Academic Press. 1970. Pp. xiv + 425. £4.50.

The subtitle of this book is: 'Proceedings of a Conference at the Australian National University, Canberra on 24–28 February, 1969'. Seventy-eight people attended this conference and 12 of them were authors of its 16 papers. None of the five people coming to the conference from psychology departments were among the dozen authors. However, the most prolific author (Clowes, who contributed his own three papers, part of a fourth, and the foreword) has subsequently returned to the Laboratory of Experimental Psychology at Sussex. These notes on the conference 'cast of characters' lead one to a justified conclusion: not all the pages in this book are intended to carry arguments, reports, and exchanges of definite interest to psychologists. Fortunately, many pages would accord with such an intention.

The first four chapters each introduce a topic. The participants at the conference came from departments as diverse in interest as tropical pasture and philosophy and their need to realize common ground undoubtedly helps readers who are not familiar with the four topics. Narasimhan outlines what 'Picture Languages' are about; some programs for interaction between a computer and a person are reviewed by Bobrow, how 'Linguistic Descriptions' can be obtained

from linguistic rules is discussed by Clowes, Langridge and Zatorski, and Jacks considers 'Design Principles for Graphics Software Systems'. This last-mentioned introductory chapter is the only one psychologists interested in language and perception might find stony ground, but it is relevant to ergonomists and may intrigue those who are sceptical of the wonders of automation. The chapters by Narasimhan and by Bobrow are useful reviews, the latter one making clear how careful people who assert that 'You will never make computers x' are going to have to be to maintain their aplomb. X will probably never be a verb in a null set, but its set is currently a shrinking one. Clowes's chapter on linguistic topics manages in 27 pages to present a lucid but sometimes critical summary of much material that is central to developing views on language, and less directly, on perception.

Written material is one link between language, which is primarily spoken, and pictures which cannot be. In some way the sentential structures of spoken language can be, if imperfectly, appreciated from their written form. In perception we deal with objects, as Gibson and also Gregory have stressed. Objects can be visually depicted. Hence there is a hint that the structures we can discover in understanding language may also be useful in understanding perception. Here, one basis for this hint is the observation that both the meanings of sentences and the identity of objects are inferrable from their graphic forms. A common feature of pictures and of languages is that the number of examples of each is practically infinite. As must by now be well known the notion of generative rules is adequate to deal with the problem of infinite utterances from finite means. Perhaps less well known is the attempt being made to apply the kinds of rule found fruitful in language studies to the processing of pictures. The chapters by Clowes entitled 'Picture Syntax' and by Macleod entitled 'On Finding Structure in Pictures' illustrate how this recent work is being pursued. I doubt if people not specializing in this area would read these chapters very quickly but both authors make their main points clearly enough for them to be grasped by most of us. Clowes is especially concerned with the relational aspects of pictures and concludes that at least in recognizing letters, relatively small degrees of abstracting may suffice. Macleod concentrates on more traditional stimulus aspects of pictures, and concludes that no general system for processing pictures may be practicable for engineers.

The chapter by Narasimhan on 'Computer Simulation of Natural Language Behaviour' is in part concerned with problems that Chomsky has brought to widespread attention. A main theme of the paper is that Chomsky's views are founded on fallacies. This theme is not, of course, novel, and it is, of course, perfectly possible. Narasimhan's arguments against Chomsky are not uninformed which makes them preferable to some. They are more stimulating than others that I have read but they have weaknesses; the topics are far from simple and relate to many studies. This may be why there is no mention of the writings of Lenneberg in formulating *phylogenetic continuity* as one of two boundary conditions in a metatheory of behaviour in general. As Lenneberg (1967, and most succinctly on p. 237) has argued in detail and at length against any easy clear notion that is equivalent to Narasimhan's one of phylogenetic continuity, this omission is an important one. In the more specific parts of this paper there are some approaches to natural language conversation that may help those of us who want to do more than count the frequency of whatever it happens to be in transcripts of spontaneous speech.

Bobrow and Klatt consider 'Information Reduction as a Technique for Limited Speech Recognition' by computer. They report success rates above 90 per cent for items spoken by one speaker and from sets of less than 150 words. The recurrent difficulties of speech-recognition systems recur. Speech from a different speaker is not well recognized; different accents threaten to disrupt recognition; the extension of these recognition methods to sequences of spontaneous speech is certainly going to be complex.

Kaneff's chapter 'Learning in Picture Processing' is one that undergraduates might well find interesting, and that, of course, is not at all to belittle its content. He attacks empty formalisms and doubts the seriousness of non-adaptive models of learning. The stress in the chapter is on action and the constraints on it are seen as topics that demand study; 'action is determined by structure' is an appealing aphorism.

Barter, writing on 'Data Structure and Question Answering', extends the review undertaken by Dobrow in the introductory chapters and considers some of the languages in which programs like Bobrow's STUDENT were written. Some of his examples of how to set about answering grammatically complex questions are especially informative.

There are other papers in the book, for example, the one by Stanton, that confirm *Picture*

Language Machines as a book that certainly contains enough to make it a sensible buy for departmental libraries. Unhappily the book is less than well produced: the print is double spaced but has 19 letters to the inch of line, only one restricted typeface is used and eked out by hand formed characters, only left-hand justification is maintained. Editorially there are failings: there is no index, references are given paper by paper and while one knows where the end of a book is the *middles* of chapters are less easily found. (The references for each paper follow the paper and precede the barely edited discussions.) The references are not always complete and some appear in unlikely places: a book by Guido Küng is mentioned with understandable enthusiasm in one discussion and cited in the reference list for the following paper. These deficits are a pity because there are parts of this book that should be widely read.

GODFREY HARRISON

The Analysis of Intelligence. By J. P. GUILFORD and RALPH HOEPFNER. New York: McGraw-Hill. 1971. Pp. xiv + 514. £7.90.

Publication of Guilford's 1967 book *The Nature of Human Intelligence* represented a noteworthy event in the history of intelligence testing. For too many years, psychologists have perhaps taken too much pride in the widespread acceptance and use of their intelligence tests. In 1967, and earlier, Guilford voiced concern about the need to better delineate the basic components of these tests. However, over a period of 20 years he has not only voiced concern, but also tried to do something about it. The product of his labours took the form of the now well-known structure of intellect model, an effort to assess creativity, divergent production and social intelligence, and an attempt to relate his model of cognitive functioning to Piaget's and other theorists' ideas about human development.

This 1971 contribution by Guilford and Hoepfner is primarily a source book containing descriptions and examples of tests used in defining the structure of intellect and much up-to-date empirical evidence relevant to it. In addition, the authors comment on the status of past and current work on intelligence and make suggestions for future investigation. Their aim, coming as no surprise to followers of Guilford's earlier articles and books, is to help construct '... a taxonomy of intellectual functioning with ability concepts that are invariant over marked changes in analysed test batteries and moderate changes in subject population' (pp. xi–xii). The authors believe that the needed taxonomy can best be achieved within the context of an 'operational-informational' psychology and by greater emphasis on the 'what' of cognitive existence. To permit the needed intensified study of the taxonomy of intelligence, the authors make clear that they believe a somewhat less vigorous pursuit of the 'how' and 'why' of human behaviour would not be a tragedy. They single out the behaviourists for being primarily responsible for overemphasis on these latter questions: 'A great distracting influence has been the preoccupation with the minutiae of stimulus-response psychology, and the kind of thinking that goes with it' (p. 349).

The polemical character of this pronouncement is itself something of a distraction. One of Guilford's important contributions has been his recognition and elucidation of the need to integrate more adequately the studies of intellectual assessment and cognitive development in children. It seems unfortunate that he has not given similar recognition to the interplay between the abilities in an individual's repertory at any given time and the learning process. Sorely needed, if the study of intelligence is to get off its present plateau, is the greater commitment to the dispassionate construct validation of concepts of intelligence than to the existence of an elusive invariance or a perpetual motion of S–R bonds.

The authors clearly believe that the path of advance will be more along the lines of conceptual complexity than of simplicity: 'Psychologists have taken the demand from Occam's razor much too seriously' (p. 350). Which path a researcher takes is often a highly personal one and the science of psychology is still too young an endeavour to be confident about the tactics that are most likely to facilitate progress. However, what is possible, and needed, is a constructive critical attitude toward empirical findings regardless of the theory or methatheory out of which they grew. From this perspective, the reviewer cannot help but express impatience with an approach that rests so heavily on test construction – no matter how rational the procedures employed – and factor analysis. Factor analysis is a powerful method for isolating the dimensions of particular sets of data, but having isolated them construct validation, including experimental

studies, should then commence. In that way, one can determine the degree to which distinctive statistically derived dimensions bear on behaviour in particular types of situations under particular conditions. Research directed toward this sort of determination might well solve some of the apparent puzzles in the structure of intellect model. For example, the results of Guilford's research programme suggest that divergent and convergent thinking abilities are distinctive and tend toward independence. Yet, several other workers have not been able to corroborate these findings. Is this the case because Guilford's samples of subjects tend to be relatively homogeneous? Or are there theoretical flaws in the convergent–divergent thinking distinction?

These questions do not reflect unfavourably on Guilford's dedication, perceptiveness, and skill as a factor analyst. They are simply a natural part of the scientific enterprise, whose hallmarks are inquiry and individuality in the pursuit of knowledge. This book contributes to this enterprise through its detailed presentation of one influential approach to intelligence. It is not easy reading. In places it may seem either overly confident or a bit dogmatic. But students of intellectual processes will find it a rich source of test items and of ideas.

I. G. SARASON

Cognitive Psychology. Edited by A. SUMMERFIELD. *British Medical Bulletin*, vol. 27, no. 3, pp. 191–290. London: The British Council. 1971. £2.00.

This issue of the *British Medical Bulletin* is a sequel and a complement to the 1964 issue entitled 'Experimental Psychology'. It resembles its predecessor in format, intention, general content area and high editorial standard. There are 16 contributions from predominantly British authors who are actively engaged in the experimental analysis of human information-processing performances. Each contribution succinctly reviews a current research topic and lists a generous bibliography. This issue differs from its predecessor in its contributors and its selection of specific topics which, in brief, cluster around problem-solving, psycholinguistics, memory, neurological disorders and skills. Although the contributions are too compressed to be readily digested by a general reader, they fulfill two functions. First, they give a quick and an impressive overview of the kind of work which is currently being done in one important corner of human experimental psychology. Second, they provide authoritatively up-to-date starting points for any student who wishes to become acquainted with this work and who has access to appropriate library facilities. Both functions are valuable, especially at a time when so much psychological work is being published. So the editors are to be congratulated. It is also to be hoped that this issue of the *British Medical Bulletin* will not be the last to be devoted to experimental psychology.

I. M. L. HUNTER

The Study of Twins: By PETER MITTLER. Harmondsworth: Penguin Books. 1971. Pp. 192. 45 p.

The Study of Twins forms part of the Penguin Science of Behaviour series designed to give an up-to-date view of psychology and provide a flexible alternative to conventional teaching texts. As the best short introduction to twin studies in psychology currently available it is a welcome and valuable addition to the series.

The book is in three parts. The first part describes the methodology underlying twin studies, taking care to emphasize the problems of generalization that follow from using twins as well as their unique advantages. The reader is steered away from an outmoded 'nature–nurture' approach towards the more fruitful interactionist approach which attempts to relate both biological and social influences in behaviour and development. The second part of the book discusses, within this framework, findings from a wide variety of studies grouped under the headings of biological factors, cognitive processes, personality and mental disorders. The final section, an overview, draws conclusions, identifies recent important developments and makes suggestions for the future.

Dr Mittler's book is very well written and informative and those of us who teach psychogenetics will be grateful for such a useful and inexpensive text to recommend to students. The only flaw in this otherwise admirable book is its failure to introduce the reader to the more quantitative aspects of the subject. The multiple abstract variance analysis of Cattell, the applications of biometrical genetics carried out by the Birmingham group and the multivariate methods developed by Vandenberg and his associates are all recognized as important developments neces-

sary for a better understanding of how genetic and environmental influences on behaviour interact, but the reader is given very little idea of their scope or power. While a full account would certainly have been out of place in a short introduction, more about these quantitative aspects of twin study, in order to better orientate the reader towards future research, might have made this very good book even better.

D. W. FULKER

The Development of Behaviour. By W. MARY WOODWARD. Harmondsworth: Penguin Books. 1971. Pp. 208. 50 p.

Contrary to what one would expect from the title, the book is almost solely concerned with cognitive development, and follows in broad outline the basic series of stages as first described by Piaget. Of the six chapters, three are descriptions of intellectual development in infancy, one and a half are concerned with Piaget's middle stages and one-half of a chapter describes formal operations.

Most readers will find great value in Woodward's excellent interpretations in depth of observations made by Piaget and her descriptions of what is involved in operations carried out by children at different developmental levels. The author goes beyond mere description of the stages and has attempted to explain why and how change occurs. A central theme is borrowed from Piaget: learning occurs through the child's interaction with his environment. However, Woodward is willing to be tentative in areas where data are not sufficiently specific. Other models and explanations are used where they seem to fit. She discusses the way in which Miller, Galanter and Pribram's TOTE, as well as operant conditioning, can be incorporated into her basic theory. At the same time, the weaknesses of simple S–R associationistic models when applied to complex human learning are shown. She also pulls together data from diverse sources to validate her model, e.g. Kendler and Kendler's study with children on ease of learning reversal and non-reversal shifts, Berlyne's work on exploratory behaviour and Terman and Merrill's norms for the Stanford–Binet IQ test.

The author stresses, as Anastasi did 13 years ago (and Gestalt psychologists before that), that change occurs in the course of interaction between learned and unlearned behaviours, and for this reason, distinctions between the two types are irrelevant. Such a stance may be critical to the solution of the nature–nurture controversy. Another, related, philosophical position Woodward takes and psychologists would do well to note is that 'the problem situation, the inferred central processes and behaviour, are being considered as an indissoluble whole' (p. 181). Thus if one wishes to understand and to explain behaviour, one must look at the acting, feeling, thinking individual in the context in which he is behaving.

The book criticizes the traditional methodology of traditional psychology and points out that momentary behaviour samples in time may be missing important issues. For example, Woodward reports one of Piaget's findings that a behaviour pattern stayed in an infant's repertoire for three days only!

The text is clearly written and there is less unnecessary technical jargon than that found in most contemporary monographs. I liked the chapter headings (e.g. 'Looking and Listening', 'Acting for Effect'), as they serve as summaries of what the child is actually doing.

Almost all of the references cited are of studies carried out on British, American and Swiss children. The author makes the point that the study of children from other cultures is important in order to understand what processes are common to all and which are culturally based, but she ignores studies such as that of J. Goodnow or of B. Lloyd, who has applied Piaget's theory to studies of Yoruba children.

The book is not really meant for as wide an audience as the Series Editor suggests. It does presume that the reader has some background in developmental theory and in theories of concept formation and learning. She suggests many different topics which need further investigation, so that the book would be extremely useful to researchers.

The last comment to be made is both criticism and compliment. Woodward does not go into detail on any stage after the sensorimotor period. However, her exposition of infancy is so well done that I wish she had expanded on the other stages as well.

Overall, this is an excellent monograph that attempts explanation of cognitive development based on detailed observation. It is worthwhile for advanced students and for investigators concerned with developmental theory, cognition or learning.

SHEILA ROSSAN

Recent Developments in Psychogeriatrics. Edited by D. W. K. KAY and ALEXANDER WALK. London: British Journal of Psychiatry Special Publication, no. 6. 1971. Pp. 125. £1.60.

Despite being intended primarily for psychiatrists, three earlier members of this series – those on schizophrenia, affective disorder and anxiety – have proved of lasting value in the teaching of abnormal psychology. It is difficult to foresee whether the same will hold for this new addition. Some of the chapters are entirely clinically orientated – Post on the tactics of diagnosis and Donovan, Williams and Wilson on their experiences in setting up a psychogeriatric service – and these will be of greater interest to psychiatrists than psychologists.

Of more general interest are a review by Roth of classification and aetiology, which considers especially the brain changes to be expected with normal and abnormal ageing and the relevance of these for disturbances of behaviour. There is an interesting chapter by de Alarcón on the contribution of various social factors to illness and hospitalization in the elderly. Brothwood provides a survey of the available helping services. Of additional interest is Anderson's chapter on the relationship of physical and mental disorder, although it does suffer from a tendency towards the intermingling of experimental and clinical observations. Lishman's chapter on the neuropathology of amnesic states is very informative but is by no means specific to the problems of old age and its inclusion does underline the distressing tendency to regard ageing and organicity as more or less 'the same thing'.

The other contributions are more research orientated. According to the editorial foreword, contributors were encouraged to concentrate on their own recent work and this they have done. Thus 'recent developments' becomes 'what I have been doing' and the omission of related work tends to detract from the value of the contribution. These papers concern neurosis in the community aged (Bergmann), suicide amongst the elderly (Barraclough) and, of most immediate interest to psychologists, Savage looks at the psychometric assessment of intellectual functioning and its relevance for clinical diagnosis.

For those already interested in the problems of psychological disorder in the elderly, there is plenty of interest in this fairly cheap book; but, unfortunately, there is little that is likely to whet the appetite of those currently outside the field.

ANTONIA WHITEHEAD

An Introduction to Abnormal Psychology. By J. E. ORME. London: Methuen; New York: Barnes & Noble. 1971. Pp. ix + 141. £1.40 and 70 p.

This book is an attempt to provide a text in abnormal psychology appropriate to the early stages of a psychology course. It sets out to fill the gap between the facile overviews of this area often found in general introductory texts and the unnecessary complications of more advanced presentations.

The author has attempted to cover the basic topics of importance, and interest, to the student unfamiliar with this subject matter. An outline of the traditional approaches to psychiatric classification is followed by some indication of attempts which psychologists have made to apply techniques, such as multivariate analysis, to the problems which arise. Delinquency and the psychosomatic disorders are given brief mention in separate chapters (each of less than four pages of text). This presentation of a variety of abnormal syndromes is followed by an examination of the principal techniques of personality assessment, illustrated by some reference to appropriate measures of personality.

The remainder of the book is largely devoted to an examination of the origins of the disorders reviewed earlier. Ante-natal influences are examined, with reference to many aspects of this problem. The author avoids the traditional concentration on specific genetic and chromosomal factors. A rather brief mention of selected physiological influences, covering the topics of body-build, periodic phenomena and the EEG, leads into a broadly based discussion of general post-natal factors. This chapter is a useful introduction to a diversity of conflicting viewpoints on the causation of abnormality, from psychoanalysis, through learning theory, to the existentialists. Objectivity and excessive dogmatism have been avoided among these pitfalls for the unwary.

The abnormal state in a basically normal individual forms the basis for a discussion which

ranges from hypnosis to the paranormal. This is followed by the last substantive chapter, covering aspects of intellectual abnormality, from deficiency to genius and including reference to measures of cognitive functioning. Also included is a strangely placed section on the mind-body problem and the effects of physical therapies. The book concludes with a short review of outstanding problems in the field of abnormal psychology, and a discussion of the role which the psychologist might play in attempts to obtain clarification of these problems.

As a brief introduction to the area, this text should contain information, presented clearly, which will interest, inform and stimulate the student. In his choice of content, the author has clearly tried to present an overview and has, to a great extent, succeeded. Most of the important topics are at least mentioned, although the quality of presentation and of further references varies considerably from topic to topic. Two areas are, I feel, rather lacking in coverage; first, the whole problem of childhood and adolescent abnormality, and secondly, the neurological disorders.

The style of the book could lead to difficulties and, perhaps, this is most obvious in the section on delinquency. The need for brevity seems to have led to a chapter which lacks essential cohesion. A further example of the problem posed by excessive brevity is the possibility of confusion which could exist when a reader encounters six second-order factors from Cattell in the discussion on classification, but only four second-order factors from the same author in the discussion on the 16 PF as a personality test. An additional sentence or two in explanation would have avoided this difficulty.

The inclusion of some reference to specific measures of personality and cognition is a possible area for concern. It may well be that the treatment possible at this level is not adequate to provide a useful introduction. This is possibly best left to a later stage in a course, where the principles of the construction of measures of individual and group differences can provide a basis for more informed discussion.

In summary, despite the minor problems outlined, the book fulfills its aim. It should provide an interesting and adequate text to support the brief outline of abnormal psychology which the author, and the reviewer, feel should form part of any introductory course in psychology.

PETER BRITTON

Drugs, Brain and Behaviour: Proceedings of Symposium on Psychopharmacology in Durham, 1970. Edited by DAPHNÉ JOYCE. Published by University of Durham and North East of England Branch of the British Psychological Society. Pp. 98. £1.10.

The North East Branch of the British Psychological Society, which organized this symposium, aimed at bringing together an audience of varied interests and to stimulate interdisciplinary exchanges. The editor of this monograph has attempted to make the seven contributions understandable to most readers interested in psychopharmacology.

There are three parts – Extrapolations, Dependence and Tolerance, and Drugs and Endogenous Chemicals – which do not really reflect the breadth implied by the title. Nor do the papers in each section reflect the range implied by each section heading. The overall impression, therefore, is that the monograph has been somewhat 'contrived', and the editor has obviously tried to make the most out of a very limited range of contributions.

The first chapter by Daphné Joyce on extrapolations in psychopharmacology is an excellent, broadly based, yet selective review of the problem. She stresses the magnitude of human use of psychoactive drugs and argues that our ignorance about their properties, mode of action and effectiveness necessitates a multiple-research approach involving several kinds of between-subject extrapolations. It is suggested that animal tests have an important role in the development of psychoactive drugs; but such extrapolations from animals to man must take into account the uncertainties involved in the assessment of drug effects in human subjects.

In the section on dependence and tolerance, Alan Ogborne reports a study of heroin users, and K. Davison and J. W. Osselton describe their rather limited study of REM sleep in six heroin addicts. They very briefly discuss their observations in relation to current theories of the role of 5-HT in the development of opiate dependence and tolerance.

Roger Porsolt reports some interesting experimental studies of drug tolerance in the rat and

points out the distinction between behavioural and pharmacological tolerance. It is suggested that compensatory behaviour by the animal to counteract the effects of a drug accounts for behavioural tolerance. This is an interesting distinction and its implications are quite justifiably emphasized both for the design of experiments which test for drug tolerance and for the clinical use of drugs.

D. M. Warburton discusses some implications for psychopharmacology of the theory of 'neurochemically coded' pathways. Although Warburton reviews a number of studies involving drug-induced changes in cholinergic systems, his evidence for the cholinergic hypothesis of schizophrenia does not seem as conclusive as he suggests. This is much more of a theoretical paper than the other contributions in this monograph.

C. Van Toller describes a recently developed technique of immunosympathectomy, used to investigate the role of catecholamines in behaviour. This paper is probably the least relevant to psychopharmacology.

On the basis of self-stimulation studies (mainly using rats' brains) S. J. Cooper examines the general hypotheses that a deficit in neural mechanisms for reward and punishment underlies depressive illness, and that therapeutic drugs act on endogenous chemicals in reward and punishment areas of the brain. Various evidence is reviewed and a rather limited study (using amphetamine-barbiturate mixtures) is reported.

Studies concerned with amphetamine-barbiturate mixtures almost tend to dominate these proceedings. This obviously reflects the research interests of several of the contributors. However, one gets the impression that their concern in the psychoactive effects of these mixtures has been fostered by evidence of their widespread use amongst drug users, rather than by their use as a direct pharmacological tool.

The anticipated interdisciplinary exchanges apparently did not materialize at the symposium, since no discussions or commentaries have been presented in the monograph. This is a pity, because the editor introduces each section very well, and the inclusion of discussion and comments should be an essential ingredient of all symposium proceedings.

The papers give extensive references and, on the whole, this is a useful monograph for students and research workers in psychopharmacology.

Finally, the stencilled production of this monograph contains a sprinkling of typographical errors, but the proceedings do not really justify publication in any other form. G. LOWE

Psychopathy: Theory and Research. By ROBERT HARE. New York and London: Wiley. 1970. Pp. x + 138. £2.25 and £1.35.

This book is one of a series to be published on behaviour pathology under the general editorship of Brendan Maher. It presents an experimental psychologist's approach to the study of psychopathy. The explicit aim is to provide a concise but authoritative review of theoretical issues and empirical research suitable for undergraduates and postgraduates in psychology and others, principally in psychiatry, criminology and social work.

Psychopathy is a condition of considerable theoretical and practical interest to a great number of persons of diverse disciplines, ranging from sociology to physiology and genetics. Inevitably, the literature is extensive and at present confused and it would be unreal to expect any one volume to provide a wholly satisfactory integration of so much diffuse material. To attempt to do this for such a varied readership, as this volume does, and then within 118 pages of print, might be taken by some clinicians as savouring of psychopathy itself! Yet with the rapid expansion of information (but slow accretion of knowledge) there is a need for continuing attempts at cautious integration, even though restrospectively over time such efforts may be seen as premature. Professor Hare encompasses a vast range of material with apparent equanimity and students will be grateful for his summary of such a controversial area.

Brief but well-documented coverage is offered on cortical and autonomic correlates of psychopathy, together with a summary of psychometric studies. A broad spectrum of various kinds of learning experiments related to psychopathy is also presented. The book concludes its eight chapters with one on socialization and another on the modification of psychopathic behaviour. Thus, the author, while emphasizing the experimental approach, does not ignore broader social and clinical issues.

Needless to say, much of the research reported provides equivocal results and the usual prob-

lems of lack of comparability arising from, for example, sample differences, variations in methodology and experimental design, are made explicit; this may prove of course too perplexing for the non-psychologist. Eleven pages of references provide an adequate enough diet for further reading by the advanced student. At times, however, the multi-level aims of the volume are clearly seen to be in conflict with the space available for exposition. Thus, for example, the few lines of print spent on explaining conditioning or retroactive inhibition seem excessively extravagant in relation to the complexity of material so briefly presented elsewhere. The author's own work, of course, is well represented but not selfishly so and many investigations are summarized and commented upon with commendable clarity.

The book is likely to prove useful to second- and third-year undergraduate courses in abnormal psychology, in addition to providing a basic framework and stimulation for further reading for the more advanced psychology student. Certainly, in this reviewer's opinion, and in spite of its highly condensed presentation, a volume of this kind is much to be preferred to some of the relatively *ad hoc* collections of papers published elsewhere. However, its appeal to non-psychologists may prove more limited.

R. MALIPHANT

Emotional Problems of the Student. Edited by GRAHAM B. BLAINE, JR. and CHARLES C. MCARTHUR. New York: Appleton-Century-Crofts; London: Butterworths. Second edition. 1971. Pp. xxx + 388. £5.00.

This book is a product of the University Health Service at Harvard. Of the 16 contributors, all are or have been involved with students there as psychiatrists, clinical psychologists, deans or counsellors. Thus, it is clear that this is a work of some authority. But Harvard is rather special, as some of the authors are not above pointing out, and British readers will want to know how much of the Harvard experience has relevance to universities and colleges that are co-educational, less elitist and somewhat less well endowed with psychiatric help.

On the whole, this test is comfortably passed. A statement that subsidization of students may 'diminish the vigour of the maturation process' and 'exaggerate dependency' (pp. 12–13) might suggest a frontiersman philosophy somewhat extreme, but in fact points only to a minor cultural difference. The problems discussed and the method of approach outlined are very relevant indeed. There are abundant detailed case studies, and in Walters' chapter on 'Student Apathy' and the editors' own contribution on 'Problems Connected with Studying' both teachers and physicians will have no difficulty in recognizing familiar presentations. Although the contributors share the assumptions of the psychoanalytic approach, this should not be allowed to deter even the most sceptical. The introductory chapter, by Erikson, sets the tone for the whole book, for the emphasis throughout is on explanation and analysis in terms of identity formation, role determination and ego psychology. The language is refreshingly free from jargon, and almost completely avoids the opposite error (not uncommon in American writing on counselling) of cracker-barrel folksiness – 'sophomore slump' (p. 129) tends a little that way, but describes succinctly a well-known syndrome.

McArthur, in the chapter on patterns of neurosis in students, describes well the tight-rope walked by many: 'Their social role as intellectuals can easily be perverted into intellectualizing ...and their "egg-head" value on seeing myriad shades of grey in every event perverted into compulsive doubt...Intellectualizing is most pervasive; since the use of that defence is so strongly prescribed both by the university culture and the "good" families who sent the boy to the university, one can only be surprised that it is not universal' (p. 64). A main task of adolescence is to conciliate between the idealized self and a realistic self-concept. We achieve this usually by lowering our sights, and bringing our ideal within reach, but for the students who over-value intellectual achievement, this is often just what they cannot do. The on the whole hopeful prognosis for neurotic episodes in students described here accords well with British experience, and the reported incidence (about 10 per cent) closely matches that in male British university students.

'The Role of the College Psychiatrist' as described by Farnworth and Munter is seen as being at least as concerned with the identification of points of stress in the academic community as with the treatment of individual patients. This would seem to be a reasonable ordering of priorities, not yet widely accepted in British universities. On the other hand, one of the tasks deemed proper for the Harvard psychiatrist (a task which has not yet come the way of his

British counterparts) is to advise the college president as to which threatening letters should be taken seriously!

Perhaps the most stimulating chapter of all is that by Sorensen on 'The Return of the College Drop-out'. The results presented here (70–87 per cent graduation rate amongst those who return) support those institutions who practise a liberal readmission policy, and open several avenues where multidisciplinary research needs to be done. The identification of those in need of further support by the use of psychiatric interview and psychological testing, especially projective testing, is persuasively advocated. Indeed, the usefulness of the clinical psychologist in Student Health Services is stressed in many of the contributions.

This second edition (the first was published in 1961) is considerably extended – the passage of a decade is marked by a chapter on the use and abuse of drugs (has it all really happened *since* then?) which is Woottonish, restrained and perceptive. Other additions range from the socio-psychological 'The Movement' to the intensely practical 'The Text is the Adversary'. Everyone having to do with students, whether as teacher, administrator, physician or counsellor, will find in this readable book new insights and new challenges to established practice.

J. L. CRIGHTON

Psychology of Women: a Study of Bio-Cultural Conflicts. By JUDITH M. BARDWICK. New York: Harper & Row. 1971. Pp. 242. $7.95.

With a title like 'a study of bio-cultural conflicts', the reader might expect this book by Judith M. Bardwick to be a compilation and evaluation of biological, psychological and sociological studies, with some attempt to indicate how various factors interact to generate a 'psychology of women'. To some extent these expectations are met. The author begins with a discussion of the contributions of psychoanalytic theory to ideas about the psychology of women, and outlines her disagreement with some of these notions (primarily the emphasis on the absence of male genitals as the crucial factor in feminine development rather than the presence of 'creative inner space', p. 15). She then reviews some studies on the effects of hormone fluctuations during the menstrual cycle, pregnancy and menopause on behaviour of various types (the emphasis is on mood shifts and sexual behaviour) and some of the literature dealing with hypothalamic control of reproductive hormones (this in a chapter called 'Differences between male and female brains'). After a selective review of sex differences in various abilities, and in such personality traits as dependency, passivity and aggression, she develops the central theme of the book, namely that the observed psychological differences between men and women are the result of physical differences which directly and indirectly affect personality development. She believes that constitutional sex differences in size, activity level, impulsiveness, and sensitivity to social stimuli, as well as differences in genitals and level of sexual activity, cause boys to experience more and earlier conflict than girls in meeting social expectations and prohibitions. Since girls do not experience as much conflict with their social environment, they have less need to develop internal standards for conduct and self-esteem, and remain more dependent upon the approval of others. Women do experience more role conflict as adults, however, in that although they have a wider range of role models, the traditionally feminine role of wife and mother is not accorded the same high status as occupational achievement is for men whose choice, though more restricted, is unambivalently socially-valued. Bardwick believes that in 'normal' development, marriage and maternity allow women to develop feelings of self-esteem and achievement. This internalized self-esteem, together with a need to develop an identity outside the home once the traditional role demands decline ('10–15 years after marriage', p. 89), prepares the woman psychologically to risk success or failure in the realm of occupational achievement. This view fits nicely with a movement to encourage women (only) to devote themselves to parenthood while their children are young and then to resume or begin a career outside the home. It is based upon the author's beliefs that 'the differences in external genitals result in less intense and less significant sexual impulses for the girl' (p. 11), that 'girls seem initially [i.e. from birth] better equipped than boys to perceive cues from people, to appraise responses accurately, to respond verbally – in short to be "good"' (p. 207), and that 'there is a phylogenetic inheritance that makes maternity the most fulfilling role for women, at least when children are young' (p. 211). This last at least is identified as a 'hunch', but it would seem that every step of her argument depends upon similarly unsupported statements. Bardwick's view of the 'psychology of women' (what, for heaven's sake,

is psychology – unqualified – about?) is for the most part psychoanalytic; she does not question the validity of such constructs as the oedipal complex or the process of identification. Nor is there evidence that she is familiar with social learning theorists' arguments against the adequacy of an analysis of personality in terms of traits. She asserts rather than argues that hormonal variations in women have a direct influence on personality unmediated by cultural factors.From the somewhat chatty tone and asides which incongruously vie with the jargon of the psychological studies, it seems that much of the book is congruent with the experience of the author and her friends, but it does not provide the sort of analysis and evidence which one might have been led by the title and topic to expect.

MARY B. PARLEE

The Psychology of Fear and Stress. By JEFFREY A. GRAY. London: Weidenfeld & Nicolson. 1971. Pp. 256. £2.10 and £1.05.

This refreshing book could more accurately be called 'Fear in Animals', for that is its main subject. Dr Gray gives a thoughtful account of ethological, genetic, psychological and physiological aspects of fear in animals, and is not constricted by current taboos which so often hamper a broad view of the area. As the title suggests, he is not afraid of words like 'fear' to describe emotional states in animals, and is equally at home describing overt behaviour, its physiological correlates, and ethological background. Several chapters are noteworthy. Genetic and early environmental influences on fear in the rat are traced in illuminating detail, though the work of Dykman and others in dogs is surprisingly omitted. Emphasis is on the dialectical interaction between genes, prenatal and postnatal environment, and clear diagrams explain experiments on the influence of parent rats, their offspring and siblings on one another.

The best section concerns the learning of fear and its neurophysiological substrate. The review of classical conditioning and acquisition of avoidance behaviour would be hard to better (e.g. work of Miller, Mowrer, Sidman, Amsel and Roussel, Brown and Wagner, Solomon, Kamin, Herrnstein). Description of the extinction of avoidance is less adequate, and relevant experiments are left out which might modify his conclusions about the inefficacy of blocking avoidance or prolonging the CS (e.g. Baum, Katzev, Weinberger).

In many accounts of the learning of fear the brain still remains an unopened black box. Dr Gray is most original in the way he peers inside the box to understand how it modifies input and output to produce the end result which we label as fearful behaviour. From experimental data about acquisition of approach and avoidance he infers which properties a brain requires to produce such behaviour, and then turns to see how his model fits the neurophysiological facts. His tentative model on p. 218 deserves to be quoted: 'We can imagine the following sequence of events when an animal is faced with a stimulus which is at first novel, but which acquires some sort of significance as a result of the environmental regularities in which it participates.

'On the first few occasions when the animal is exposed to the novel stimulus, the frontal cortex/septo-hippocampal "stop" system dominates, and inhibition of both approach and fight flight is maintained. As the meaning of the stimulus changes with repetition (through the operation of the hippocampal analysis of familiarity), or with the attachment of rewarding or punishing significance by following unconditioned stimuli (acting via the amygdala), the relative strengths of the inputs along the fornix, the *stria terminalis* and the medial forebrain bundle to the medial hypothalamus will change correspondingly, until either approach behaviour or escape behaviour dominates. Skilled escape behaviour and active avoidance behaviour...would be learnt on the basis of inputs arriving along the medial forebrain bundle. Frustrative non-reward, on the other hand...would act via the septo-hippocampal system and the fornix'. An attractive feature is the flexible search in discussion for integrating principles and readiness to discard theories which depart from the known evidence.

Less convincing is the author's description of the general adaptation syndrome and its homeostatic role in controlling population density. The relevance of this to fearful behaviour does not emerge. Space is also lavished on vague relationships between sexual physiology and fear, but decisive links are hard to find.

It is sad that the author changes abruptly in the final chapter when he turns from animals to human beings. Careful fitting of theory to clinical fact disappears, and he enters a mythological realm barely recognizable to those who have to treat psychiatric disorder. The vast literature on the treatment of fear in volunteers and in patients is virtually ignored. The poverty and

arrogance of this chapter highlight the riches and humility in its predecessors. It could have been left out with profit.

The book seems to have been long in gestation, since there are few references after 1966, and important ideas since then aren't noted, e.g. those of Seligman on the role of 'preparedness' in facilitating the formation of CS–US links, and the experiments of Baum and others on extinction of avoidance. This makes the book a bit dated, but it is, nevertheless, an excellent introduction for students of the psychology and neurophysiology of fear in animals.

ISAAC MARKS

Measuring the Concepts of Personality. By DONALD W. FISKE. Chicago: Aldine. 1971. Pp. xviii + 322. $9.95.

Fiske presents a strictly conceptual, non-mathematical and even non-technical approach to the questions of personality measurement. The only mathematics is a brief section on indices of homogeneity (p. 153 ff.). The emphasis throughout is on the ideas and the constructs of personality: it is described as 'a textbook in the old tradition' (p. x). The argument rests heavily on an analysis of six modes of personality measurement or response. These are self-description, current experiencing, capabilities, prior behaviour, behavioural observation and psychophysiology. This is a useful scheme that facilitates a reiteration of the multi-trait, multi-method arguments. While Part 1 covers 'what is measured', Part 2 covers the operations of measurement and the procedures. A major thesis is that 'we should try to measure narrowly defined variables that can be measured by stimuli that are highly homogeneous' (p. 145), but fuller lists of references would have increased its use (especially, for example, with the effects of psychotherapy on p. 195). There is a lot of good material in this text but one wonders where it will be used outside the American university system. There is continuous repetition of the main points and it ranges between the rather elementary and the highly sophisticated. Perhaps the comment on p. 197 that the discussion of change 'is intended not to prepare the reader to conduct studies of change but rather to bring out some of the complexities' suggests its usefulness as a general introduction to an understanding of the concepts now being applied in personality measurement. But in doing this, one wonders why he kept returning to the Rorschach and to the related projective tests for his examples, and 'personology' *is* an ugly word, even if it means 'the science of personality'. The book might also be useful for those 'who are concerned with evaluating the measurement of personality in current research studies' (p. x) and for those generally interested in the logic of personality constructs.

L. B. BROWN

The Growth of Sociability. By H. R. SCHAFFER. Harmondsworth: Penguin Books. 1971. Pp. 199. 50 p.

This book, by one of Britain's foremost developmental psychologists, is an attempt to give a coherent account of early social development in humans. Professor Schaffer sets himself three questions. The first, why infants attend to people rather than to other features of the environment, is answered in terms of species-specific sensitivities to particular stimulus characteristics. Evidence is quoted from a wide range of infancy studies to support the contention that the most salient stimuli for the infant are those which are usually most strongly represented by the mother. While a need for more research is mentioned, this section gives an impression of much greater tidiness in the area than is in fact the case. In essence the answer remains a very plausible hypothesis for which there is some evidence, rather than a deduction from well-established fact. The second question is how the infant learns to distinguish between a familiar mother and unfamiliar strangers. The solution here is in terms of the development of neuronal models or schemata, an idea which seems currently to be providing common ground between theories as far apart as those of Sokolov, Kelly, Piaget and Werner. Repeated exposure to the mother in interactive situations gives rise to a differentiated schema from which strangers can be recognized as discrepant, the effectiveness of the process being dependent on the capacity of the infant to elicit social responses from the mother, and on his being presented with a precise, articulated standard. The implications for rate of acquisition of this capacity are drawn with particular reference to institutionalization. The final issue is how the infant forms a 'lasting emotionally meaningful bond with certain specific individuals'. Here the concepts established earlier are

combined with an extended analysis of attachment behaviour, based largely on the author's own work, together with additional ideas such as that in most situations recognition alone will not permit fear of strangers to appear, the onset of this phenomenon, therefore, being dependent on the development of ability to recall. In the final chapter these concepts are applied to bring out the reciprocal nature of the mother–child relationship.

Professor Schaffer has produced a theory very much in the current trend of developmental psychology, having a strong biological basis while pointing to ways in which the environment can mediate substantial variation in the end product. The argument is an interesting one, strongly and coherently stated, and must be taken seriously by anyone interested in human development during the first year of life. However, the approach leads to some deficiencies if the book is to be considered as a student text. Certainly, all of the concepts and issues one would require in a study of infancy are discussed, and the references are extensive and up to date. However, the citations are not only (and necessarily) selective, the weight given is at times distorted by the needs of the argument. For example, the statement that 'in the early weeks of life, repeated stimulation produces no response decrement' (p. 92) cannot go unchallenged when it requires that a decade of intensive study of auditory habituation be ignored on the grounds, apparently, that the findings were not corroborated in studies concerned only with vision. By contrast the early work of Fantz, despite serious methodological flaws, is taken at face value. These are matters of detail which the argument is strong enough to sustain, and the book as a whole makes stimulating reading. It is to be hoped that it will provoke both the criticism and empirical study which may lead to the elaboration and refining of its thesis.

PETER STRATTON

Self-Disclosure: an Experimental Analysis of the Transparent Self. By SIDNEY M. JOURARD. London and New York: Wiley-Interscience. 1971. Pp. xiii + 248. £4.75.

For over ten years Jourard has been arguing that disclosure of the self to others is an important element in mental health, and a psychotherapeutic weapon. This book is a collection of short reports (most of them already published) written between 1958 and 1970 by Jourard and various collaborators, giving quantitative evidence about various correlates of self-disclosure. As well as the empirical reports, there are brief linking commentaries, and a final section that includes a disclosure game (arising from one of the empirical studies, but claimed to be 'also an effective means of getting pairs of people deeply acquainted with one another in a hurry') and a plea for more transparency on the part of psychologists, whether they are acting as therapists or as experimenters.

The main aim of the book is to show that solid scientific work can be done on self-disclosure; it is addressed to people who are about to begin research, whether they have already decided to work on self-disclosure or not. Unfortunately the work reported has several features that might well put off people who are not already committed to this field.

The first point is one that has to be made about most books in social science, but is none the less important: the data are so presented that they are very difficult or impossible to reanalyse satisfactorily in any way. Yes, this *is* a polite way of saying that I sometimes suspect the author has used the wrong statistic, or misused the right statistic. But even apart from that, the customary form of presentation of data conceals far more than it clarifies.

The second point is that many of the observations are vitiated by technical weaknesses, including many of the classic ones such as very small numbers of subjects, heavy reliance on student subjects, and contamination between variables. I think the most serious weakness is the crudity of ratings of self-disclosure in the self-completed questionnaires that form the basis of about two-thirds of the studies. The only way to avoid a very discreditable-looking score is for a subject to claim: 'Have talked in full and complete detail about this item to the other person. He knows me fully in this respect and could describe me accurately' – or other phrases to a similar effect. The items vary a good deal from one study to another, but they usually range from things like 'My favourite beverage, and the ones I don't like' to things like 'What it takes to get me real worried, anxious, and afraid'. This seems to me to be asking for trouble. Jourard himself remarks that 'It could be that the thousands of questionnaires are a record of thousands of lies...' but '...at least the subjects lied with some consistency...' Well, they would, wouldn't they? It's a bit harsh to talk about lies, but those subjects haven't spent 18+ years learning their culture's norms for nothing.

But the most disappointing aspect of the work is that although Jourard mentions Lewin's famous comparison of German and American personality, he does nothing to explore Lewin's basic point that 'disclosure' involves movement across *barriers* round central regions of an individual personality. Even in Jourard's experimental studies, there is no indication that subjects really render themselves vulnerable when they disclose information about themselves. All they have to do is accept a norm set for the situation by an experimenter who is a senior member of the institution they belong to. I can see that they might be profoundly happy to accept the norm, and that it might be a genuinely enriching educational experience. (But I wonder; I wonder what is really happening when *E* says to the *j*th *S*: 'I have 20 topics I would like to discuss with you. I will first state the topic, then openly and truthfully express my thoughts and feelings regarding each of the topics....') In any case Jourard makes it clear by occasional comments that he knows that the main difficulties about self-disclosure arise when seniors or equals *don't* initiate or reward it. But in the work reported, there is no attempt to tackle this issue.

D. M. BARKLA

Black Americans: a Psychological Analysis. By E. EARL BAUGHMAN. New York and London: Academic Press. 1971. Pp. xxi + 113. £3.25.

Black and White Identity Formation: Studies in the Psychosocial Development of Lower Socioeconomic Class Adolescent Boys. By STUART T. HAUSER. New York: Wiley. 1971. Pp. xv + 160. £4.75.

The Psychological Consequences of Being a Black American: a Sourcebook of Research by Black Psychologists. Edited by ROGER WILCOX. New York: Wiley. 1971. Pp. xiii + 492. £4.75.

All three books are responses to the issues and concerns generated by what one of the authors calls the 'black revolution'. *Black Americans* is a partial spin-off from the work Baughman published with Dahlstrom in 1968, and is intended for use in introductory courses in psychology. The book is a rather superficial and uncritical review of much of the main work on the intellectual and personality characteristics of black Americans, with more detailed attention to his own studies of the psychological differences between black and white children in the rural South. He shows a degree of sympathy with the Jensen position on intelligence, dismissing criticisms that existing tests are biased against blacks as 'overly severe' without explaining why. At the same time he favours the attainment of equal opportunity for all as a means of eliminating whatever bias the tests contain but says nothing about the role of testing in sustaining inequalities. On self-esteem he quotes empirical findings – mainly Ph.D. work from his own department – that query the common view that black Americans typically develop lower self-esteem than whites. Self-esteem is defined in commonsense terms and it is not seriously questioned whether the measures used in the different studies cited are tapping the same variable. In one study the concept is operationalized as the discrepancy between self and ideal-self ratings; and the conclusion is drawn that blacks have higher self-esteem than whites, but Baughman fails to comment on the finding that black ideal-self ratings are lower than whites'. The book is not helped by the author's repeated use of often marginally relevant data to grind an axe about the need for school desegregation to be carried out more slowly.

Hauser's *Black and White Identity Formation* is a report of a longitudinal study of 23 white and black adolescent boys. Though written for the Wiley Series on Psychological Disorders it is of interest well beyond the strictly clinical field. In contrast to much work on identity and self-esteem, Hauser's concept of identity formation is theoretically firmly rooted, in the psychology of Erikson, in terms of the structural integration and temporal stability of the individual's different self-images. Furthermore, these concepts are convincingly operationalized by means of correlations between self-image (How I am now, How I will be in ten years, How I appear in the eyes of my friends, etc.) obtained by means of Q-sorts of statements elicited from each boy individually. The Q-sort data was supplemented by a range of other tests and material gathered by interview. Clinical descriptions of some of the subjects help put flesh on the quantitative data. A major finding was that while whites typically exhibited progressive identity development the

blacks showed a pattern of arrested identity formation, or identity foreclosure. In contrast to the whites, the blacks showed derogation of their origins and colour with associated self-hate, but there was no evidence of the much-postulated desire for whiteness. The blacks also had fewer positive identification figures but more anti-heroes than the whites. The book ends with a useful theoretical discussion of the pattern of black identity foreclosure in which Hauser attempts to link intrapsychic processes with socio-cultural conditions. This book is important both for its theoretical analysis of the question and for its methodological contribution. The study could bear replication on different populations, including groups in this country.

The Psychological Consequences of being a Black American is a collection of articles and papers that grew out of a lecture course given by the editor on the research and writing of black psychologists. Because a major criterion for inclusion was that the author should be black, important work by whites in the area appears only in the bibliographies, and the book is by no means the best collection on the subject that could have been made. Loosely organized under seven section headings – including cultural disadvantage, intelligence and achievement, and educational psychology – it contains empirical work and more general discursive articles from the thirties up to 1970, and concludes with a speech by Dr Martin Luther King on the role of the behavioural scientist in the Civil Rights Movement. The empirical work varies from highly specific small-scale studies of no obvious merit to more sophisticated and substantial investigations. Among the latter is the early work by Clark and Clark on the development of racial identification in black children and a 1948 article by Jenkins and Randall on the characteristics of black college students, which along with a number of others serves to document the importance of socio-cultural factors in determining psychometric test performance. Also included is William F. Brazziel's 'Letter from the South' written in reply to Jensen's *Harvard Educational Review* article. The collection is heavily dominated by attention to intelligence and education and a concern to shift the terms of the discussion away from questions of racial differences towards individual and cultural differences. For all that, apart from Brazziel's reply to Jensen, what questioning there is of prevailing American psychometric and educational assumptions is surprisingly tentative. Among the dross is a remarkable piece of patronizing ethnocentrism by Regina M. Goff entitled 'Psychology and Intercultural Interaction' in which, on the basis of a study of the values of 300 Iranian teachers, the author goes on to suggest that it would be better if the Iranian value system were more like the American one. Since this is not about black Americans, and considering that the book as a whole records the deleterious effects of the cultural derogation of one group by another, it is not clear why this jargon-ridden piece was included. Much of the material in the volume has rather specific reference to the American situation and educational system and is of little interest on this side of the Atlantic, except as more evidence of the disadvantaged position of American blacks. Even allowing for that, the book, which does contain some valuable material, would have been better had it been thinner.

PAUL HARTMANN

Psychological Modelling: Conflicting Theories. Edited by ALBERT BANDURA. Chicago: Aldine-Atherton. 1971. Pp. 210. $7.95 and $2.95.

This book consists of a 56-page introduction by Bandura, the remainder of the volume being a collection of papers by B. M. Foss, M. D. Gerst, R. F. Peterson, R. H. Walters, D. I. Slobin, R. M. Liebert, T. L. Rosenthal and, of course, Bandura.

Bandura, in his introductory 'Analysis of Modelling Processes', presents a useful review of the more important theoretical attempts to explain imitation. He looks in detail at the processes involved in modelling, constructively assessing the role of such factors as attending to the model; retention of modelled actions; motor reproduction, and motivation, with particular reference to the effects of reinforcement. He mentions in passing the fact that some observers are more influenced by particular models than by others, but considers research in the area to be limited in its generality and hence, presumably, its usefulness. Bandura takes the opportunity in this article to reply to a number of criticisms which have been directed against his experimental work – possibly the most notable being the objection to his view that behaviour formed through unique combinations of available elements represents learning, even though these elements already exist in the subject's repertoire. Regrettably he says little that has not already been said in print in one form or another.

The collection of papers which makes up the remainder of the volume is interesting as an illustration of the literature dating from 1963 to 1971; but since, with one exception, the papers are referred to reasonably comprehensively in the first section by Bandura there seems little point in including them fully.

In short, the book says very little that has not been said before, but may serve as a compact introduction to the field for the undergraduate.

B. H. KNIVETON

Introductory Statistics for the Behavioral Sciences. By JOAN WELKOWITZ, ROBERT B. EWEN and JACOB COHEN. New York and London: Academic Press. 1971. Pp. vii + 271. $8.50 and $3.95.

Practical Statistics for Non-mathematical People. By RUSSELL LANGLEY. Newton Abbot: David & Charles. 1971. Pp. viii + 399. £2.50.

Statistical Methods for Behavioral Science Research. By LEONARD A. MARASCUILO. Berkeley: University of California. McGraw-Hill. 1971. Pp. ix + 578. £6.50.

The output of new introductory texts on statistics continues to flow steadily in large volume. Why not new editions of the old textbooks? Have all the previous authors become lazy or out of date? Or is it that the books have not sold well and the publishers want to try somebody new? Be that as it may, statistics are becoming increasingly important in biology and industry and in consequence there is both an increasing number of students and of courses to provide for them. There are several reasons why new introductory texts should be issued and these can be used also to evaluate them.

Most students have the greatest difficulty in learning statistics which deal with a subject matter in which they themselves have no interest or which they do not understand. The exception to this is, presumably, in the training of professional statisticians, but students need to be familiar with the background of their subject and statistics should be introduced to them in those terms. This is particularly important in the application of statistical techniques to psychology. There are various methods of quantification in psychometrics which are in common use in psychology but which are almost unknown to professional statisticians. For example, it is customary to grade severity of symptoms in rating scales used in clinical psychology and to treat these grades as if they were true measures. Half a century of experience has shown that this produces reasonable results but it still horrifies the average statistician.

Statistical theory is developing continually with the introduction of new ideas, many of which must influence even the elements of the subject. An introductory text must obviously take into account the general educational and mathematical background of the students and as this alters, the approach to the teaching of statistics must obviously change as well. This shows itself very much in the text in the type of mathematics used and in the number and type of proofs introduced. Finally, it must be recognized that the introduction of calculating machines and computers has made the techniques of calculation very different. For example, the older introductory textbooks used to make much point of the methods of grouping data in order to make the calculation of statistics easier, but clearly this is quite unnecessary if one has good and fast electrical calculators. Now that computers have eliminated so much of the incredible amount of labour involved in, say, multivariate analysis, we are beginning to accept the value in practice of using tests of homogeneity of covariance and dispersions.

These three books range from the very simple to the more elaborate. The text by Marascuilo satisfies all the above criteria. His introduction to the theory of probability is based on set theory and is a most elegant way of doing so. The section on sampling also includes the special case of sampling without replacement, a little point which is of great value when it is wanted. There is a very good introduction to the sign test, the central limit theorem and the Gaussian distribution and also to the problems of power of statistical tests. There is much emphasis on the binomial distribution, a subject which most textbooks tend to deal with very briefly if at all. How does the author cope with some of the newer trends in statistics? He is very good in his discussion of confidence intervals and their relation to the testing of hypotheses. He deals, in analysis of variance, with problems of multiple contrasts and he gives the newer concepts of r^2 in relation to significance. Psychologists will be particularly pleased with his detailed examination

of the problem of repeated measures. It is good to see Cochran's test for equality of variance described and appropriate tables included. One section is particularly valuable: what to do with freakish scores (high or low). The author gives an appropriate test and includes the table of F. E. Grubbs for this purpose. The book is well written and the English is simple and clear. The student has enough to cope with in struggling to learn statistical ideas; he should not have to have the additional burden of trying to translate gobbledygook. One minor but useful point: the author does give good formulae for calculation of statistics which can be used with calculating machines. Against all these assets one places the introductory section. It is certainly very long and will be very difficult for students of psychology and sociology but it will be obviously insufficient for students who are reading statistics. One is therefore uncertain for whom this text would be most appropriate. One minor grumble which reflects my own experience: the author does not give sufficient emphasis to the importance of checks on the calculations of statistics.

Welkowitz, Ewen and Cohen have produced a very elegant text which satisfies all the criteria above. It too deals very well with the problems of confidence limits and also includes a section, on confidence limits of proportions, that is worth knowing. There is a very good part dealing with the evaluation of t tests and F ratios in terms of correlation and proportion of variance. The discussion on the use of two-tailed and one-tailed tests is particularly good and the authors rightly come down against the use of the latter. The section on 'power' is good and there are tables for evaluation. The authors have a couple of pages on the contingency coefficient and here they introduce Cramér's phi, which is a great improvement on the traditional contingency coefficient. Finally, the text does give warnings about proper checks of computing.

Langley's text is written at a very much simpler level. The section on the design of experiments is simple, clear and good and the explanation of interaction in analysis of variance, although long-winded, is clear and worth going through. The author rightly issues warnings about the use of one-tailed tests. He gives a number of statistical tests and tables for using them, which are not very common. Particularly noteworthy are the description and tables for Friedman's test, Fisher's test and also a 'runs test' for randomness. The description method for calculating statistics is incredibly old-fashioned and is of use only to those who have to depend solely on paper and pencil. There is no organization requiring the use of statistics which is so poor or small that it cannot afford at least a simple adding machine. The beginner will be surprised at the equipment he will find if he makes friends in the accounts department. An introductory text should give some information about formulae which will economize labour. The book is light-hearted in style (or horribly facetious if you do not like that sort of thing) and well written except for one painful exception. The author uses 'data' as a singular noun, but although he defends this on the grounds that it is now being commonly used as such, I am forced to say 'Dr Langley, you should not follow in the footsteps of them people who do not know 'ow to use English'.

Some minor grumbles: why do textbooks of statistics always take two bites at the cherry when giving the list of tables? It is annoying, in looking up the list of contents, to find that the list of tables has to be found on another page; there is no reason why they should not be included in the list of contents in detail. The Welkowitz, Ewen and Cohen book disguises the list of tables as 'Appendix', and Langley has neither a list of tables nor an index. In summary, the Langley is a useful little introduction to the subject but the beginner will have to go further. The Welkowitz is a first-rate text which covers everything that most people will need. For those who would like to have accessible some of the background of theory, even if they don't need it at the moment, nothing could be better than the Marascuilo.

MAX HAMILTON

Reading and its Difficulties: a Psychological Study. By M. D. VERNON. Cambridge University Press. 1971. Pp. 211. £3.20.

Since Professor Vernon's book *Backwardness in Reading* was published in 1957, there has been an explosion of interest in the study of reading difficulties and a wealth of research material has accumulated which needed to be sifted and collated. This Professor Vernon has done and has produced, in one small volume, a source book which will be of enormous value to research workers, teachers and students.

She examines four main types of psychological process involved in reading – visual perception, auditory/linguistic perception, intellectual and motivational processes – and concludes that de-

fective functioning in any one of these areas may result in learning difficulties. Some teachers having read Professor Vernon's comments may feel that many children, deficient in one or more of the basic cognitive abilities, might benefit from a postponement of formal instruction in reading until the age of six years or even later. Many experienced teachers, however, have become disenchanted with the concept of reading readiness and have observed that children may be even more frustrated and discouraged by delay. It is possible that an earlier introduction of language experience, an improvement in grading and content of reading material and a modification of methods to suit individual needs, would be a more productive approach in the early stages.

The interaction of the child and his parents in the pre-school period is of fundamental importance in the acquisition and exercise of basic skills and in the development of motivation. At school, effective learning depends largely upon the teacher's relationship with the child. Professor Vernon points out that children with poor motivation and with lower degrees of cognitive ability are the most likely to be affected by unsatisfactory methods and by unskilled teaching. Such children form a high proportion of backward readers in primary schools and yet only brief consideration is given to the effects of teaching methods and of the teacher–child relationship on the child's interest and motivation in learning to read.

Teachers have long felt that most children need some 'phonic teaching' by the end of their first year in the infant school. Professor Vernon describes interesting research in this area which should help teachers to work out a systematic scheme of phonic training so that children will more easily be able to associate printed letters with the sounds in spoken words. Her account of investigations into the possible existence of an independent dyslexic syndrome will be of great interest to many readers.

In *Backwardness in Reading* Professor Vernon refused to use the term dyslexia which had been originally introduced by neurologists to describe a specific reading disability in children of normal intelligence. At that time the term seemed to suggest some cortical injury or deficiency, comparable to that of alexia, but not demonstrable as organic damage or defect.

Many educationists still do not accept that specific developmental dyslexia exists except in those very few cases where damage can be demonstrated neurologically, but Professor Vernon now states that evidence would seem to be adequate to establish the existence of a disability, specific, constitutional and genetically determined in at least some backward readers. She regards the disability as not purely perceptual; difficulties are experienced by some backward readers in comprehending the correspondence between spatially ordered visual sequences and temporally ordered linguistic sequences. This constitutional disability may be mild and easily overcome but in severe cases there may be prolonged difficulty in spite of individual attention and improvement in environmental conditions.

Professor Vernon discusses very fully the evidence for the existence of a dyslexic syndrome and recognizes that there is considerable disagreement concerning the significance of the evidence.

Much of the research has been carried out with very small samples; the diagnosis of dyslexia has been made by doctors, clinical psychologists, educational psychologists and sometimes by the parents themselves. The establishment of Dyslexic Associations by parents is certainly not evidence of a widespread incidence of the condition.

Practising teachers may well feel at this stage that although the recognition of a basic disability in a small number of cases is interesting, it has only marginal relevance to the task of dealing with large numbers of children with reading difficulties in the classroom. Many children have difficulty in reading; a minute proportion can properly be called dyslexic. The introduction of an originally neurological term into an educational context may in fact already have had an adverse effect on the motivation of the teacher.

Of far greater importance would seem to be a more serious national concern for teacher training in the skills of teaching reading and in remedial work with children who, because of poor linguistic development, lack of early stimulation and indifferent motivation, are recognizable on entry to school as likely to experience difficulty.

Professor Vernon's admirable book is recommended to everyone concerned with the teaching of reading; it is a pity that the index does not do justice to this important contribution to the subject.

JOHN BRADLEY

Design and Analysis in Educational Research. By G. E. R. BURROUGHS. School of Education, University of Birmingham. 1971. Pp. xii + 327. £2.00.

Professor Burroughs has modest aims for this book which was written to meet the needs of experienced teachers undertaking courses of study in education and 'to give them something on which they might lean'. Despite the author's unpretentious claims, the ground covered by the text is considerable. In all there are 34 chapters dealing with a range of topics, including the application of scientific modes of inquiry in educational research, setting-up hypotheses, designing experiments, sampling, data collection by a variety of instruments, hypothesis testing and inferential statistics. There are, in addition, an up-to-date treatment of simple designs for the analysis of variance, sections dealing with different types of correlation and factor analysis, and a mathematical appendix whose content suggests that the writer takes little for granted from his readers. The book also contains advice on topics such as 'getting started' and 'writing the report'. With such a coverage it was inevitable that depth should be sacrificed to breadth (the questionnaire, for example, receives only two-and-a-half pages) but Burroughs has provided a useful introductory and annotated bibliography – composed mainly of books considerably more expensive than his own – to extend the treatment he has presented.

Unlike similar books emanating from the USA, Burroughs adopts an economic style which successfully communicates the tough-minded ethos of this particular brand of scientific approach. He makes several references to the perennial difficulties in educational research and cautions against the retreat from worthwhile problems into methodology so often followed by workers in their search for scientific respectability. Whilst several of his examples and literary asides are amusing, it is difficult to see why bizarre illustrations are preferable to actual examples of experiments conducted in educational and psychological research. One imagines that Burroughs's aim was to provide light relief for the reader.

Bearing in mind the characteristics of his audience, the writer has postponed even minimal numerical treatment and symbol manipulation until comparatively late in the book. The order of presentation and content dictated by this principle, however, appears to be a limitation, though one assumes from the introduction that generations of students have helped to pilot previous versions of the text. Thus, extensive reference is made to the concept of variance in the early chapters long before a value for this statistic is computed. It is at least arguable that students attain an operational concept of variance more readily through the working-out of examples contrived to demand limited arithmetical competence than by a consideration of differences between groups of children in unquantified shades of orange as in the hypothetical experiment reported on pages 24–26. Similarly, concepts of standard error, confidence limits and the like are dealt with in advance of the characteristics of the normal curve. Nevertheless, a strong feature of the book is the careful cross-referencing aided by a coded segmented format which enables the reader to consult a relevant and more detailed section.

Other commendable aspects of this book include a consistent use of symbols and an obvious intention by Burroughs that the reader should understand the logic of inquiry. This reviewer was, however, able to detect a number of arithmetical and typographical errors. In a proliferating market of books on similar topics this one seems to stake a claim for consideration on the grounds of coverage, clarity and price. It should prove of value to students laying some of the foundations necessary for the appraisal and conduct of research, to teachers wishing to gain insight into the present state of the art and for beginning students of psychology prepared to face the risks involved in the investigation of real-life situations.

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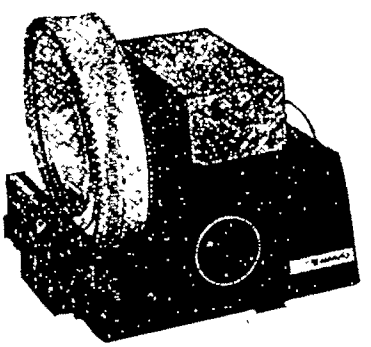

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Publication
Quarterly, February, etc.

Subscription
Volume 24, 1972: UK £6.50, overseas £7.50; plus 75p postage

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Volume 63, Part 4 *November 1972*

THE BRITISH JOURNAL OF PSYCHOLOGY

Edited by W. SLUCKIN
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AMERICAN BRANCH:
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Annual subscription (including postage) £7.50 net ($25.00 in the U.S.A.).

Second class postage paid at New York, N.Y.

Published by Cambridge University Press, Bentley House, 200 Euston Road, London, NW1 2DB and 32 East 57th Street, New York, N.Y. 10022.

Br. J. Psychol. (1972), 63, 4, *pp.* 497–508
Printed in Great Britain

HEMISPHERIC ASYMMETRIES IN TACHISTOSCOPIC INFORMATION-PROCESSING

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A considerable number of experiments, reported since 1965, has been concerned with cerebral hemisphere differences and the processing of tachistoscopic visual information. The present paper critically examines the findings from laboratory experiments with normal *S*s and from clinical experiments with brain-damaged patients. Three general conclusions are reached from this analysis: (1) Much of the available evidence on tachistoscopic laterality differences and hemispheric asymmetries is ambiguous, equivocal, and inconsistent; this is attributable to inadequate methodological considerations and misinterpretations of response indicators. (2) The reliable evidence suggests the hemispheres differ in efficiency to initiate specific responses and in ability to analyse/encode visual information. (3) Future research into hemispheric determinants should supplement traditional recognition-accuracy indicators with non-vocal, non-identificatory measures in order to separate response-initiation from visual-analysis effects.

When stimuli are tachistoscopically exposed in the left visual field (LVF) or in the right visual field (RVF) recognition is seen to be more accurate sometimes for the LVF stimuli, sometimes for the RVF stimuli. The direction and magnitude of the visual laterality difference is a complex function of many factors including acquired reading habits, stimulus presentation procedures, and dominances of eye, hand, and hemisphere (White, 1969*a*). Psychologists have interpreted visual laterality differences through known functional asymmetries in speech, language, and other cognitive abilities; generically these have been called *cerebral dominance* interpretations (e.g. Barton *et al.*, 1965; Kimura, 1963, 1966, 1969; Koetting, 1970; McKeever & Huling, 1970*a*, *b*, *c*). Psychologists and neurologists have also employed the tachistoscopic paradigm as a means of teasing-out hemisphere differences in the processing of stimulus materials (e.g. Dorff *et al.*, 1965; Kimura, 1963; Warrington & James, 1967*a*, *b*).

A large number of experiments has been reported in the last six years which are relevant to an understanding of how the cerebral hemispheres differ in their handling of rapidly presented visual information. There is a twofold purpose for the present paper: (1) To analyse these experimental findings, for their reliability and consistency with hypothesized functional processes, and (2) to synthesize the results from four lines of inquiry in order to clarify the role of cerebral dominance in tachistoscopic information-processing. The four lines are: laboratory experiments employing traditional recognition-accuracy indicators, clinical experiments with brain-damaged patients, research with brain-bisected patients, and laboratory studies employing reaction time and non-vocal accuracy indicators.

All tachistoscopic laterality effects cannot be interpreted by hypotheses based on functional differences. There are two stimulus presentation procedures commonly used: In one the stimuli are presented both sides of a fixation point (bilateral presentation) and in the other the stimuli are presented either to the left or to the right of a

fixation point (unilateral presentation). A LVF recognition superiority has commonly been found for bilateral presentations of words, letters, digits, and binary stimuli. Varying the structural characteristics of the stimuli, report-order instructions, stimulus redundancy, and inter-element spacing, can alter the direction and magnitude of the laterality difference. Explanations based on dominance factors are unable to account for and predict these changes of direction and magnitude, but interpretations which emphasize the roles of acquired reading habits, hemiretinal training, and the manner in which the stimuli are scanned post-exposurally, are able to. Research relating the effects of stimulus characteristics, report-order, and experimental instructions to laterality differences, has been interpreted as evidence for the operation of a *post-exposural directional trace-scanning mechanism* and has been reviewed elsewhere (see Harcum, 1972; White, 1969*a*).

Trace-scanning hypotheses are also able to accommodate the typical observation that unilaterally presented English words are recognized more accurately in the RVF than in the LVF. Cerebral dominance has been viewed as a likely determinant of this perceptual effect, but again this interpretation suffers in that it cannot account for changes in the laterality difference consonant with stimulus and experimental variations (see Harcum & Finkel, 1963; White, 1969*b*). There are, on the other hand, considerable data not readily explained by any trace-scanning hypothesis which are relevant to hypotheses based on cerebral functional asymmetries. These have usually arisen in studies employing single stimuli and unilateral presentations.

Two related and converging lines of inquiry into hemispheric asymmetries are from dichotic listening and dichoptic recall experiments. Discussion of these has been omitted from the present paper because they fall outside the purview of the tachistoscopic paradigm. Typically, dichotic and dichoptic studies involve relatively long stimulus-presentation periods of the order 3 to 6 sec. and the use of competitive inputs, to both ears, both eyes, temporal hemiretinae, etc. White (1969*a*) has commented on some of this research and its relationship to tachistoscopic laterality differences.

BINOCULAR RECOGNITION

Table 1 summarizes the findings from 41 experiments which have used unilateral stimulus presentations. It can be seen there is little consistency among studies employing *non-verbal* stimuli; 15 of the 25 experimental results showed no hemisphere/hemifield differences in recognizing these materials. Some authors (e.g. Kimura, 1963) have viewed the absence of a laterality difference for geometric and nonsense forms as indirect support for a dominance hypothesis. It has been argued that because these stimuli are non-verbal (improbable in the case of geometric forms) their recognition should not be facilitated by a prepotency of the left, language-dominant hemisphere (Kimura, 1963, p. 269). Non-significant results may, on occasions, have resulted from poor psychophysical procedures which in turn may have obscured reliable effects. In three experiments, Kimura (1969) found dot detection to be as accurate in the LVF as in the RVF. Her procedure left much to be desired. For example: '*S* simply reported whether he had seen a dot or not. No blank cards were given, each card was presented only once, and it was explained the *S* must report "yes" only when he was certain he had seen the dot.... The score for each field was the number of "yes"

Table 1. *Laboratory experiments showing hemisphere–hemifield differences: binocular recognition*

Stimulus	Superior hemisphere: left / Superior hemifield: right	Superior hemisphere: equal / Superior hemifield: equal	Superior hemisphere: right / Superior hemifield: left
Non-verbal			
Contour discrimination (White, 1971*b*)	1	—	—
Dot detection (Kimura, 1969)	—	3	—
Dot enumeration (Bryden*; Kimura, 1966)	—	2	2
Dot figure reproduction (McKeever & Huling, 1970*c*; White & Barr-Brown, 1972)	—	1	1
Dot localization (Kimura, 1969)	—	—	4
Form enumeration (White, 1971*a*)	—	2	—
Form recognition (Bryden, 1960, 1964; Bryden & Rainey, 1963; Heron, 1957; Kimura, 1966; Terrace, 1959)	—	7	—
Light fragmentation (McKinney, 1966, 1967)	2	—	—
Verbal			
Drawing identification (Bryden & Rainey, 1963; Wyke & Ettlinger, 1961)	2	—	—
Letter enumeration (Kimura, 1966; White, 1971*a*)	1	1	—
Letter recognition (Bryden, 1964, 1965, 1966, 1968; Bryden & Rainey, 1963; Hayashi & Bryden, 1967; Heron, 1957, White, 1971*b*; Zurif & Bryden, 1969)	9	3	—

* Unpublished data; personal communication.

reports for that field' (Kimura, 1969, p. 453). Wyke & Ettlinger (1961) adopted a modified method-of-limits to obtain thresholds, a procedure which Harcum (1972) has criticized. It is possible to explain the results from two of Kimura's experiments (1969) in terms of relative stimulus refraction and the properties of planar tachistoscope mirrors. In two other experiments where this problem was obviated, the mean LVF superiorities for dot localizations were only 1·4 and 1·8 of a possible maximum of 24.

Some of the findings summarized in Table 1 may have little interpretative value because of these deficiencies in design and procedure. The observation of McKeever & Huling (1970*c*) that dot figures were better reproduced when shown in the LVF has not been made by White & Barr-Brown (1972). The former gave no fixed criteria to judges to rank one hemifield drawing better than the other hemifield drawing and their dot figures were not bilaterally symmetrical in both hemifields. The latter experimenters employed several fixed criteria and the configurations were all symmetrical. There is also the problem of what to make of experiments which fail to reject the null hypothesis. Fifteen of the 25 non-verbal results shown in Table 1 reported no difference in accuracy of recognition for stimuli presented in the LVF and in the RVF.

There was slightly more consistency among studies using stimulus material which was more readily verbally encoded; no one, for example, has reported a left hemifield superiority in the recognition of single letters. The relationship of these results to cerebral dominance is, however, not clear. White (1971*b*) found a significant correlation in left-right differences between unilateral letter recognition and the discrimination of line orientations. The orientation of a single line was better discriminated when the stimulus was shown in the RVF. It is difficult to explain this on the basis of a

superiority of the left hemisphere for *language processes* but it might suggest that for specific visual tasks such as contour discrimination, stimuli in the RVF are favoured by a selective contour-tuning mechanism operating at a peripheral-retinal or central stage in the visual system.

MONOCULAR RECOGNITION

In an effort to tease-out eye, acuity, and hemiretinal effects, and those effects attributable to the superiority of crossed and uncrossed optic pathways, studies have been conducted wherein stimuli were presented monocularly to each eye. A majority of these studies has employed letter-string and/or word stimuli and their results therefore have questionable value as regards hemisphere functioning. Three general findings have arisen from monocular experiments (Crovitz & Schiffman, 1965; Hirata & Osaka, 1967; Koetting, 1970; Markowitz & Weitzman, 1969; McKeever & Huling, 1970*a*, *b*; Neill *et al.*, 1971; Overton & Wiener, 1966): (*a*) Stimuli presented to the left eye were recognized as accurately as when presented to the right eye; (*b*) stimuli presented to the left temporal hemiretina were most accurately recognized; (*c*) a RVF superiority was consistently demonstrated.

Left hemisphere language-dominance has been used by Barton *et al.* (1965), Koetting (1970), and McKeever & Huling (1970*a*, *b*) to account for the monocular RVF superiorities but of these studies only that by Barton *et al.* did not employ horizontally mounted letter-strings. Barton *et al.* (1965), Bryden (1970), and Goodglass & Barton (1963) have used vertically aligned stimulus arrangements in order to eliminate or reduce trace-scanning effects. But even here, observed RVF superiorities may not have provided reliable indicators of superior left hemisphere functioning. In the Barton studies the stimuli were displaced less than 2·5 deg. from fixation. There is the possibility that 3–5 deg. of central vision are bilaterally represented at the cortex and that stimuli displaced by less than approximately 2·5 deg. from fixation are transmitted to both visual receiving areas (Adler, 1965; Ruch *et al.*, 1965; Wolf, 1968). The very few monocular studies which have employed *single* stimulus forms have shown equivocal results. No laterality differences have been found for light discrimination (Wyke & Chorover, 1965), and random shape and single letter recognition (Hirata & Osaka, 1967). The results for Landolt C discrimination are inconsistent (Hirata & Osaka, 1967; Markowitz & Weitzman, 1969). McKinney (1967) has, however, shown that lights perceptually fragment more rapidly when presented in the LVF.

UNILATERAL LESIONS

Psychologists who favour cerebral dominance as an explanation of visual laterality differences make use of several criterial findings to validate the explanation. The argument that superior LVF dot localization is determined by a superior spatial coordinate system within the right hemisphere is presumed validated by the positive clinical correlation between right hemisphere functioning and non-verbal perception (Kimura, 1969). Similarly, the argument that superior RVF letter recognition is determined by the superiority of the left hemisphere for language processes is presumed validated by the clinical correlation between left hemisphere functioning and language disorders (McKeever & Huling, 1970*a*).

Table 2. *Clinical experiments comparing effects of left and right hemisphere damage in recall and recognition*

Stimulus	Hemisphere (damaged) showing lesser impairment		
	Left	Equal	Right
Non-verbal			
Closure (Lansdell, 1968, 1970)	2	—	—
Dot detection (Warrington & James, 1967*b*)	1	—	—
Dot enumeration (Kimura, 1963; Warrington & James, 1967*b*)	2	—	—
Faces (Milner, 1968)	2	—	—
Meaningless figures (Boller & De Renzi, 1967; De Renzi, 1968; Kimura, 1963)	2	—	1
Nonsense forms (Kimura, 1963)	1	—	—
Visual construction (Lansdell, 1970)	—	1	—
Visual fragmentation (De Renzi & Spinnler, 1966; Meier & French, 1965; Warrington & James, 1967*a*)	3	—	—
Verbal			
Familiar figures (Boller & De Renzi, 1967; De Renzi & Spinnler, 1966; Dorff *et al.*, 1965)	1	3	1
Familiar objects (De Renzi & Spinnler, 1966; Kimura, 1963)	—	1	1
Letters (Dorff *et al.*, 1965; Kimura, 1963; Warrington & James, 1967*b*)	1	2	—

Table 2 shows summaries from 25 clinical experiments where the results have been ordered according to the nature of the stimulus materials and the locus of brain damage. The right hemisphere classification included patients with lesions in the right temporal, occipital, and parietal areas; the left hemisphere classification included patients with lesions in the left temporal, occipital, and parietal areas. While the classification in Table 2 is somewhat arbitrary, one pattern is clear. Of the 15 experiments included under the non-verbal stimulus classification, 13 showed recognition and recall to be impaired less by left hemisphere damage than by right hemisphere damage; one experiment showed no effects attributable to locus of lesion and one experiment showed lesser impairment for damage confined to the right hemisphere. This constitutes fairly conclusive evidence that right hemisphere damage is selectively involved in impairment on a broad class of non-verbal perceptual tasks.

The results for verbal stimuli were less clear-cut. In the present analysis, two studies showed recognition and recall of verbal material to be impaired less by left hemisphere damage, two showed lesser impairment from right hemisphere damage, and six studies showed inconclusive results. If left hemisphere dominance for language can be invoked by experimental psychologists (e.g. McKeever & Huling, 1970*a*, *b*) to explain superior RVF tachistoscopic letter recognition, should not damage to the left hemisphere produce large and consistent verbal recognition impairments relative to right hemisphere lesions?

Two points are involved in answering this question. One has to do with the adequacy of clinical experiments in a methodological sense, and the other has to do with the conception of cerebral dominance as a simple unitary phenomenon. Clinically, poor methodology and analytic procedures are responsible for much of the confusion. To take one example: Warrington & James (1967*b*) tested groups of left and right lesioned patients on three tasks – letter recognition, dot detection, and dot enumera-

tion. Patients with right-side brain damage were found to be impaired on all tasks relative to left brain-damaged patients, but in what specific way was not clear from the data. The dot detection and letter recognition results were combined for analysis notwithstanding psychophysical differences and the possibility that the tasks might be subserved by different functional mechanisms (cf. Kimura, 1969). All results were compared against those from a control group of fifteen patients with peripheral nerve lesions who showed a LVF superiority on the combined detection and letter recognition tests – opposite to that commonly reported for letter recognition with normal subjects (e.g. White, 1971*b*). The statistical treatment involved 48 separate tests ($k = 2$) thus obscuring interaction effects.

The experimental psychologist has himself often taken a simplistic view of hemisphere function. Three assumptions are commonly made in formulating a dominance interpretation of results: (1) Stimuli can be tachistoscopically presented in the LVF or in the RVF provided fixation is maintained by the subject and the exposure is below eye-movement latency. (2) Stimuli shown in the RVF are transmitted to the left visual receiving area and those in the LVF to the right visual receiving area. (3) The dominance of a hemisphere for the 'processing' of particular stimulus materials will determine a contralateral visual hemifield superiority in recognition. The first assumption is often not tenable on the grounds of improper stimulus presentation procedures (cf. Geffen *et al.*, 1971; Moscovitch & Catlin, 1970). The second assumption is often violated by the use of minimal stimulus eccentricities (see under Monocular Recognition, above). The third assumption is vague. A simple unitary conception of hemisphere dominance is unwarranted; the locus and extent of brain damage, the presence of dysphasic symptoms, the period between operation and testing, and the testing procedure itself, all vary from clinical study to clinical study. It is highly unlikely, given the intricacies of cortical functioning, that damage to the left hemisphere *per se* will result in a consistent letter recognition deficit and in this respect psychologists have overgeneralized their validating data.

To summarize: Right hemisphere damage appears to be implicated in the recognition and recall of a broad class of non-verbal stimuli. Milner (1968) has suggested that lesions to the right hemisphere will cause an 'impairment on all tasks using non-verbal patterned stimuli, regardless of whether these are abstract "nonsense" designs or representational patterns such as snapshots of human faces' (p. 206). This might lead one to expect an LVF superiority with such materials in normal subjects. The relationship between left hemisphere damage and verbal recognition deficit is not clear. There are few *clinical* findings to validate the experimental conclusion that, for example, 'there is a dominancy of the left cerebral hemisphere with respect to the recognition of words' (Koetting, 1970, p. 65).

BRAIN BISECTION

The roles of the cerebral hemispheres in tachistoscopic recognition have been clarified by the research of Gazzaniga, Sperry, and their associates, working with commissurotomized patients (Gazzaniga, 1967, 1968; Gazzaniga *et al.*, 1965; Gazzaniga & Sperry, 1967). Two salient findings emerged from these studies. (1) When words and pictures were tachistoscopically exposed in the RVF the brain-bisected patient

was able to make accurate vocal and written identificatory responses; when the stimuli were exposed in the LVF – and the stimulus information transmitted to the disconnected right hemisphere – the patient was not able to make vocal and written responses. (2) If the patient was allowed to make non-verbal responses, such as matching the exposed stimulus to one of several objects, 'identification' accuracy was seen to be nearly as good for stimuli exposed in the LVF as for those exposed in the RVF. These observations point up a critical difference in hemisphere functioning: While the left hemisphere may be 'dominant' in speech and language output processes (in the initiation of speech and language responses) both hemispheres may be capable of analysing and encoding verbal stimulus material. Few clinicians and psychologists have bothered to separate these components, typically employing vocal or written identificatory response measures, and creating a confounding of response with analysis determinants.

The full implications of brain-bisection research are yet to be reported but corroboration of the preliminary findings will necessitate a change in the conception of cerebral dominance as a determinant of tachistoscopic laterality differences: The superior RVF recognition for verbal stimuli probably reflects the left hemisphere's ability to initiate vocal response more than its ability to analyse verbal stimuli. One further important result from this research supports the previously quoted comment from Milner (1968). Brain-bisected patients appeared to show superior non-verbal visuospatial and visuoconstructive ability using their left rather than their right hand, i.e. the right hemisphere was demonstrably superior to the left in these tasks (Gazzaniga, 1967; Gazzaniga *et al.*, 1965).

Reaction time

In the majority of studies discussed so far, vocal recall and recognition has been used as the psychophysical indicator. Interest is currently being expressed in an independent indicator of interhemispheric processes – reaction time. As with many clinical and laboratory experiments, reaction time experiments have suffered from procedural and design inadequacies making for difficult interpretation. Filbey & Gazzaniga (1969) presented stimuli within 2 degrees of fixation while Egeth (1971) presented stimuli within 1 degree of fixation; in both cases bilateral stimulation may have resulted. Moscovitch & Catlin (1970) criticized an earlier experiment on this point but committed a procedural error themselves by presenting blocks of trials exclusively in one hemifield thus allowing the development of fixational biases (cf. White, 1969*c*). Klatzky (1970) compounded this error by using supralatency exposure durations of 400 msec. Reaction time findings sometimes disagree with those from the more conventional recognition experiments. Kimura (1969) observed dots were detected equally well when presented in either hemifield; Filbey & Gazzaniga (1969) found vocal reaction times to the presence or absence of dot stimuli to have lower latencies for dots presented in the RVF.

Five methodologically sound experiments have been reported by Geffen *et al.* (1971). They observed manual reaction times (same–different) to ambiguous face stimuli had significantly lower latencies when faces were presented in the LVF; vocal reaction times (yes–no) showed no difference in latencies for faces presented in the

LVF and RVF. Reaction time latencies for the vocal identification of digits tended to be significantly lower for digits presented in the RVF but were similar for non-identificatory vocal responses whether digits were presented in the LVF or RVF.

RESPONSE AND ANALYSIS PROCESSES

The results of Geffen *et al.* (1971) agree with those of Milner (1968) and others in showing the right hemisphere superior for processing non-verbal stimuli (Table 2). Specifically, their results show that this processing has little to do with output factors and everything to do with efficiency of *analysing* the stimulus information. If it is assumed that both hemispheres are equicapable of initiating manual motor responses then the faster manual responses to LVF faces presumably reflect hemispheric asymmetries in efficiency of information analysis. The left hemisphere's function seems more related to response-output determinants. Geffen *et al.* have hypothesized that face stimuli (the traces thereof) arriving at the right hemisphere are rapidly and efficiently analysed but the stimulus excitation must cross the corpus callosum to the left hemisphere for initiation of vocal responses (see Teitelbaum *et al.*, 1968). Face stimuli arriving at the left hemisphere are less efficiently analysed but this hemisphere is capable of rapidly initiating vocal responses. A mutual cancellation of the effects due to efficiency of information-analysis and efficiency in initiating identificatory vocal responses results in negligible visual field reaction time differences for *vocal* responses. This hypothesis accords with their own data and with the results from many experiments employing more conventional indicators. No laboratory study has as yet reported reliable visual field differences in the recognition of non-verbal stimuli (Table 1). These null findings might be suggestive of a functional symmetry in the *analysing* of non-verbal stimuli. Alternatively, they might reflect the confounding of a true functional asymmetry in the analysis of non-verbal material with identificatory vocal output processes.

An understanding of how the hemispheres process visual information demands a consideration of both analysis and response determinants. It is clear from the work with commissurotomized patients that because the right hemisphere is incapable of eliciting identificatory vocal responses it is not incapable of analysing ('identifying') verbal material. Similarly, because the left hemisphere is usually prepotent for language and speech processes and initiation of vocal motor responses, it does not necessarily follow the same hemisphere is prepotent or 'dominant' for stimulus analysis. In an unpublished experiment from this department, subjects were required to rapidly respond 'same' or 'different' to identical or different pairs of letters randomly presented in the lateral visual fields. A recognition-accuracy indicator was employed and while significantly more discrimination errors were associated with identical-pair stimuli no visual field differences were observed. Collectively these findings suggest the following: (1) the right hemisphere is more efficient in analysing non-verbal stimuli or classes of stimuli which are not immediately verbally encodable; (2) both hemispheres are capable of analysing verbally encodable stimuli; (3) the left hemisphere is more efficient in initiating identificatory vocal responses; (4) the right hemisphere is capable of initiating crude non-identificatory vocal responses (cf. Butler & Norrsell, 1968).

Handedness and eyedness

Discussion of indirectly relevant stimulus parameters has been minimized in this paper because of their minimal role in tachistoscopic laterality research. Handedness has often been considered a critical determinant of laterality differences, primarily due to its presumed relationship with cerebral dominance. Milner *et al.* (1964) have pointed out, however, the majority of right- and left-handers are *left*-brained for speech. The psychometric assessment of handedness is unsatisfactory (Satz *et al.*, 1967) and recently Zurif & Bryden (1969) observed, 'non-familial left-handers are as strongly left-hemispheric dominant as are right-handers' (p. 185). Eye dominance, as measured by sighting tests, has been shown unrelated to the direction and magnitude of visual laterality differences (Hayashi & Bryden, 1967). Again, there is little evidence suggesting exposure duration and luminance have any critical effect on laterality differences providing exposure durations above eye-movement latency are not used. These statements are qualified by reference to unilateral single-stimulus studies.

Conclusions

This evaluation of tachistoscopic laterality research allows for three general conclusions relating to the status of present work, the role of the hemispheres in tachistoscopic recognition, and directions future research might take.

(1) The principal reason why much of the experimental evidence is inconclusive, ambiguous and equivocal is that many experiments have been poorly designed, poorly executed, and the results poorly analysed; these features are common to clinical and laboratory research. To enumerate some of the typical faults: (*a*) poor psychophysical methodology and an almost total lack of concern with subjective judgemental behaviour; (*b*) lack of specification of stimulus parameters – the characteristics that define *verbal* and *non-verbal* information; (*c*) in laboratory experiments, the use of averaging statistics and the disinterest with individual performance; (*d*) the use of multiple-letter stimulus displays, supralatency exposures, minimal retinal eccentricities for stimulus placement, and single hemifield trial-blocks.

(2) Cerebral dominance, as talked about by experimental psychologists, is anything but a simple concept and to explain right hemifield superiorities for verbal recognition in terms of the superiority of the left hemisphere for *language* processes is to beg many questions. It does seem as if both left and right hemispheres have the capacity for analysing and encoding visual information which might broadly be termed verbal but that the left hemisphere is responsible for the initiation of spoken and written identificatory responses. The right hemisphere appears superior to the left in a variety of 'non-verbal' tasks involving visuospatial abilities. Studdert-Kennedy & Shankweiler (1970) have recently suggested interhemispheric differences in response initiation and analysis processes for dichotic speech perception: 'specialization of the dominant hemisphere in speech perception is due to its possession of a linguistic device, not to specialized capacities for auditory analysis...while the general auditory system common to both hemispheres is equipped to extract the auditory parameters of a speech signal, the dominant hemisphere may be specialized for the extraction of linguistic features from those parameters' (p. 579). The term

'cerebral dominance' has arguably outlived its usefulness and adds more confusion than clarity to discussions of hemispheric asymmetries in information-processing.

(3) The most important recommendation for future research is the separation of analysis-encoding indicators from response-output indicators. Until we can be sure that observed tachistoscopic laterality differences *do not* reflect hemisphere differences in efficiency of response initiation, we shall not be able to meaningfully discuss functional differences in information analysis. To this end, laterality research must employ response measures which are not left hemisphere-dependent.

Preparation of this paper was supported by a grant (70/51) from the University Grants Committee.

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(*Manuscript received* 21 *October* 1971)

Br. J. Psychol. (1972), **63**, 4, *pp.* 509–514
Printed in Great Britain

PROCESSING IN PERCEPTUAL INTEGRATION BETWEEN AND WITHIN THE CEREBRAL HEMISPHERES

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Two experiments are described in which, by a divided visual field technique, simple non-verbal stimulus material is directed to either the right hemisphere, left hemisphere, or divided between the hemispheres. It is found that when material to be matched is shared between the hemispheres, response latencies are reduced, suggesting increased capacity for the processing of such information and supporting the hypothesis that each hemisphere contains its own processing and analysing system. When the material is directed to only one hemisphere, and a bimanual motor response required, a relative superiority of the right hemisphere is found. If the response required is a vocal response, then further relative blocking of the processing undertaken by the left hemisphere is observed.

Gazzaniga & Young (1967) found that the split-brain monkey can process and respond to more information involving bimanual motor sequencing than can controls with their commissures intact. There is considerable evidence also that human commissurotomy patients can carry out double voluntary reaction time tasks as fast as they can carry out a single reaction (Gazzaniga & Sperry, 1966). This suggests not only that each hemisphere possesses the necessary equipment to analyse and to register signals as well as to organize responses, but also that in commissure-linked hemispheres something of this additional capacity could well remain.

Studies employing spreading cortical depression in animals suggest widespread integration of function between one hemisphere and the other. Burešová *et al.* (1966) implanted different memory traces in each hemisphere in rats, the left hemisphere containing the memory of a task which required the animal to alternate performance in a T maze, and the right hemisphere containing the memory of a task in which the animal was given an electric shock terminated by jumping on to a platform. The animals, tested with both hemispheres intact, were tested in a situation combining both tasks, and found to be superior in performance to animals trained on both tasks using only one hemisphere, or to normal animals trained on both tasks. This suggests that while each hemisphere has the capacity to learn and remember, the possession of two such systems may be better than one, the limits of intellectual capacity and information load being closely related to the joint action of both.

The possibility that two such systems are better than one and that cooperative interaction is a customary mode of brain function in the normal human brain has been investigated using a divided visual field technique. When two simultaneous signals for response are distributed between the hemispheres, reaction times are faster than when they are both projected to the same hemisphere (Dimond, 1970). Total output of the brain is increased whilst performing two tasks, if the perceptual load of a visual task is divided between the hemispheres instead of directed to one hemisphere alone (Dimond & Beaumont, 1971). When two signals are projected simultaneously to a single hemisphere, blocks to function are observed. This is true not only of reaction time tasks but also of verbal tasks (Dimond, 1971). This suggests

not only that each hemisphere possesses the capacity to analyse the information which enters it, but also that it proceeds some way along the chain of analysis before distributing information load to the other hemisphere.

When the ability to relate perceptual events is considered, these effects may be revealed on tasks which demand the combination of elements to form a composite figure. If part of a figure is projected at the same time as part of the same or a different pattern, the subject can indicate whether the components are part of the same or a different figure. The advantage in the use of two hemispheres can be demonstrated by projecting a component to one hemisphere and the other component to the other.

Levy-Agresti & Sperry (1968) suggest that the right hemisphere with its grasp of spatial relationships apprehends events in a Gestalt fashion whereas the left carries out a sequential analytic procedure. They quote evidence from split-brain patients that the left hemisphere succeeded on a task which required sequential analysis, while the right succeeded where spatial relationships could be readily grasped. If such a relationship exists in the normal human brain, a more rapid grasp of the relationship between different parts of a composite figure should be demonstrated by the right hemisphere.

In support of this conclusion, it has been shown (Egeth, 1971), that if pairs of simple geometric forms, which may or may not match, are tachistoscopically presented to the same visual half-field, and all trials are of this type, then reaction time is unaffected by the locus of the stimuli, but that if the random sequence of trials includes trials with one form in each half-field, then reaction time to pairs in the left visual field is faster than for pairs in the right visual field. This suggests not only the importance of implicit scanning patterns, but relates these to right hemisphere superiority in a simple non-verbal task.

METHOD

In the two experiments to be reported here, the stimulus material consisted of pairs of semicircles which each contained within them one of six simple symmetrical half-circular designs, which are shown in Fig. 1. These stimuli were each back-projected on to one of four screens (28 × 44 cm, 63 cm from the subject) which were arranged together with partitions between the screens and the subject's eyes, so that, as the subject gazed at a central fixation light, the material on screen 1 (numbered left to right) was directed at the nasal hemiretina of the left eye, on screen 2 at the temporal hemiretma of the right eye, on screen 3 at the temporal hemiretina of the left eye, and on screen 4 at the nasal hemiretina of the right eye. Thus material appearing on screens 1 and 2 (left visual field: LVF) is directed to the right hemisphere, and on screens 3 and 4 (right visual field: RVF) to the left hemisphere.

A diagram of the apparatus is shown as Fig. 2. It should be noted that the scale of the apparatus together with the screens placed parallel to the line of sight of the subject meant that a considerable deviation of fixation was required for the presentation of stimulus material on any one screen to reach a hemiretina at which it was not directed. As the fixation of the subject was monitored throughout, by the visual inspection carried out by the experimenter, the apparatus was considered to be reliable. Stimuli were projected using a pair of standard Hanimex 1200A projectors placed 155 cm behind the screens.

Of the 80 trials in each experiment, in 26 the two stimuli appeared in the LVF (on screens 1 and 2), in 26 in the RVF and in 28 one in each visual field (BVF: randomly allocated to screens within this condition), the order of these trials being randomized. In half of the trials the two stimuli were symmetrical and therefore matched, and in half asymmetrical and so not matching, again randomly allocated to trials and stimulus designs. The response in Expt. I was to vocally respond 'same' or 'different' as appropriate, which terminated stimulus presentation, and from

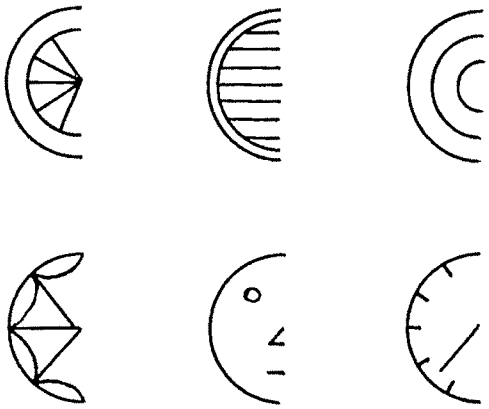

Fig. 1. Stimulus figures as they appear at the left of the pair to be matched. Mirror reversal gives right.

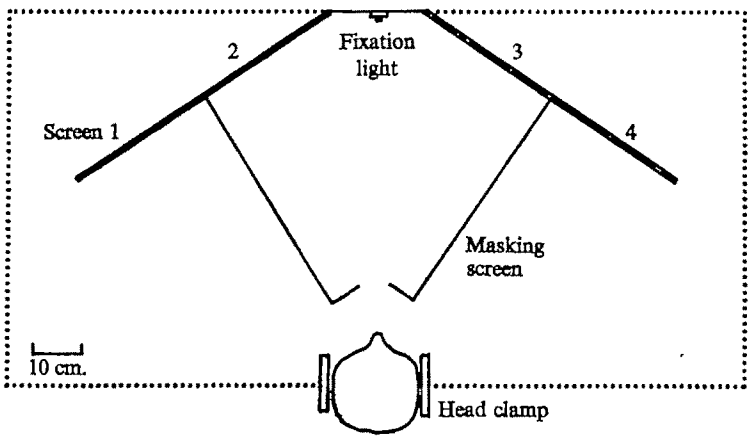

Fig. 2. Diagrammatic scale plan of apparatus.

which response latencies were recorded, from stimulus onset, by means of a Forth Instruments voice key. In Expt. II the response required was to bimanually depress two microswitches, one held in each hand, the faster of these two depressions being taken as the response time. Subjects were to press once bimanually for 'same' and twice for 'different'. In each case, the response terminated the trial and resulted in the removal of the stimulus material. The interstimulus interval was 1 sec.

The twelve subjects were first-year undergraduate students, five of whom were male, and their handedness was measured before the experiment using a handedness questionnaire (Annett, 1970). Fixation on the central light was continually monitored, but deviation of the eyes by the subject did not prove a problem.

Results

Means for each subject were calculated and combined to yield group parameters. In neither experiment do the number of correct responses show any significant differences between the conditions (overall mean: Expt. I, 79·14 per cent; Expt. II, 82·82 per cent), and while only the mean reaction times for correct responses are given below, the reaction times for total responses, which do not differ from the reaction times

Table 1. *Mean reaction time for correct responses (seconds)*

Condition	Expt. I Mean	Expt. I S.D.	Expt. II Mean	Expt. II S.D.
LVF	1·296	0·248	0·867	0·267
RVF	1·359	0·322	0·886	0·290
BVF	1·204	0·190	0·812	0·227

Table 2. *Percentage increase in reaction time of conditions in which material is directed to one hemisphere, as a function of response time of BVF condition*

Condition	Expt. I	Expt. II
LVF	8·33	7·41
RVF	13·33	9·88

for correct responses at accepted tests of statistical significance, yield an identical pattern of results and correlate highly with reaction times for correct responses.

The results for reaction times of correct responses in the three conditions in each experiment are given in Table 1. In Expt. I it will be seen that the fastest response latencies occur in the BVF condition and that this is significantly superior to the RVF condition ($t = 3{\cdot}14$, $P < 0{\cdot}01$), while the LVF condition lies between the two and is statistically different from neither. For the second experiment, the order of means is preserved, the BVF condition now being significantly faster than both the RVF ($t = 2{\cdot}63$, $P < 0{\cdot}05$) and the LVF ($t = 2{\cdot}35$, $P < 0{\cdot}05$) condition.

The relative performance of the left hemisphere against the conditions in which material is presented to the right hemisphere or divided between the hemispheres is of interest in relation to the response required. As can be seen from Table 2, in which the relative increase in latency of the LVF and RVF when compared with the BVF condition is presented, in Expt. I, where a verbal output is required there appears to be a relative blocking of left hemisphere function, which we can assume to be the hemisphere primarily involved in the organization of verbal output.

The possibility that subjects adopted the strategy in Expt. II of making a simple bimanual depression before the decision of match or non-match, and then either pressing again or not pressing on the second occasion as appropriate is not thought to be likely, as there is a difference, though not statistically significant, in terms of reaction times to 'same' presentations being consistently faster than those to 'different' presentations. Further, there was no noticeable delay between the two depressions in the case of 'different' bimanual responses.

Neither sex nor handedness, which naturally had a restricted range on the handedness continuum, was found to relate significantly to the primary experimental parameters.

DISCUSSION

The relative superiority of the BVF condition, in both experiments, where half of the stimulus material is directed to each hemisphere confirms previous findings (Dimond, 1970; Dimond & Beaumont, 1971) which suggest that the division of

perceptual load between the hemispheres allows total output to be increased. In terms of the analysis of the material, it appears to be no disadvantage that stimuli to be compared should arrive for analysis by two separate channels. This in turn supports the hypothesis that each hemisphere not only possesses its own analysing system, but proceeds some considerable way with such analysis before decisions based on the sampling of all inputs are made.

That the condition in which simple figural material is presented to the left hemisphere leads to consistently poorer performance in terms of increased response latency when compared to the right hemisphere, is also in line with previous findings (Levy-Agresti & Sperry, 1968; Egeth, 1971) and must lead to the conclusion that either processing of such material can proceed at a faster rate in the right hemisphere, or that there is some qualitative difference in the method of analysis as has already been suggested (Dimond, 1970).

With reference to the finding that there is a relative increase in latency of RVF trials against BVF trials in Expt. I, this suggests that a relative blocking of left hemisphere function occurs, as the overall processing capacity of the hemisphere is reduced by the requirements of the additional vocal task. By comparison, when a bimanual motor response is required, as in Expt. II, thereby placing output load on both hemispheres to more equal degree, the observed difference between the hemispheres is considerably reduced, while still allowing for faster response times to be attained with the right hemisphere.

These experiments, taken together, therefore demonstrate that previous findings indicating that perceptual load shared between the two cerebral hemispheres results in greater capacity being available for the analysis of incoming information can be extended to simple non-verbal material. When the two hemispheres are compared, previous work indicating the superiority of the left visual field in dealing with simple non-verbal percepts is supported and shown to be attributable to the superiority of the right hemisphere in the processing of such information. If an additional requirement of the task is to perform some verbal response, so that the output load is unequally divided between the two hemispheres, the result is a further relative decrement in the performance of the left hemisphere in dealing with simple perceptual material entering into it.

This work was carried out under tenure of MRC grant no. G 969/96/C. The authors are also indebted to Mr Philip Kinsey for his assistance with both the apparatus and running of the experiment.

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(*Manuscript received* 19 *August* 1971; *revised manuscript received* 11 *January* 1972)

Br. J. Psychol. (1972), 63, 4, *pp.* 515–522
Printed in Great Britain

AROUSAL VALUE OF THE STIMULUS AND EEG ABUNDANCE IN AN AUDITORY VIGILANCE TASK

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The EEG was monitored during an auditory vigilance task. The wanted signal consisted of three consecutive odd digits. Five other types of signal varied in their approximation to the wanted signal and were ranked on that criterion for their 'arousal' value. Each signal was followed by a rest period. The key results were: (i) alpha abundance (8·5–13·5 Hz) diminished as the arousal value of the signal increased ($P < 0·005$); (ii) theta (4·5–6·5 Hz) and beta (13·5–19·5 Hz) showed either weak or no effects; (iii) a very-low-frequency filter (2·0–4·5 Hz) yielded a strong effect for one class of signal only ($P < 0·021$); (iv) during rest periods which followed the three most arousing types of signal, alpha abundance was greater than during the preceding signal ($P < 0·025$): the reverse held for the least arousing signals ($P < 0·025$); (v) theta abundance was very much lower following response (rest period following the wanted signal) than following other signal types ($P < 0·01$) and theta abundance during rest was generally smaller than during signals ($P < 0·021$); (vi) for the three least arousing signals, activity at 2·0–4·5 Hz was lower during rest than during signals ($P < 0·05$); (vii) subjects rated themselves as being more 'keyed up' as cue or arousal value of the signals increased ($P < 0·001$) and (viii) similarly, reported that alertness increased during rest periods associated with signals of increasing cue value ($P < 0·01$). These findings demonstrate that during a task of this sort, the subject experiences variation in alertness; this variation in subjective state is reflected by systematic change in the EEG. It is concluded that these results lend support to arousal models of vigilance and to Lindsley's activation theory.

The vigilance task is a good candidate for psychophysiological research. Arousal theory explanations of variation in vigilance performance clearly call for monitoring of physiological indices of arousal. However, substantive support for arousal theory has not been forthcoming (Davies & Tune, 1970). Several different questions have been asked. *Do vigilance tasks which yield a decrement also yield physiological change?* Temporal change in the patterning of EEG, EDA and EMG has been demonstrated in tasks which yield decrements (Davies & Krkovic, 1965; Eason *et al.*, 1965). But results such as these demonstrate merely gross group changes in physiological level, associated with a simple contingency classification of 'good' and 'bad' performance. *Does rate of decrement relate to rate of physiological change?* Eason *et al.* (1965) calculated regression slopes for both detection efficiency and several physiological measures, but only their measure of Eye Region Response yielded a fit with decrement. *Can pre-signal levels differentiate between detection, omissions and false positives?* Theta activity (Daniel, 1967) and frequency of spontaneous skin potential responses (Surwillo & Quilter, 1965) differentiate between detections and omissions; but in the Daniel study (1967) findings were directly opposite to those which arousal theory would predict. Moreover, the findings in both studies were won after considerable manipulation of the data and the assumptions underlying the statistical treatment are questionable (Coles, 1970). Again, group data is derived, rather than sound evidence of covariation of physiological state and performance within the individual. *Do individual differences in patterning of physiological response differentiate between good and poor vigilance performers?* Ross *et al.* (1959) failed to relate type of individual

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trend in skin conductance to level of performance. However, Coles & Gale (1971), testing for vigilance performance and EDA habituation on *separate* occasions showed that three measures of EDA reactivity could serve to predict level of vigilance performance.

The above findings have been selected from a large body of data, most of which fails to support arousal theory. Demonstrations of covariation between physiological indices of arousal and vigilance performance are lamentably few and far between, and some studies go quite against theory (Gale *et al.*, 1972).

More recently, Gale, Haslum & Penfold (1971) have adopted a different strategy in dealing with the problem, by examining fluctuations in the EEG *throughout* the task. The cue value of non-wanted signals was varied systematically by constructing signal types successively approximating to the wanted signal. Since the wanted signal consists of three consecutive odd and unequal digits, it is possible to present signals containing, no, one, or two odd digits. Each signal is separated by a rest period. The four types of signal used may be ranked in terms of the expectancy built up in the subjects as the digits for each signal are presented. Thus if the first digit is an even number, the subject may relax for the remainder of the signal; if only the first digit is odd, he may relax after the second digit, and so on. In the case of wanted signals, the subject becomes progressively expectant that he must prepare for response and arousal builds up as consecutive odd digits occur, subsiding again after response, during the post signal rest period. Gale, Haslum & Penfold (1971) show that the occipital EEG (particularly in the alpha range) varies systematically, with the cue value of the digits. In addition, retrospective reports by the subjects of fluctuations in alertness during the task, parallel the EEG data. Thus the question asked here is *Does the subject's arousal wax and wane with the cue value of the stimuli?* Rather than employ the usual, *post hoc* correlation approach, we are attempting in advance to vary systematically the subject's level of arousal. Since EEG data is available for every individual digit in the task, ranking tests are employed to demonstrate the robustness of the effects *within* the signal types. Thus for the wanted signal, it was possible to show that for a significant proportion of subjects, EEG abundance becomes progressively diminished as the digits are presented, and increase in magnitude again after response, and so on. Thus the analysis is based essentially on fluctuations within individuals over the different types of signals. These findings have been replicated for EDA (Gale, Bull & Haslum, 1972). We were not successful, however, in relating pre-signal EEG levels to reaction time, nor was the task difficult enough to yield many omissions. The task therefore does not answer the questions asked in earlier tasks, but rather represents a change of emphasis.

For technical reasons, in order to obtain EEG samples of adequate duration, presentation rate in the Gale *et al.* (1972) task was exceptionally slow, individual digits occurring at the rate of 1/5 sec., making a full 20 sec. for each signal and its associated rest period. The present study is designed to make comparisons *across* rather than within signals, and we are therefore able to present the whole signal within 5 sec., with a rate of approximately one digit per 1·4 sec., a rate more typical of conventional tasks. Six signal types are employed rather than the four used in the original task and presentation is auditory rather than visual. Whereas in Gale *et al.* (1972) we were concerned with *directional* change within a signal type (i.e. increase

or decrease in abundance on presentation of each digit within a signal), our question here concerns *absolute magnitude* of abundance for signal types. It is predicted that (i) EEG abundance will vary systematically with the arousal or cue value of the signals; (ii) that subjective ratings of experienced alertness will vary with the arousal value of signals; and (iii) the rest periods following detections will yield increased alpha abundance.

Method

Subjects

The subjects were five male and five female undergraduate students at the University of Exeter, age range 18–22 years.

Procedure

The task was a 25 min. auditory vigilance task. Each signal consisted of three consecutively presented digits and was of 5 sec. duration. There were 150 signals in all, each being followed by a 5 sec. rest period. Thus signals were presented continuously at a rate of 1/10 sec. The wanted signals were three, odd, and unequal digits. There were six signal types in all, as follows:

Type 1	(EEE)	even–even–even	e.g. 4, 6, 2
Type 2	(OEE)	odd–even–even	e.g. 1, 2, 4
Type 3	(OEO)	odd–even–odd	e.g. 3, 2, 7
Type 4	(OOE)	odd–odd–even	e.g. 9, 7, 2
Type 5	(OOO=)	odd–odd–odd (equal)	e.g. 3, 9, 3
Type 6	(OOO)	odd–odd–odd	e.g. 5, 7, 3 (the wanted signal)

Each signal was followed by the word 'rest'. There were 25 signals of each type in the task. The signals themselves were made up with the aid of random number tables, using digits 1–9 and their order of presentation was random. To ensure maintenance of continuous attention, there were two occasions on which wanted signals were adjacent. The subject received printed instructions giving a sample set of signal sequences which he checked for wanted signals. No statement was made concerning the variety or nature of non-wanted signals. He was instructed to press a reaction time button as soon as wanted signals were detected, not to make errors, but to forget errors if in fact they were committed.

The task was read into a microphone by one experimenter (A. G.) for each subject. A metronome (controlled by the EEG analyser, see below) indicated to the experimenter the onset of each EEG epoch/sampling period. Upon hearing the metronome, the experimenter read out the three digits for the signal. The word 'rest' followed on the succeeding beat. The experimenter was practised in the task until his reading rate was approximately one digit per 1·4 sec. The aim of this procedure was to derive a separate EEG sample for every signal and rest period in the task.

The subject sat blindfold, with eyes shut, in the dark, in a soundproof cubicle. The task was presented through a loudspeaker set at the subject's feet.

Although the subject was instructed to respond to wanted signals as in a reaction time task, no such reaction times could be recorded, as the delay between stimulus presentation (i.e. the third digit of a wanted signal) and response could not be measured accurately. However, a record of omissions, detections and false positives was maintained.

After the task the subject completed a questionnaire, rating samples of each type of signal and its associated rest period on a five-point scale. The scale ranged from 1 ('completely relaxed') to 5 ('really keyed up').

The EEG

Loose-pad, silver–silver chloride electrodes were placed along the midline, the rear electrode being between the occipital lobes and the anterior electrode approximately 3 in. forward. The neutral electrode was on the wrist of the non-preferred hand. The primary record was taken on a San'ei Polygraph, at time constant 0.3 sec., with a gain of 30 mm peak to trough for 100 μV. EEG output was integrated by a San'ei Low Frequency Analyser through eight pure band-pass

filters over a 5 sec. epoch or sampling period. The filter frequencies were: 2·0–4·5 Hz, 4·5–6·5, 8·5–13·5 (in five single frequency steps), and 13·5–19·5. The filters are steep-skirted and flat-topped and were tested for linearity before the experiment with sine-wave oscillators set at their mid-frequency point. The voltage outputs of the analyser were converted into a binary code and stored on punch paper tape by a Lion Systems Development A-D Converter, and subsequently transformed to decimal numbers by a computer.

Data reduction

EEG. Each of the six signal types occurred 25 times in the task. Thus for each signal type there were 25 × 8 filter values for the signal itself and the same number for the associated rest periods. The alpha filters (8·5–13·5 Hz) were collapsed for each subject, for each signal type, and slow (2·0–4·5), theta (4·5–6·5) and beta (13·5–19·5) filters treated separately. The average value for each subject, for the 25 trials of each signal type, constituted the raw data for the statistical tests.

Ratings. Each subject gave a total of 12 ratings, one for each signal type and one for each associated rest period.

Errors. There were very few errors (i.e. omissions or false positives) and therefore no analysis was possible.

STATISTICAL ANALYSIS AND RESULTS

Several non-parametric tests of trend, rank, and paired comparison were used. For the EEG, three types of difference were examined; across signals, across rest periods and between signals and their associated rest periods.

EEG for signals

Alpha range (8·5–13·5 *Hz*). The mean integrated output values and their associated ranks for the six signal types were:

	Type					
	EEE	OEE	OEO	OOE	OOO=	OOO
Mean output	43·70	44·67	44·89	41·61	41·54	40·29
Rank	40	49	45	25	31	20

A monotonic trend test (Ferguson, 1966) showed a significant trend for this data ($P < 0·02$). Similarly, a ranking test (Friedman two-way analysis of variance, Siegel, 1956) showed that the signals were derived from different populations ($P < 0·005$) and even when the wanted signal (OOO) is excluded from the analysis ($P < 0·02$). A series of Walsh tests (Siegel, 1956) were then used to compare pairs of signals taken separately. All possible comparisons were made and the following results were obtained (tests were two-tailed unless otherwise stated). Since the aim was to demonstrate the transitivity of the relationship EEE greater than...OOO, the first in each of the following pairs is of the greater value. EEE:OEE (n.s.), EEE:OEO (n.s.), EEE:OOE ($P < 0·021$), EEE:OOO= ($P < 0·025$, one-tailed), EEE:OOO ($P < 0·021$), OEE:OEO ($P < 0·025$, one-tailed), OEE:OOE ($P < 0·010$), OEE:OOO= (n.s.), OEE:OOO ($P < 0·010$), OEO:OOE ($P < 0·010$), OEO:OOO= ($P < 0·021$), OEO:OOO ($P < 0·010$), OOE:OOO= (n.s.), OOE:OOO ($P < 0·021$); OOO=:OOO ($P < 0·021$).

When values for adjacent signal types are summed, then EEE+OEE > OOO= +OOO ($P < 0·010$), OEE+OEO > OOE+OOO= ($P < 0·021$) and, of course, EEE+OEE+OEO > OOE+OOO=+OOO ($P < 0·010$).

It appears therefore that the trend is equally distributed across the signal types, is transitive in nature, is not dependent merely upon a large difference between wanted signal and the rest, and is resistant to the effects of the relatively low value and rank displacement of signal type EEE (see Discussion below).

Slow (2·0–4·5 *Hz*). A Friedman test was significant at $P < 0{\cdot}05$. However, this was not due to an overall trend, but rather to a difference between OOE and four other signal types. Thus OOE < OEO ($P < 0{\cdot}001$) and OOE < OEE, OOO= and OOO ($P < 0{\cdot}021$ in all cases). Also EEE < OEO ($P < 0{\cdot}021$).

Theta (4·5–6·5 *Hz*). There were no significant effects.

Beta (13·5–9·5 *Hz*). Neither the Ferguson nor Friedman tests were significant. However, Walsh tests showed OEO > OOO ($P < 0{\cdot}010$) and OOE > OOO ($P < 0{\cdot}010$).

EEG for rest periods and comparison of rest and signal

Alpha range. There were no significant effects for rest periods as such. However, when rest periods following the three least arousing signals are collapsed and compared against collapsed values for signals, rest is smaller than signals ($P < 0{\cdot}025$, one-tailed). The reverse holds for the three most arousing signals, i.e. signals are smaller than rests ($P < 0{\cdot}025$, one-tailed).

Slow. There were no effects for rests, scores being virtually equivalent for all conditions. However, when abundance values for signals and their associated rests are compared, then for the three least arousing signals, value for rest is lower than for signal, EEE ($P < 0{\cdot}025$, one-tailed) OEE ($P < 0{\cdot}021$) and OEO ($P < 0{\cdot}010$).

Theta. The Friedman test was significant ($P < 0{\cdot}01$). This effect proved to be attributable to the difference between rest following OOO and four of the remaining rest periods. Thus rest for OEE, OEO, OOE and OOO= were all greater than rest for OOO ($P < 0{\cdot}010$ in all cases). When abundance values for signals and associated rests are compared, rest is smaller in all cases and significantly so for the following types: EEE ($P < 0{\cdot}021$), OOE ($P < 0{\cdot}025$, one-tailed), OOO= and OOO ($P < 0{\cdot}021$).

Beta. There were no significant effects for rest periods, nor were there any differences in abundance between signals and rests.

Subjective ratings of alertness

For both signals *and* rest periods subjects reported that they were progressively more aroused with signals of increasing cue value. The mean group ratings were:

	Type					
	EEE	OEE	OEO	OOE	OOO=	OOO
Signal	1·0	2·2	2·3	3·7	3·9	4·1
Rest	1·3	2·4	2·2	3·1	3·4	3·2

Friedman tests on both sets of data yield significance (signals, $P < 0{\cdot}001$; rests, $P < 0{\cdot}01$). Since subjects gave ratings on only a five-point scale, there were a large number of ties. However, for signals, the following pairs are significantly different. (The first value is always the smaller.) EEE:OEE ($P < 0{\cdot}01$), EEE:OEO ($P < 0{\cdot}01$),

EEE:OOE ($P < 0{\cdot}001$), EEE:OOO= ($P < 0{\cdot}001$), EEE:OOO ($P < 0{\cdot}001$), OEE:OOE ($P < 0{\cdot}001$), OEE:OOO= ($P < 0{\cdot}01$), OEE:OOO ($P < 0{\cdot}01$), OEO:OOE ($P < 0{\cdot}001$), OEO:OOO= ($P < 0{\cdot}001$), OEO:OOO ($P < 0{\cdot}001$). There were strong but insignificant trends for the rest periods.

DISCUSSION

This study demonstrates the possibility of an EEG abundance gradient, related to the cue value of auditory stimuli. We have obtained similar effects employing both complexity of visual stimulation and cue value of visual stimuli in vigilance as the independent variable (Gale, Christie & Penfold, 1971; Gale, Haslum & Penfold, 1971; Haslum *et al.*, 1972). Studies of this nature indicate that the notion of an arousal continuum is viable. One of the earlier difficulties with Lindsley's (1960) activation theory was the paucity of data showing covariation of psychological and physiological variables within the waking state. The fact that we have obtained these results for auditory as well as visual tasks further supports the generality of the notion. This need not imply that we are espousing the whole framework or all constructs of arousal theory (for example, the ill-supported but often-quoted 'inverted-U' relationship between activation and performance), but merely that we are asserting that arousal or activation does accompany certain aspects of behaviour, and is operationally measurable by polygraphic techniques. Further, we suspect that this type of task is more likely to bear fruit in studies of individual differences in arousal, since the task allows for continuous stimulus control over the subject. In the more typical 'habituation' study (e.g. Gale, Coles, Kline & Penfold, 1971) the subject is only under stimulus control during the very low proportion of experimental time when stimulation actually occurs. The present task is far more specific in terms of the demand characteristics as perceived by the subject.

It could be argued that this is not a vigilance task proper. First, a high proportion of signals have alerting properties, and secondly, the possibility of error is low. However, the rationale underlying the task does not preclude an increase in the order of difficulty. Thus, this task provides a basis for further work designed to examine individual differences in vigilance performance and physiological correlates of vigilance decrement.

The present data provide support for the predictions set out in the introduction. However, there are a number of points at which the data appears to be lacking or requires further explanation. First, the alpha frequency data. The overall rankings are not perfectly in accordance with prediction (see alpha range signal means). This is of course to be expected with any set of data, particularly with the EEG, where inherent variability and ratio of noise are so high. However, in the present case, variation from prediction is almost completely due to one individual subject, whose scores are perfectly the reverse of the predicted trend. Nevertheless, the data for remaining subjects is robust enough to sustain the trend. The relatively low abundance and rank value for signal type EEE are not what one would predict on the basis of a ranking of signals for arousal value. We have replicated this finding on two occasions (Gale, Haslum & Penfold, 1971; Haslum *et al.*, 1972) and it appears

puzzling that the least arousing signal should yield a low alpha abundance value. On a signal of this type, the subject is under least experimental control. Thus a possible explanation is that once he has heard the first digit (an even number) the subject is free from task requirements until the next signal occurs, and is, as it were, left to his own devices. Thus the subject may arouse himself at this time by redirection of attention or may employ it for consideration of the task itself and possibly of a task strategy. Such activity would attenuate the EEG. If such considerations do apply in this task, then they must be an even stronger source of experimental error in habituation tasks.

The effect for rest periods is not strong. In our previous study (Gale, Haslum & Penfold, 1971) there was a considerable increase in alpha abundance following response to wanted signals and subjects reported being particularly relaxed at that time. In the present study, there is not a marked change in the EEG for rest periods and subjective report shows that experienced alertness during rests parallels the effect for signals. The differences for the two studies may be a function of the increased presentation rate for the present task.

The results for the other measured EEG frequencies cannot be fitted into an existing framework of knowledge. By far the greater part of human EEG studies have focused on the alpha range. The alpha range appears to reflect level of arousal and in particular, activity within the visual system; the significance of other frequencies is not really understood and the sheer absence of data concerning these frequencies makes interpretation difficult (e.g. see Gale *et al.*, 1969). We have shown in this task that theta activity is lower during rest periods than during signals and particularly low following wanted signal/response. Since activity at this frequency is often associated with drowsiness and light sleep, we might have predicted an *increase* during rest periods, indicating habituation to the word 'rest'. We cannot account for the present results; the most which can be said is that theta does differentiate between signals and rests, and between rests of different types.

The absence of significant data for the beta range may be due to the insensitivity of the measuring instrument rather than to a real invariance, for abundance values in this range are extremely low. In other studies (e.g. Gale, Coles & Boyd, 1971) we have shown that alpha and beta follow similar trends, when samples of greater duration are available.

The slow-frequency filter was included in this study because we thought that in spite of our mode of recording and short time constant, we might possibly detect aspects of the CNV wave reported by Walter *et al.* (1964). Although the rationale underlying this experiment is reminiscent of the CNV paradigm, it is difficult to predict what the outcome might be, given our abundance technique. The particularly low value for OOE must stand unexplained for the present time.

The subjective ratings for signals are in the predicted direction. These ratings were of course obtained after the task and are subject to the criticism that responses may not genuinely reflect experience during the task, but merely the effects of social desirability in answering a highly structured questionnaire. It is technically feasible to obtain such ratings during the task and such studies are underway.

In conclusion we may observe that studies showing discrete covariation of EEG

and an independent variable are possible. The failure of earlier studies to obtain effects of this sort may have been due to the use of an essentially correlational technique where existing tasks are used and physiological reactivity is measured as a mere accompaniment to performance. The present technique involves the restructuring of existing tasks to allow for the limitations of physiological measurement.

This work was supported by a grant from the Medical Research Council (reference G. 969/185/C). Mary Haslum is a Social Science Research Council postgraduate student.

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(*Manuscript received* 31 *August* 1971)

Br. J. Psychol. (1972), 63, 4, *pp.* 523–531
Printed in Great Britain

TYPES OF IMAGERY AND IMAGERY TYPES: AN EEG STUDY

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The occipital EEG of subjects differing in reported vividness of mental imagery was monitored during a series of mental imagery tasks. (i) Vivid imagers (selected on the basis of the Betts Vividness of Imagery Scale) had a higher mean dominant alpha frequency (m.d.f.) than weak imagers ($P < 0{\cdot}025$), but there were no individual differences for alpha abundance. The difference for m.d.f. held for only one task – an eyes-open, minimal imaging condition. (ii) Greater alpha suppression followed presentation of High Imagery (High I) words than followed presentation of Low Imagery (Low I) words ($P < 0{\cdot}005$). (iii) High I words elicited more imagery than Low I words, for both vivid and weak imagers ($P < 0{\cdot}005$). (iv) Reported elicited imagery was greater for the vivid imagers ($P < 0{\cdot}005$). (v) For two voluntary imaging tasks, alpha suppression was greater in that task reported by the subjects as being harder to visualize ($P < 0{\cdot}01$), thus reversing the findings for (ii) above. (vi) Subjects completed the Eysenck Personality Inventory. None of the above findings could be attributed to individual differences in either extraversion or neuroticism. It is concluded that the act of imaging *per se* does influence the EEG and that alpha suppression is not merely a by-product of the arousing nature of imaging tasks. However, difficulty in the act of imaging also influences the EEG. The equivocal results of earlier research may therefore be accounted for in terms of the differential effects of instruction to image, upon the EEG, as demonstrated in the present study.

Golla *et al.* (1943) claimed that individuals employing visual imagery for problem-solving had a characteristically lower amplitude occipital EEG than subjects who employ verbal methods. Many attempts have since been made to establish a necessary connexion between the presence or absence of mental imagery and some property of alpha activity. However, subsequent work has failed to replicate their findings and the arguments underlying the expected relationship have been questioned. The literature has been reviewed by Richardson (1969).

The main methodological difficulties concern the measurement of the appropriate imagery and alpha characteristics. For example, imagery has sometimes been measured by asking subjects to report on the way in which they attempt to solve a set of mental tasks, and sometimes by noting scores obtained on tests of spatial ability. Neither procedure is entirely satisfactory. The first leads to a rather crude classification of subjects into those who are judged to be predominantly visual in their mode of thought or predominantly verbal, or a mixture of the two. The second makes the unwarranted assumption that performance on some types of spatial task will necessarily require the employment of visual images for its solution. In the measurement of alpha activity, properties like amplitude, abundance, percentage and dominant frequency have all been examined with varying degrees of technical sophistication.

The main conceptual difficulties concern the justification of any argument that asserts a connexion between visual imaging and alpha activity. Because alpha activity tends to be suppressed when anyone is actively engaged in visual contact

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with a changing environment, the manipulation of mental equivalents (i.e. visual images) when the eyes are closed is believed to have a similar effect. It is known, however, that *any* form of heightened intellectual effort may produce alpha suppression. Oswald (1957) suggests that alpha suppression in imaging tasks may be entirely due to the increase in cortical arousal produced by an increase in effort rather than in the use of visual imagery *per se*. This alternative account of the relation that has sometimes been found between imagery and lowered alpha activity may well apply when a subject is trying to solve a 'difficult' spatial task with his eyes closed. But whether it provides a satisfactory explanation of alpha suppression when 'easy' visual tasks are employed is more doubtful. For example, in a study by Kamiya & Zeitlin (1963) alpha suppression was produced voluntarily by a subject when it had been learned that the controlling stimulus was his own imagery. Though alpha suppression occurred at the onset of visual imaging, no concomitant changes were found for *other* physiological indices of arousal, i.e. heart rate, respiration pattern, eye movements or muscle tension. However, it is clear that ease of difficulty of the imaging task may have been a confounding variable in the earlier studies.

There is now substantial support for the view that imagery is an important experimental variable in learning tasks (Paivio, 1969; Morris & Reid, 1971). Moreover, individual differences in imaging have been related to individual differences in performance (Ernest & Paivio, 1971*a*). Thus in spite of the fact that the work of Golla *et al.* on visual imagery has been subject to criticism, there does appear to be some ground for believing that *imaging ability as such* (whether visual or in other modalities) is a basis for the classification of subjects (Ernest & Paivio, 1971*b*).

The present study is concerned with individual differences in imaging ability and with the effects of differential instructions to image upon the EEG. The aims of the study are: (i) to examine the resting EEG of individuals classified as 'vivid', 'intermediate', or 'weak' imagers, (ii) to measure the effects of passive, spontaneous, voluntary or non-imaging conditions upon the EEG, and (iii) to examine a possible interaction between individual differences and type of instruction. Finally, because other personality variables (e.g. extraversion and neuroticism) have been shown to influence the EEG (Gale *et al.*, 1969), to take account of such variables in the analysis. Two measures of the EEG are taken, namely, alpha abundance and mean dominant alpha frequency.

METHOD

Subjects

An abbreviated version of the Betts QMI Vividness of Imagery Scale (Sheehan, 1967) was group-administered to 71 undergraduate students at the University of Exeter. Only the first 15 items were used, i.e. the scales relating to visual, auditory and kinaesthetic imagery. The product-moment correlations of these subtests with the total score were 0·76, 0·78 and 0·79 respectively. Thirty subjects were selected for the criterion groups shown in Table 1.

Mean age for the experimental group was 20·5 years with a range of 19–32 years. These subjects later completed the Eysenck Personality Inventory (Eysenck & Eysenck, 1964).

The EEG

Silver/silver chloride, loose pad electrodes were placed transoccipitally for bipolar recording (Cooper *et al.*, 1969). Interelectrode resistance was always below 10 kΩ. The neutral electrode was placed on the left wrist. The EEG was recorded on a San'ei Polygraph, calibrated at 30 mm

Table 1

	Vivid imagers			Intermediate imagers			Weak imagers		
Sex	Mean	Range	*n*	Mean	Range	*n*	Mean	Range	*n*
Male	29·6	27–32	5	41·4	37–46	5	54·3	51–62	6
Female	26·6	19–32	5	40·8	38–44	5	55·3	52–58	4
Overall	28·1	19–32	10	41·1	37–46	10	54·8	51–62	10

peak-to-trough for 100 μV, with a time constant of 0·3 sec. The EEG was integrated over 5 sec. epochs by a San'ei Low Frequency Analyser over five pure band-pass filters, covering the range 8·5–13·5 Hz in single frequency steps. The output voltage of the analyser was converted to digital form and stored on paper punch tape by a Lion Systems Development A-D Converter. The digital values were subsequently converted to decimal form by a computer. The EEG was recorded throughout Tasks 1–4 (see below).

Physical arrangements

The subject sat in an adjustable chair, in a soundproof booth, facing a frosted glass back-projection screen (6 in. high by 16 in. wide). The room was moderately illuminated. The experimenter could observe the subject through a peep-hole. Tape-recorded instructions were administered through a loudspeaker.

Procedure

There were six tasks, performed in the following order. (1) Eyes-open (minimal imagery) and eyes shut (spontaneous imagery) (Gale *et al.*, 1969). (2) Imaging to words of high and low imagery (passive elicited imagery) (Paivio *et al.*, 1968). (3) Imaging while receiving instructions for a voluntary imaging task (voluntary elicited imagery). (4) Imaging in accordance with instructions (voluntary autonomous imagery) (Drewes, 1958). (5) Ratings of imagery elicited in Task 2. (6) Assessment of ease of imaging in Task 4.

Instructions for Tasks 1–4 were pre-recorded on magnetic tape and played back to the subject.

Task 1

The resting EEG of 'vivid', 'intermediate' and 'weak' imagers under conditions of minimal and spontaneous imagery. The subject was instructed to relax and open or shut his eyes upon request. One eye closed trial (EC) was followed by one eyes open trial (EO), both of 1 min. duration.

Instructions. 'The instructions for the task will be given in 15 sec. from now. Here are the instructions. These instructions are being given over a tape recorder to ensure that they are identical for all subjects. Please shut your eyes and relax. We have a number of tasks for you to do. In the first task all you have to do is to open or shut your eyes when we ask. Just relax. Try not to think of anything. Do not mark the passage of time. Keep your mind as clear as possible. Our apparatus is very noisy. Please try to pay no attention to it. Do not count the beat of the metronome [a reference to the tape-punch]. Open your eyes please. When we ask you to open your eyes during the task, could you please look at the rectangular screen of light in front of you [a reference to the frosted screen]. Again, relax, try not to think of anything, keeping your mind as clear as possible. We will tell you when to open or when to shut your eyes. So please keep them open or shut as instructed. We are now ready to begin. Shut your eyes please [EC trial]. Open your eyes please [EO trial].'

Task 2

The EEG during passive elicited imagery. Twenty words (containing 6, 7 or 8 letters only) were selected from the Paivio *et al.* (1968) list. Morris & Reid (1972) have shown the list to be applicable to British students. The High Imagery (High I) words were: acrobat, boulder, corner, factory, forehead, officer, square, string, tripod and village. The Low Imagery (Low I) words were: amount, answer, attitude, belief, chance, interest, method, position, quality and thought. Mean I for the High I is 6·30 (range 6·13–6·53) and for the Low I, 2·81 (2·50–3·13). Both sets of

words were equated for meaningfulness (m). Thus mean m for High I and Low I is 5·63 and 5·62, with ranges of 5·08–6·04 and 5·20–6·24. The words were prepared in capital type, photographed, and projected singly on to the rear of the frosted screen. The subjects were allocated at random to five groups and each group was presented with a different random order of words.

Instructions. 'Thank you. Please shut your eyes and relax. Here is the second task. Please open your eyes. You will now be shown slides on the screen in front of you. A sample slide is now on the screen. Each slide will contain one word. Some people find that the words we show you arouse mental pictures, while others find they don't. For any person, the words we will show you differ in their capacity to arouse mental imagery. Some words arouse a sensory experience such as a mental picture or a sound, quickly or easily; whereas other words do so only after a long delay or even not at all. When the slide comes on the screen, please read the word carefully. As soon as the slide goes off the screen, close your eyes, relax, and see if the words suggests mental pictures, sounds or any other sensory experience to you. Do not worry if nothing comes to your mind. Relax and wait for the next slide. When we are ready we will ask you to open your eyes again. Remember, do not make any effort to force the images into your mind; just relax and let them occur naturally. But it is important that you should not think of anything else. Try to let the images related to that word come into your mind. If they do not come, do not worry, but do not think of anything else. Look at the word while it's on the screen and shut your eyes as soon as it goes off the screen. We will tell you when to open your eyes again. Please do not open your eyes until we ask you to.'

Schedule. Each slide was presented for 5 sec. The subject then shut his eyes. An experimenter observed the subject during the first trial to ensure that instructions to close eyes were being obeyed. After an interval of 25 sec. the subject was instructed to open his eyes and the next slide was presented. Slide presentation and instruction to open eyes were synchronized with the epoch time of the EEG analyser. The sequence of slide presentation, slide removal, eye closure, instruction to open eyes and presentation of next slide was continued until all 20 slides had been presented.

Tasks 3–4

The EEG during voluntary elicited imagery and during autonomous voluntary imaging. Two passages were used: Passage 1 (Table-top) is a modified version of a passage used by Drewes (1958); Passage 2 (Seaside) was especially devised for this experiment. The order of presentation of the passages was reversed for the final fifteen subjects, to control for possible order effects. For each passage, there was an instruction period of 2 min. 50 sec. (Task 3), followed by a voluntary imaging period of 2 min. (Task 4).

Instructions. 'Thank you. That is the end of that task. Please keep your eyes closed and relax. Form a mental picture of a large table and imagine yourself looking down on the top of the table. The table top is painted white. In the top right-hand corner of the table is a small black triangle. In the top left-hand corner is a small black circle. Get a clear mental picture of the triangle and the circle in position on the table. Keep your eyes closed. For the next two minutes I want you to move the triangle and circle about in your mind. They can move together or independently. Make them move up and down, left and right, or diagonally. Please start doing that now, keeping your mind on the problem.' (End of passage 1, 'table-top'.)

'And now we come to the final task. Please keep your eyes closed and relax. Imagine that you are walking along the sea front, down the promenade. You see a car drive up. A family get out of the car; a father, a mother, and two small children. The children have buckets and spades. They walk along the front, down some steps, and on to the beach. The parents get deck chairs. The children play on the sands. Get a clear mental picture of the family on the beach. For the next two minutes please imagine the family on the beach and the sorts of things they would do. Please keep your mind on the family until I ask you to relax.' (End of passage 2, 'seaside'.)

'Thank you very much. That is the end of all the tasks we have for you in here. Please relax until we come and let you out of the cubicle. Thank you.'

Task 5

Ratings of imagery elicited in Task 2. Subjects rated each word used in Task 2, on a seven-point Low Imagery–High Imagery scale, according to the degree of imagery experienced during exposure of the word. The following instructions were printed above the scales.

Instructions. 'Here are the words that you were shown earlier. We would like you to rate them on the ease with which they aroused mental images for you during the experiment. A word which aroused a mental mage (i.e. a mental picture, sound or other sensory experience) very quickly and easily should be given a *High Imagery* rating; any word which aroused a mental image only with difficulty or not at all should be given a *Low Imagery* rating. Your rating will be made on the seven-point scales next to each word. Make your judgement by putting a circle round the number that best indicates the ease with which the word aroused imagery *for you*. Feel free to use the entire range of numbers. However, don't be concerned about how often you use a particular number *as long as it is your true judgement*.'

Task 6

Comparison of ease of imaging for passages 1 *and* 2 *of Task* 4. Subjects were asked which of the two passages was easier to visualize.

Results

Two measures were used, EEG abundance and Mean Dominant Frequency (m.d.f.). Abundance was calculated by summing and averaging the analyser integrated outputs. M.d.f. was calculated by dividing the sum of products of central filter values and their integrated outputs by the sum of integrated outputs (Gale *et al.*, 1969). Log transformations were performed on all abundance data to reduce skewedness.

Task 1. For each subject, abundance and m.d.f. were calculated for the first 30 sec. of the EC and EO trials.

There were no significant differences for abundance in EC or EO, nor for m.d.f. in EC. The m.d.f. for EO gave $F = 4{\cdot}89$; d.f. = 2, 27; $P < 0{\cdot}025$. The mean m.d.f. for the three groups were: Vivid imagers 11·032 Hz, Medium imagers 11·023, and Weak imagers 10·966. The Tukey test showed means for the Vivid and Weak groups to be significantly different ($P < 0{\cdot}05$).

Task 2. For each subject, abundance and m.d.f. were calculated for (i) the 5 sec. of slide exposure and (ii) the 10 sec. following exposure.

Calculations for (i) yielded no significant effects. For (ii), two-way analyses of variance revealed a significant difference between slides but not between criterion groups. Mean abundance for high I slides was 12·48 and for low I slides was 13·28 ($F = 13{\cdot}49$; d.f. 1·27; $P < 0{\cdot}005$). M.d.f. effects were not significant.

Task 3. For each subject, abundance and m.d.f. were calculated for the first 30 sec. of the instructions (following the words 'please close your eyes and relax').

The means indicated that no further analysis for criterion groups was justified. There was a significant difference in abundance between the passages. The mean for Passage 1 was 39·02 and for Passage 2 was 41·91 (related $t = 2{\cdot}47$; d.f. = 29; $P < 0{\cdot}01$; two-tailed test). There were no differences for m.d.f.

Task 4. For each subject, abundance and m.d.f. were calculated for the first 30 sec. following the end of the instructions. For abundance, a two-way analysis of variance yielded only a main effect for the Passages ($F = 9{\cdot}89$; d.f. = 1, 27; $P < 0{\cdot}005$). The means were: Passage 1, 39·68; Passage 2, 44·52; Vivid imagers (1, 35·74; 2, 42·62), Medium imagers (1, 46·76; 2, 50·82), Weak imagers (1, 36·56; 2, 40·12).

Task 5. Mean ratings per word per criterion group were calculated. A two-way analysis of variance yielded significant main effects for words ($F = 173{\cdot}20$; d.f. = 1,18; $P < 0{\cdot}005$) and groups ($F = 27{\cdot}73$; d.f. = 2,36; $P < 0{\cdot}005$) and a significant interaction of words and groups ($F = 6{\cdot}62$; d.f. = 2,36; $P < 0{\cdot}005$). The mean values were: vivid imagers, high I 6·21, low I 3·42; medium imagers, 5·54, 3·40; and weak imagers, 5·44, 2·81. The interaction can be accounted for as follows:

(*a*) For high I words, vivid imagers have significantly higher ratings ($P < 0{\cdot}01$) than medium and weak imagers, who do not differ significantly. (*b*) For low I words, weak imagers have significantly lower ratings ($P < 0{\cdot}01$) than vivid and medium imagers, who do not differ significantly.

Task 6. Twenty-one subjects reported that Passage 2 (Seaside) was easier to visualize, five reported Passage 1 (Table) as easier and four reported no difference. The binomial test showed that a significant number of subjects reported Passage 2 as easier ($z = 2{\cdot}94$; $P < 0{\cdot}002$).

EPI. One-way analyses of variance for extraversion and neuroticism scores showed no significant differences between criterion groups. A product-moment correlation between Betts' score and extraversion yielded an r of $-0{\cdot}336$ which is significant ($P < 0{\cdot}05$). However, this result is questionable since the use of discrete subject groups constitutes a breach of the assumptions of the test. Neuroticism and abundance were positively correlated ($r = 0{\cdot}351$, $P < 0{\cdot}05$) but this correlation became smaller and non-significant when a positive correlation (non-significant) between extraversion and abundance was partialled out. Correlations between m.d.f. and the personality variables were non-significant.

DISCUSSION

The four key findings of this investigation are: (i) the EEG of weak and vivid imagers may be differentiated, but only in terms of mean dominant frequency (m.d.f.) rather than the more typically employed measure of alpha abundance, and only under an eyes-open minimal imaging condition; (ii) passive elicited imagery following presentation of High Imagery (High I) words leads to greater alpha suppression (abundance rather than m.d.f.) than does imagery elicited by Low I words; (iii) this finding is reversed under voluntary imaging (both elicited and autonomous), where a passage rated as harder to visualize leads to greater alpha suppression (abundance) than an easier passage, and (iv) criterion groups (vivid, medium and weak imagers) are differentiated in terms of strength of imagery elicited by High and Low I words and at the same time criterion groups all differentiate between High and Low I words.

Before considering the implications of these findings, a preliminary caution is necessary concerning our particular nomenclature. We believe that the labels we have attached to the different types of imaging investigated here are not merely labels which discriminate between our procedures, but do reflect genuine differences in type of imagery. Although the use of our particular terminology is novel and arguable, we believe that the instructions do lead to different types of imaging experience. The reader is invited to examine the detailed instructions given in the procedural section, in conjuction with our discussion of the findings.

The most straightforward results concern the High and Low I words and the

Passages. These indicate that imagery *per se* leads to an attenuation of alpha activity, but manipulation of degree of difficulty of imaging by variation of instruction also influences the outcome. When subjects are instructed to image but without effort and with reassurance that there is no penalty for failure to image, then High I words lead to alpha suppression. In contrast, when instructions invite collaboration either in active imaging to an ongoing descriptive passage or in production of autonomously controlled imagery which must be sustained for a long period, then the degree of effort involved has its effect. Thus the reversal of findings for Words and Passages is attributable to the effort involved in satisfying the demand characteristics of the experimental situation. A straight comparison of absolute abundance values for Words as against Passages would further substantiate this interpretation, since if Passages (and voluntary imaging) is a harder task then abundance values should be lower than for Words. However, because of the nature of the tasks and the necessary differences in EEG sampling, such comparison is not possible since it would involve comparison of means derived from samples of different duration. The m.d.f. for Passages is higher than for Words for all criterion groups taken separately and the whole sample (Words m.d.f. 10·99 Hz, Passages m.d.f. 11·03 Hz) but these differences are not significant. In retrospect, it is unfortunate that subjects were not invited to provide comparison of degree of difficulty *across* tasks; however, inspection of the instructions indicates that there is a prima facie case for our interpretation of the data. It is possible that existing discrepancies in the literature may be accounted for in terms of the differential effects of instruction to image (Morris & Gale, in preparation).

The ratings for Words require no further explanation. They show firstly that the Betts inventory is robust for this particular mode of external validation, and secondly that weak imagers, although they appear to operate within a narrower band of imaging experience, are nevertheless able to discriminate between High and Low I words.

We have considerable difficulty, however, in explaining the findings for the EEG and individual differences in imaging. Our measure of m.d.f. was taken under nine conditions and for all of these, the mean m.d.f. for weak imagers is lower than for vivid imagers; but paradoxically, the effect is only significant when eyes are open and there is no instruction to image. One possibility is that under *all* conditions of imaging, weak imagers experience some difficulty, which would have the effect of suppressing alpha activity. Vivid imagers, however, also show suppression either because of richness of imagery *or* because of difficulty. Thus, on all occasions except for eyes open, the differences between the groups are not strong enough to be significant, since both undergo a common shift in abundance. One difficulty here is the eyes closed condition where there is also no instruction to image; however, this condition yields least experimental control over the subject and there is a strong possibility that when instructed to relax and close his eyes, the subject will experience spontaneous alpha suppression, because this condition provides the opportunity for reflexion upon the task, the apparatus and so on. This explanation of our paradoxical finding involves a good deal of speculation and is not wholly satisfactory, but does gain some support from the findings for Words and Passages, which indicate that both imaging and effort to image can reduce alpha abundance.

Are the differences between vivid and weak imagers accountable for in terms of other sources of individual variation? We have obtained a low but significant correlation between extraversion and vividness of imagery; but since our vivid imagers have a high m.d.f. and extraverts a low m.d.f. (Gale *et al.*, 1969), extraversion could not underlie our findings. Differences in the resting EEG between extraverts and introverts have been accounted for in terms of 'arousal' and it must remain a possibility that the similar differences observed between vivid and weak imagers may be a function of some more fundamental difference (e.g. arousal) of which richness of imagery is merely a by-product. Thus the association between EEG characteristics and imaging characteristics may not be as direct as earlier workers have assumed.

Finally, we may observe two weaknesses in this study. First,findings for our two EEG measures, abundance and m.d.f., do not form a systematic pattern; significant findings for individual differences are limited to m.d.f. and differences for tasks are in terms of abundance only. It is generally assumed that m.d.f. and abundance are inversely related, but there is little empirical data to support this view (Gale *et al.*, 1969). Secondly, we have made comparisons between tasks in the absence of a design which would allow for direct quantitative comparison and our interpretation of the findings is lacking in this respect. However, we were unable to construct a design which would provide such direct comparison and yet at the same time yield the range of imaging situations which an exploratory experiment of this nature calls for. Nevertheless, we submit that our findings are robust enough to justify further consideration of the effects on the EEG of individual differences in imaging capacity and the effects of different types of imaging task.

This work was supported by a grant to the first author from the Medical Research Council (reference G. 969/185/C).

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(*Manuscript received* 31 *August* 1971)

Br. J. Psychol. (1972), 63, 4, *pp.* 533–536
Printed in Great Britain

INTERFERENCE AND SHORT-TERM RETENTION

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The recall of a five-consonant sequence was tested after retention intervals of 0, 2, 4 and 6 sec. Each subject was tested only once, thus eliminating intra-experimental PI. Two experimental conditions were employed. In one (VV) both the stimulus and interpolated material were presented visually. In the VA condition presentation of the stimulus was visual with auditory presentation of the interpolated material. Significant forgetting occurred in both conditions, VA recall being superior to VV. The implications of these results for a unitary theory of memory are discussed.

A persistent problem in memory theory is whether the typical short-term retention curves first reported by Peterson & Peterson (1959) require different explanatory concepts from those developed to account for long-term forgetting. Keppel & Underwood (1962) reported that the short-term retention of an item is not independent of the number of items previously learned. They argued that the forgetting produced using the Peterson technique is due to a build-up of intra-experimental proactive interference (PI), and that the additional concept of trace-decay (Brown, 1958) is unnecessary.

Although the precise mechanisms underlying the accumulation of short-term PI are uncertain (Conrad, 1967), it is clear that any test of the trace-decay theory of forgetting must eliminate intra-experimental PI, preferably by testing each subject only once. However, the results of such experiments have proved equivocal. Houston (1965) reported almost total forgetting of five- and six-word sequences within a 6 sec. retention interval, while Keppel & Underwood (1962) found almost no forgetting on trial one of a trigram stimulus after 18 sec. Reviewing these two experiments, Baddeley & Scott (1971) suggest that Houston's data may reflect loss from a sensory store, while Keppel & Underwood's failure to find any loss on trial one was due to a masking ceiling effect. With both these variables controlled, Baddeley & Scott showed that the ability to recall digit strings declines over short retention intervals. However, it is possible that the experimental design used by Baddeley & Scott resulted in an underestimate of the amount of short-term forgetting. The forgetting curves that they report are based upon the correct recall of 3-, 5- and 7-digit sequences after various retention intervals, filled by a letter-copying task. Since the presentation of both digits and letters was auditory, it is likely that some of the *initial* retention loss was due to a suffix effect (Crowder & Morton, 1969; Morton & Holloway, 1970). The *delayed* retention loss on the other hand would be due to a combination of suffix effects and short-term forgetting. It does not follow that these two effects summate algebraically, as the maximum loss due to the suffix effect may well be similar to that at which the forgetting function reaches asymptote. In this case removing the suffix effect would increase immediate recall, but leave delayed recall unaltered. This would apply particularly for the longer digit sequences. It follows, therefore, that in order to obtain a true estimate of short-term forgetting, suffix effects must be eliminated.

Morton & Holloway (1970) have shown that this can be done by using a visual stimulus and an auditory suffix.

The present experiment was designed to investigate short-term retention under these conditions. A second condition, using the visual modality for both the stimulus and interpolated material, was also included.

METHOD

Material

A pilot study investigated what combination of stimulus length and presentation interval was necessary to produce perfect recall yet avoid ceiling effects. Twenty subjects attempted the immediate recall of consonant sequences presented in a memory drum for various time intervals. In order to eliminate PI effects, each trial was separated by a 5 min. rest period (Loess & Waugh, 1967). The results indicated that a sequence of five consonants presented for 1 sec. produced perfect immediate recall. However, increasing the number of consonants, or decreasing the presentation interval, disrupted recall. It was concluded, therefore, that five consonants presented for a 1 sec. interval would not be overlearned, thus eliminating ceiling effects. In the main study a single five-consonant sequence, TNSLP, was employed for all subjects.

Procedure

Subjects were required to learn the visually presented consonants, perform an interpolated task, and recall the consonants when instructed. Two conditions were employed. In the first (visual–visual, VV) the interpolated material consisted of a three-digit number bounded on each side by an asterisk. The asterisks and digits appeared in the same position in the memory drum aperture that the consonants had occupied. The subject was required to perform a continuous subtraction task from the number in steps of three until the experimenter asked for recall. Subtraction was paced by the memory drum which rotated once every second. In the second condition (visual–auditory, VA) the subject closed his eyes after the consonants had been presented, and performed the subtraction task from a three-digit number spoken by the experimenter in 1 sec. Subtraction was paced by the click of the memory drum.

In both conditions, subjects received a single recall test with a maximum recall interval of 30 sec. Four retention intervals, 0, 2, 4 and 6 sec., were employed. Subjects were randomly allocated to one of the eight experimental conditions. Before the learning session each subject spent 10 min. practising the interpolated task.

Subjects

The subjects were 80 male and female university students, none of whom had taken part in the pilot study.

RESULTS

Fig. 1 shows the mean per cent of consonants correctly recalled in the two conditions as a function of retention interval. Analysis of variance indicated significant modality ($F = 15{\cdot}70$; d.f. $= 1, 72$; $P < 0{\cdot}001$) and retention interval ($F = 20{\cdot}24$; d.f. $= 3, 72$; $P < 0{\cdot}001$) effects. There was no significant interaction ($F = 1{\cdot}80$; d.f. $= 3, 72$; $P > 0{\cdot}10$). A similar analysis, omitting the immediate recall data, showed similar results (for modality, $F = 5{\cdot}20$; d.f. $= 1, 54$; $P < 0{\cdot}05$; for retention interval, $F = 18{\cdot}36$; d.f. $= 2, 54$; $P < 0{\cdot}001$; and for interaction, $F < 1{\cdot}00$; d.f. $= 2, 54$).

DISCUSSION

The main purpose of this experiment was to investigate short-term retention with the suffix effect eliminated. The VA data are therefore the most relevant to this

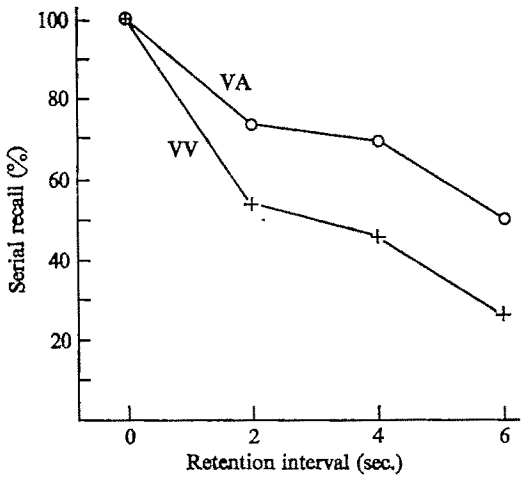

Fig. 1. Retention in the VV and VA conditions.

discussion. They show unequivocally that recall efficiency decreased over time. This is an important finding, for as the decrement cannot be attributed to a suffix effect, it follows that it must reflect short-term forgetting. What is more, this forgetting could not have been the result of intra-experimental PI.

These results are clearly compatible with trace-decay theory. However, the fact that so much forgetting occurred in the absence of intra-experimental PI poses problems for interference theory. It is, of course, possible that sources of interference other than PI were responsible. For example, Melton (1963) has suggested that retroactive interference (RI) and intra-unit interference (II) may also affect short-term retention. However, the lack of similarity between the stimulus letters and interpolated numbers used in this experiment suggests that RI would be minimal. Furthermore, attempts to demonstrate that II is a powerful source of short-term forgetting have not proved successful (Baddeley & Scott, 1971). Unless, therefore, such a demonstration is forthcoming, the results of this experiment do not support the unitary theory of memory proposed by Melton (1963).

The fact that VA recall was higher than VV recall suggests that the original level of learning was somewhat higher in the former condition. This may have been due to the VA subjects using a brief visual trace (Sperling, 1960) to read information into memory during the presentation of the interpolated number. In the VV condition this trace would be erased at the onset of the interpolated material (Averbach & Coriell, 1961). This would result in less time being available to code information into memory in the VV condition. Thereafter the same factors would operate in each condition to produce forgetting.

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(*Manuscript received* 8 *December* 1971; *revised manuscript received* 1 *February* 1972)

Br. J. Psychol. (1972), 63, 4, *pp.* 537–546
Printed in Great Britain

SELECTIVE ATTENTION AND PERFORMANCE IN DANGEROUS ENVIRONMENTS*

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Evidence on human performance in dangerous environments is reviewed and suggests that danger reduces efficiency, except in the case of experienced subjects. Perceptual narrowing is shown to be one source of decrement. It is suggested that danger increases the subject's arousal level which influences performance by producing a narrowing of attention. The nature of the performance decrement and of adaptation to danger are discussed in this context.

The present review was stimulated by a series of experiments on the performance of divers at depth in the open sea. The original aim of these experiments was to study the effects on diver performance of nitrogen narcosis, the intoxication which occurs when air is breathed at high pressure. Discrepancies between open sea results and pressure chamber simulation occurred (Baddeley, 1966; Baddeley & Flemming, 1967), and it subsequently became apparent that degree of danger was a crucial variable (Baddeley, 1967, 1971; Baddeley *et al.*, 1968). What follows attempts to review the literature on performance in danger and to suggest one way in which a subject's information processing capacity may be impaired by fear.

Evidence

Anecdotal

Evidence about the effect of danger on human performance is of three main types, anecdotal, observational and experimental. These will be considered in turn. Perhaps the most abundant source of evidence is from first-hand accounts of behaviour in danger. Unfortunately such evidence is very difficult to evaluate since it is generally highly subjective, liable to forgetting and distortion during recall and by its very nature not replicable. Nevertheless, anecdotal evidence may prove useful both as a source of hypotheses to be tested by more rigorous means and as concrete illustration of independently established principles. A good example of this is found in the work of Radloff & Helmreich (1968) on Sealab II, the U.S. Navy underwater living project. They use verbatim quotes from interviews with divers to illustrate and amplify conclusions based on more objective procedures, and in doing so give a clearer and more vivid picture. Although useful as an expository device, anecdotes are obviously a dubious source of evidence since they depend on selection by the writer.

Observational

The performance of a soldier in combat is another source of evidence of the effects of danger. It has long been known that the probability that a soldier will use his rifle effectively in the heat of battle is considerably less than in training. For example,

* Based on a paper presented at the NATO Advanced Study Institute in Human Factors/Ergonomics at Mondello, Sicily, September 1969.

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after the battle of Gettysburg in the American Civil War, over 200 of the muzzle-loading rifles used were found to have been loaded five or more times without being fired, and one had been loaded 21 times without being fired once (Walker & Burkhardt, 1965). In this case inadequate training was blamed, but more recent reports based, for example, on the Korean war suggest that even with fully trained troops performance in action is much worse than performance in training (Egbert *et al.*, 1957, 1958).

Perhaps the most useful combat information, however, comes from the quantitative evaluation of performance on a complex weapon system in training and under varying degrees of danger. Such an analysis is performed by Walker & Burkhardt (1965) who relate performance to degree of combat stress for aircraft weapons involving various types of control. Their results are shown in Fig. 1.

E_s is the ratio of error in combat to error during training, and A, B, C and D represent different degrees of combat severity, namely: A, No losses whatever in combat. B, Consistent loss of 2 per cent of aircraft per raid. C, Consistent loss of 5 per cent of aircraft per raid, the limit of attrition warfare accepted by night-bombing forces in World War II. D, Consistent losses of 10 per cent of aircraft per raid, the limit of heavy combat which is not exceeded lightly for long periods. This is about the average loss rate of the German air force in the Battle of Britain.

The three lines represent different control systems. Line X represents acceleration control with about 1·25 sec. lag. The points come from a number of guided bomb systems, both U.S. and German used in World War II. The results show a decrement of up to 900 per cent as the combat situation becomes more and more dangerous, followed eventually by abandonment of the control. Line Y represents an acceleration control with negligible lag. While there is only one point, the Henschel glide bomb used in the Mediterranean, it appears that eliminating the lag from an acceleration control system greatly reduces its susceptibility to degradation under stress. Line Z represents a velocity control with a short lag, a task which Walker & Burkhardt (1965) suggest is like that of pointing a high-speed aircraft. The points are based on air-to-ground gunnery, air-to-ground rocket fire and dive bombing. Again there is some degradation in performance with increasing stress, but this is very much less than is found with systems involving lagged acceleration control.

As Walker & Burkhardt point out, however, degree of danger is by no means the only determinant of operational effectiveness. Level of motivation is clearly an important factor, a point which is illustrated by Walker & Burkhardt as follows. During World War II, public recognition (being awarded a medal) was usually determined on the basis of tonnage of bombs dropped in enemy territory. In the case of one system, the aircraft could carry only two-thirds the normal load, and in addition bombardiers were instructed to bring the bombs home if target visibility was poor, rather than drop them on alternative targets as was normally the case. Consequently the chance of receiving a medal with this system was only one-third the normal chance, a factor which it is claimed reduced enthusiasm and doubled the error rate. In contrast to this Walker & Burkhardt cite the case of dive bombing in Korea, where the results in 1953 were considerably better than in the previous year despite much stronger opposition. They attribute this to the order that in view of the serious military situation, planes were instructed that if they missed the target on the first

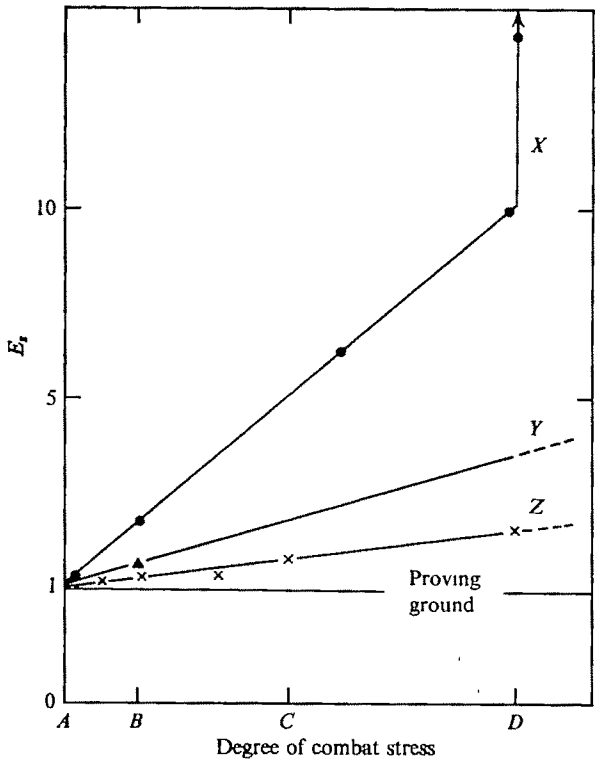

Fig. 1. Performance decrement as a function of degree of combat stress (see text for details): from Walker & Burkhardt (1965).

pass they must continue with second and third passes until the target was destroyed. Since their only chance of survival against really strong opposition was to hit the target on the first pass, they were very strongly motivated to make an accurate first shot. Combat data is inevitably difficult to interpret because of the many complicating factors of this type; nevertheless, in view of the paucity of quantitative data from really dangerous environments it provides valuable evidence.

Experimental

If we are to understand the effects of stress on performance fully, however, it is obviously desirable to carry out controlled experiments. A battlefield, however, is hardly an ideal place for experimentation. This has led to attempts to simulate the battlefield environment, and, provided one is prepared to go to sufficient lengths to deceive the subject, Berkun *et al.* (1962) have shown that it is possible to produce a simulated danger situation which will impair performance. They performed two experiments; in the first of these their subjects, servicemen, were taken on a plane flight during which an emergency was simulated. One of the engines stopped and subjects were then told that the plane's landing gear was faulty so that an emergency crash landing in the sea was probable. At this stage they were required to fill in two forms, one about disposition of their private belongings, the other a test of retention of emergency instructions, which it was explained was necessary to convince the insurance company in due course that the appropriate precautions had been taken.

The forms were intentionally badly designed to allow scoring in terms of errors. Not surprisingly these subjects made more errors than unstressed controls. The second experiment involved army recruits on a simulated tactical exercise. The subject was isolated except for a telephone link, and was subsequently led to believe that he was either in the path of a forest fire, in an area subject to intense accidental atomic radiation, was being shelled with live ammunition by mistake or that he had inadvertently blown up one of his comrades. All these situations required him to contact headquarters by radio. His radio, however, would not transmit. In order to repair it he had to follow certain instructions each requiring him to perform some task which was automatically timed and led on to another instruction. Physiological and self-rating measures of anxiety were subsequently taken. It was found that the simulated danger situations did produce anxiety and also tended to impair performance. Although these studies show that it is possible to simulate danger successfully, the problems raised by this type of experiment on both moral and practical grounds are considerable, and this does not seem to be a very promising line of development.

A second experimental approach is to take advantage of the fact that people do voluntarily subject themselves to real danger in such activities as parachute jumping, rock climbing and deep sea diving. A recent study by Hammerton & Tickner (1967) required army parachutists at three levels of practice to perform a tracking task involving velocity control well before, immediately before and immediately after jumping. They found no change in performance for experienced parachutists, but found a significant drop in pre-jump tracking efficiency for regular army trainees and an even greater decrement in the case of territorial army trainees, civilians who train on a part-time basis.

A further source of evidence comes from a series of studies of the performance of divers in the open sea. The first of these (Baddeley, 1966) was concerned with studying the effects of nitrogen narcosis on manual dexterity, and in particular in comparing performance in the open sea with performance in a pressure chamber simulation of depth. The task comprised a 6 × 12 in. brass plate with 32 holes, 16 of which contained nuts and bolts. The diver was required to transfer the 16 nuts and bolts from one end of the plate to the 16 holes at the other end. He was scored in terms of the time taken. In the open sea condition, divers performed the task three times in counterbalanced order, on dry land, and on the sea bed at depths of 10 ft. and 100 ft. The dry condition involved tests at comparable pressures in a dry chamber. Pressure chamber performance showed a significant but small effect of pressure (6 per cent), which is comparable in size to that shown in previous pressure chamber studies (Kiessling & Maag, 1962). In the open sea condition, however, the picture was quite different. Performing underwater caused a 28 per cent drop in efficiency, which when combined with the effect of pressure gave a drop of 49 per cent, considerably more than the 34 per cent which would be predicted by simply adding the pressure effect to that of being under water.

A similar exaggerated narcosis effect was found in a later study which compared the performance of divers breathing air with their performance breathing an oxy-helium mixture at a depth of 200 ft. both in the open sea and in a dry pressure chamber (Baddeley & Flemming, 1967). Similar results were obtained by Bowen *et al.*

(1966), who studied the performance of divers participating in Sealab II, the U.S. Navy underwater living project. Their divers were also working at a depth of 200 ft. and breathing an oxy-helium mixture. They showed no impairment in performance in a dry test performed inside the underwater house, but showed markedly poorer performance in the open sea than they had shown during prior shallow water tests.

Theoretical issues

In general then, these studies agreed in suggesting that a diver in the open sea will show a greater impairment in efficiency at depth than would be expected on the basis of pressure chamber experiments. While it is clearly of practical importance to know this, the possibility of explaining the effect theoretically seemed remote since the observed interaction might be due to any one or more of the many additional stresses that face a diver in the open sea. These include weightlessness, narrowed vision, cumbersome equipment, and possibly anxiety, cold and reduced illumination. An attempt to isolate the cause of the interaction by systematically manipulating these various factors did not seem very feasible. The picture changed dramatically, however, when a subsequent study under almost identical conditions failed to show the expected interaction (Baddeley *et al.*, 1968). Careful comparison of this with previous studies (Baddeley, 1967) suggested only one major difference, namely the apparent risk involved. This study involved diving from the shore under ideal conditions in a sheltered inlet with clear calm water, and with the sea bed sloping gently down to a sandy plain at 100 ft. The deep condition in the previous studies, however, had always involved diving from a boat in the open sea down into 'the blue', under conditions which were riskier than shore diving, and almost certainly provoked considerably more anxiety. In the case of the study by Bowen *et al.* we know from the work of Radloff & Helmreich (1968) that the divers involved in the project consistently reported being afraid, as indeed they should in view of the enormously hostile environment in which they lived and worked. Further evidence that the interaction effect depends on danger comes from the work of Adolfson (1967) who found no such exaggerated depth effect when divers were tested underwater in the well of a wet-and-dry pressure chamber, a situation involving many of the stresses of actual diving, but with little real danger.

Circumstantial evidence therefore points to anxiety as a crucial factor in the open sea performance of divers and points to the need for a more direct investigation which should include both physiological and performance measures. A recent study by Davis & Osborne (1970) has studied diver performance under relatively stressful conditions off Western Scotland, with simultaneous recording of a number of physiological measures of anxiety. It gave clear evidence of both diver anxiety and of the exaggerated drop in performance found previously under what were subsequently assumed to be stressful open sea conditions. They also noted that the most anxious divers tended to show the greatest impairment but unfortunately they did not have enough complete data for a correlational analysis.

On the basis of the data so far considered, it seems reasonable to suppose that danger may impair performance. This leads on to the question of how the impairment occurs, and in what way impairment can be minimized. There is very little direct

evidence available on this question. We are therefore left with two possible strategies, to wait until more empirical data has accumulated before attempting to theorize, or else to use data from related areas to produce a tentative theory which can then be used in designing further experiments. Research on performance in dangerous environments will almost inevitably involve working outside the laboratory, and as Radloff & Helmreich (1968, p. 210) point out, in field research, 'A researcher without a model will inevitably become lost in the frustrating complexities of the natural environment'. The speculations which follow represent an attempt to provide a partial model, and as such they are presented not as a final answer, but as a tool to assist further research.

There already exists a considerable literature on the effects of physiological arousal on performance, much of which suggests that they are related by a function resembling an inverted U, that is, as arousal increases, performance improves up to a maximum beyond which further increments in level of arousal lead to poorer and poorer performance (Hebb, 1955; Malmo, 1959). A good deal of experimental data can be accounted for in terms of the inverted U function, indeed one of its weaknesses as a theory rests on its ability to account for almost any result so long as the exact location on the inverted U of the task in question is not specified in advance. However, it is possible, as, for example, Corcoran (1965) has shown, to test the assumed function relatively rigorously, provided enough points along the arousal dimension are sampled. The situation is further complicated, however, by the fact that the peak of the inverted U occurs at quite different levels of arousal for different tasks (Corcoran, 1965). This is intuitively reasonable; it seems likely that level of arousal at which a man will run fastest will not be that at which he will, for example, be best at threading a needle. However, unless one has an objective means of assessing a task in advance, prediction of performance under stress becomes even more difficult. It seems unlikely that such an assessment can be made until we know what causes the inverted U relationship.

One possible explanation of the relationship lies in the suggestion made by a number of workers that an increase in arousal produces a narrowing of attention, with the subject concentrating more and more on the central features of the task and paying less and less attention to more peripheral ones (Easterbrook, 1959; Teichner, 1968). Perhaps the strongest experimental evidence for such a view comes from recent work by Hockey (1969, 1970*a*, *b*) on the effects of loud noise on performance. In one of his experiments, Hockey (1970*a*) required his subjects to perform a centrally located tracking task, while at the same time monitoring a series of six small lights distributed on either side of the central task at varying distances from the centre. Occasionally one of these lights would be briefly illuminated; if the subject detected this he pressed an appropriate response button. Subjects were tested both in continuous loud noise and in a quieter condition. Overall tracking performance was significantly higher in the noise condition than in the control condition which showed a decrement during the session. Detection scores on the peripheral task tended to deteriorate with increasing distance from the centre. Noise exaggerated this bias by improving performance on the central lights at the expense of peripheral lights. When no central task was required, noise improved detection performance (Hockey, 1969), suggesting that noise does not simply impair peripheral vision.

In a subsequent experiment, Hockey (1970*b*) showed that subjects missed more peripheral signals in noise because they regarded them as less probable than central signals, not simply because of their peripheral location. A comparable result was recently reported by Cornsweet (1969) who used threat of electric shock to increase level of arousal. The point is made particularly clearly in a further experiment by Hockey (1969) in which the subject was required to monitor three sources for occasional signals. Each source was checked by pressing one of three buttons and a signal comprised a dim light at the relevant source. The subject could check only one source at a time and the number of checks allowed was limited by the experimenter. Under these conditions, differences in signal probability between the sources tend to be reflected in the frequency of checking, with the sources bearing the greatest number of signals being checked most often. As predicted from the previous studies, when this task is performed in noise, there is an increase in the bias towards sampling the most probable source. Furthermore, when level of arousal is decreased by depriving subjects of a night's sleep the opposite effect occurs; tendency to sample the most probable source is reduced.

Although Hockey's results show a clear effect on breadth of attention of stresses that may reasonably be assumed to influence the subject's level of arousal, we have no direct evidence that danger will have such an effect. Evidence that this is in fact the case comes from a study by Weltman & Egstrom (1967) in which novice divers were required to perform a central task while monitoring a faint peripheral light. While the central task did not affect peripheral vigilance on the surface, during diving a distinct subgroup of the subjects emerged showing much slower responding to the peripheral lights, while showing no impairment on the central task. These subjects appeared to be more anxious than the other subgroup which showed no deterioration under water, but unfortunately no objective measure of anxiety was available. This defect was remedied in a subsequent study (Weltman *et al.*, 1971) in which a similar dual task was performed by naive subjects during a simulated 60 ft. dive in a pressure chamber. After an explanation of the potential dangers and emergency procedures, the door of the pressure chamber was bolted and a rise in pressure simulated, although actual pressure did not change. Experimental subjects showed a clear anxiety response in terms of both increased heart rate and subjective ratings. They also showed a clear decrement in detection of peripheral light signals but no drop in performance on the central task, relative to an unstressed control group.

It seems plausible to assume then that an increase in arousal will focus the subject's attention more and more narrowly on that aspect of the situation that is of greatest immediate importance to him. If this happens to be the task he is required to perform, then his efficiency will be increased. If not, however, his performance will deteriorate until he abandons the task.

Fortunately, however, response to a dangerous environment may be much more adaptive than this, especially as the Hammerton & Tickner (1967) parachutist study showed, if the subject has had considerable prior experience of the danger situation. Why should this be so? Common sense suggests that an experienced parachutist is less anxious because he is more competent and more confident in his ability. Some recent work by Epstein & Fenz (1965), however, suggests a more interesting answer.

ADAPTATION TO DANGER

Epstein & Fenz were interested in parachute jumping as an approach-avoidance conflict: the parachutist wishes to jump and yet he is afraid. In order to examine this they asked their subjects to rate both their keenness and their aversion to jumping at various points during the sequence of events leading to and following a jump. The novice parachutist became less and less eager to jump as jumping time approached, his reluctance reaching a maximum with the ready signal. From then on he showed increasing enthusiasm up to and including landing. Experienced parachutists gave an almost inverse picture with maximal avoidance ratings occurring on the morning of the jump and actually on landing, whereas their points of maximum approach occurred at the point of jumping. Physiological measures based on the galvanic skin response (GSR) supported the self-rating data, with novice parachutists showing a high response before jumping which fell to a more normal level on landing while the experienced parachutists showed completely the reverse effect. Unpublished work of my own has shown a similar effect on heart rate in divers, with novices showing a high pulse rate before the dive, which drops after the dive, whereas experienced divers tend to have a higher pulse rate after the dive.

Epstein & Fenz suggest that the experienced parachutist learns to inhibit anxiety since it tends to disrupt performance. They suggest that both the fear and the inhibition focus on the jump but generalize both temporally, to prior and later aspects of the jumping situation, and to other stimuli associated more or less closely with jumping. If one assumes the generalization gradient associated with the inhibition of fear to be steeper than that associated with fear itself, then the point of maximum emotional response will tend to be displaced away from the danger stimulus; the greater the degree of inhibition, the further away will be the displacement (see Epstein, 1962, for a more detailed discussion of this point). Further evidence in support of such a hypothesis comes from a study in which the GSR of parachutists was recorded on the day of a jump while they produced associations to various words, some of which were related more or less remotely to jumping (examples of words in increasing order of relevance are 'music', 'sky', 'fall' and 'ripcord'). For novices, magnitude of GSR increased monotonically with stimulus relevance, while experienced parachutists give their maximum GSR to remotely relevant stimulus words. Similar results were obtained when subjects were required to make up stories about pictures which varied in how closely relevant they were to the act of jumping (Epstein, 1962).

It seems then that subjects who are repeatedly exposed to a dangerous situation can in some as yet unspecified way learn to inhibit their anxiety and displace it away from the point of maximum danger. This is likely to be useful for two reasons, first because it prevents performance being impaired at a crucial time, and secondly because the displaced anxiety will tend to act as a warning of impending danger. Much of the process of training a diver seems to consist of exposing him to increasingly stressful exercises within the safety of a swimming pool. In the case of the British Sub-Aqua Club, this culminates in a test in which the diver's equipment is thrown into the pool and he is required to dive in, turn on the air in his aqualung and put all his equipment on without surfacing. The actual motor skills required by this

are relatively simple but the emotional control necessary to perform the task is quite considerable. In short it seems likely that such a course is mainly concerned, quite rightly, with teaching the diver an emotional skill. How this skill is achieved is far from clear. Subjectively, the feeling is one of focusing attention firmly on the matter in hand, possibly taking advantage of any narrowing of attention to shut out peripheral fears. Testing such a hypothesis, though not easy, should not prove impossible.

To return to the original problem, can the impaired performance of divers in the open sea be entirely attributed to an arousal-produced narrowing of attention? Such an explanation is adequate only on the assumption that nitrogen narcosis somehow increases level of arousal. This seems unlikely since at depths in excess of 300 ft. subjects are liable to become drowsy and lose consciousness, suggesting, if anything, a reduction in arousal. Subjects in the study by Baddeley & Flemming (1967) all rated themselves as more 'comfortable' at 200 ft. than at 10 ft., which does not suggest a simple increase in arousal due to fear. The same subjects when breathing oxy-helium at 200 ft. made the more rational judgement that they were less comfortable. It seems likely then that nitrogen narcosis has effects other than those due to a change in arousal. One of these is probably a general reduction in information-processing capacity. A similar effect is probably also responsible for the complex interaction of alcohol with other stresses noted by Wilkinson & Colquhoun (1968) and is discussed in a recent paper by Hamilton & Copeman (1970). Using the analogy of a searchlight, level of arousal will determine the breadth of the beam, while stresses such as nitrogen narcosis and alcohol may reduce the total power available.

Conclusion

To sum up then, it appears that one way in which danger affects performance is through its influence on the subject's breadth of attention. A dangerous situation will tend to increase level of arousal which in turn will focus the subject's attention more narrowly on those aspects of the situation he considers most important. If the task he is performing is regarded by him as most important, then performance will tend to improve; if on the other hand it is regarded as peripheral to some other activity, such as avoiding danger, then performance will deteriorate. With experience, subjects appear to inhibit anxiety in the danger situation and hence reduce the degree of impairment. We still do not know what mechanisms mediate the effect of arousal on the distribution of attention, or what is involved in the process of adaptation to fear. When we can answer these questions we shall be much closer to understanding human performance in dangerous environments.

I am grateful to N. K. Walker for permission to reproduce Fig. 1.

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(*Manuscript received* 13 *September* 1971)

Br. J. Psychol. (1972), 63, 4, *pp.* 547–553
Printed in Great Britain

THE INFLUENCE OF SHORT-TERM NOVELTY ON AFFECTIVE ASSERTIONS IN YOUNG CHILDREN

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It is a common finding that young children will express affective preference for short-term novelty. However, it is often not clear whether this is because they like the novelty or dislike the familiarity, or both. In this study, novel and familiar pictures were presented for binary choice under one instructional condition per group of subjects – either to indicate which one they liked or which one they did not like. The children liked the novelty but failed to 'not like' the familiarity. The results pose the problem of why the two affective expressions failed to act as opposites.

At certain ages children seem very ready to express individual affective preferences – to claim favourite toys, colours, etc. Whatever the causes of, and reasons for, this state of the organism, the result is a characteristic system of classifying objects and pictures. The interactions between that system and various environmental occurrences have formed the subject-matter of the perceptual-cognitive study of exploration as set out by Berlyne (1960). Indeed, by defining the *collative variables* (novelty, surprisingness, etc.), Berlyne has indicated that the *interaction* between the subject's classification and the characteristics of external objects is of central concern in the dynamics of exploration. Kagan (1967) has given a formulation of this type of position, and has argued for the usefulness of the concept of attention in exploratory interaction. The interest of this kind of formulation lies in its power to account for the temporary priority taken by transient (short-term) novelty in directing a whole complex of behaviour-patterns (cf. Cantor, 1970).

However, this is the point at which common agreement ends. Some theorists (e.g. Fowler, 1967) argue that a boredom-drive provides the major dynamic for exploration, with novelty acting as incentive. That is to say, although, logically, a stimulus must be classified correctly according to its novelty before it can arouse the appropriate drive, that the classification is to be regarded only as a sort of 'trigger' which sets off the drive-system. Others disagree and attribute a far greater role to the act of classification itself: '...incongruities between present inputs and past constancies in receptor stimulation could be expected to start operations of search for additional information' (Hunt, 1963, p. 54), and 'The measure of preference has the advantage of expressing, in part, the motivational aspects of cognitive uncertainty' (Munsinger & Kessen, 1966, p. 166).

We shall take as our starting-point the position indicated by Bruner (1957) when he says that 'all perceptual experience is necessarily the end product of a categorization process' (p. 124). In the case of classification of stimuli into novel and familiar there is evidence that affective processes are part of the 'experiencing' which follows classification, often a very large part indeed (Hutt, 1970). An example of the use of affective indices to cast light on classification of stimuli is given by Kagan (1967) when he discusses the relationships of smiling to visual fixation in infants. He concludes that smiling may be an index of perceptual recognition and may be sensitive

even where the more conventional measure of viewing-time is not. This enables him to interpret a decline in smiling in older infants as an index of expertise in assigning stimuli to a familiar classification. In the study reported here we shall measure the distribution of 'like' and 'don't like' responses to novel and to familiar stimuli in order to deal with the following problem.

There are many studies in which children show an affective preference for novel stimuli (Cantor, 1970). The problem is to decide whether this is primarily because they like the novelty, or dislike the familiarity (or both). (To use the terminology of O'Connell (1965), the former is a 'titillation' process, whereby novelty controls classification and the subject feels attracted to it, the latter is a 'tedium' process whereby familiarity controls classification and the subject feels repelled by it.) It is possible that both processes are operative, but there is a large body of evidence which indicates that processes based on novelty and familiarity may not be in *symmetrical* opposition. Thus, for example, Cantor (1968) asked 11-year-olds to rate stimuli on a line 12·8 cm long where 0 represented 'strongly dislike' and 12·8 represented 'strongly like'. He found that novel stimuli received a mean rating of 7·7 cm and familiar stimuli of 6·7 cm. This seems to indicate that novel stimuli were weakly liked and familiar ones were (perhaps) neutral. It certainly does not sustain his conclusion that the familiar stimuli were controlling the ratings by inducing 'tedium'. Very similar results were obtained by Cantor & Kubose (1969) using assertions of 'like' or 'don't like' in 5-year-olds: out of a possible score of 6, the novel stimuli elicited 4·8 'likes' and the familiar stimuli 3·9 'likes', both significantly above chance. In both studies it seems that responses were biased towards 'like' for both novelty and familiarity, but more strongly so in the former. Thus either novelty and familiarity are not in symmetrical opposition (perhaps because the subjects were making errors of omission of the familiar targets?) or 'like' and 'don't like' (or 'dislike') are not in symmetrical opposition for the children (perhaps by some incomprehension of the negative, or by yea-saying?).

The study reported here was designed to throw light on the foregoing experiments – and, in particular, on the utility of hypothesizing a connexion between novelty and liking. Children were asked to discriminate between novel and familiar stimuli using *either* 'like' or 'don't like'. The latter allows no room for yea-saying. The questions are (*a*) whether 'like' and 'don't like' function as opposites to indicate novel and familiar, respectively, and (*b*) which condition induces the best discrimination between novel and familiar, i.e. whether the children are more efficient at using an affective response based upon novelty (like) or one based on familiarity (don't like).

EXPERIMENT I

Method

Subjects

A class from a Co. Durham primary school served as subjects: it contained 34 children of average age 5;8, S.D. 3·0 months.

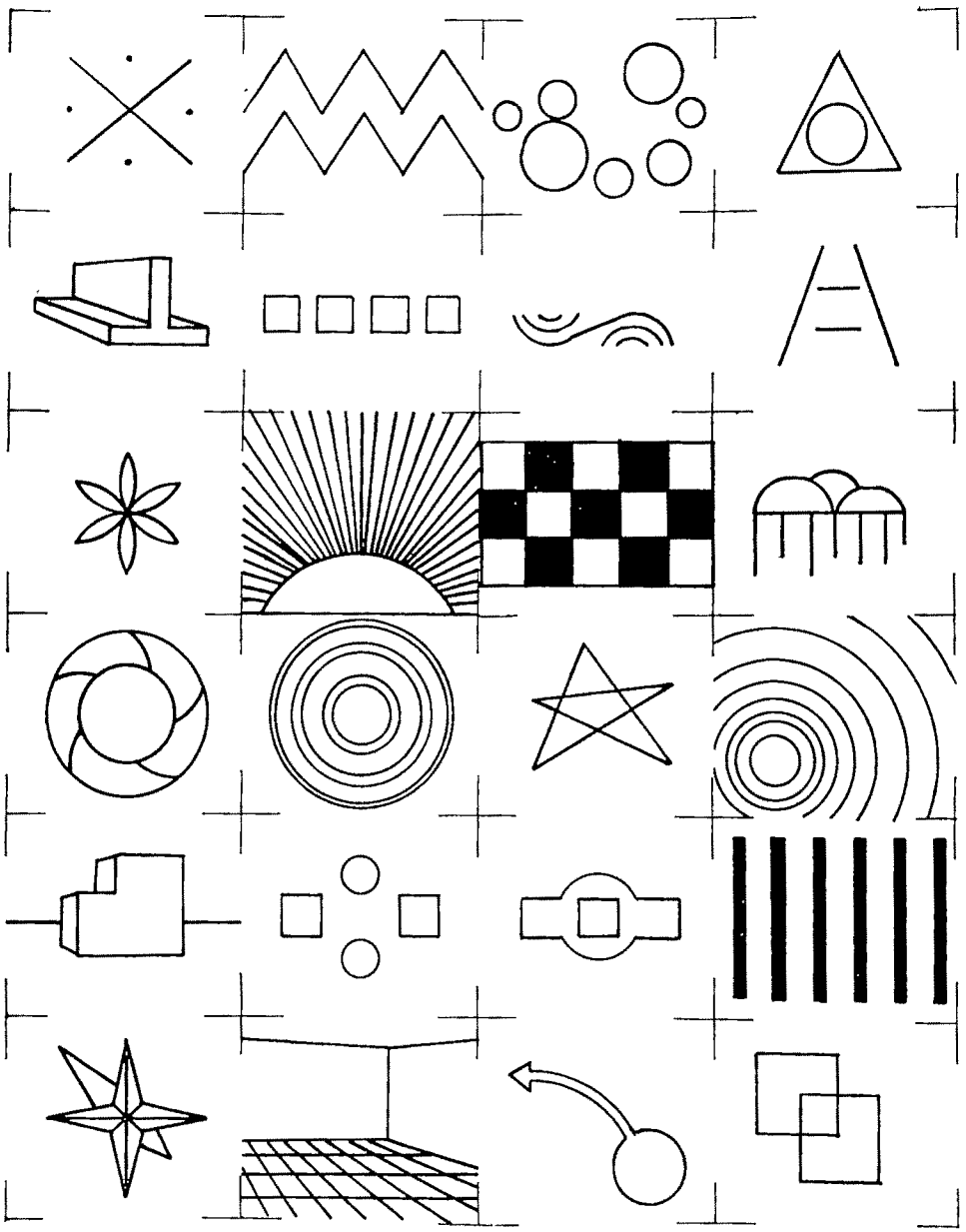

Fig. 1. Pictures presented to the children for affective and cognitive judgement. Seventeen of the pictures were redrawn by hand and reproduced from designs shown in the Welsh Figure Preference Test (1959) by special permission of Consulting Psychologists Press Inc. and G. S. Welsh.

Apparatus

Twenty-four 2 × 2 in. simple pictures were drawn. These are shown in Fig. 1. Only the first 17 are as used by Cantor, drawn from Welsh (1959). They were printed in black and white and pasted onto A4 paper, either singly or in pairs. These sheets were then stapled into books so that the first six pages contained single pictures, then came a blank page, then six pages in which the sequence of six pictures was repeated, each one paired with a novel picture. Half the books

contained the first 12 pictures shown in Fig. 1 and the others contained the second 12. Apart from that constraint no two books were alike.

Procedure

The children were tested individually at their school. After rapport had been established they were told that we had some pictures drawn by other children, and they were invited to turn over the pages one by one at their own pace and look at them carefully. Most subjects turned a page on average every 7 sec.: on the occasions when some fell below 5 sec. (or above 10 sec.) they were reminded either to look carefully (or that they could turn over when they wanted to). When the blank page was reached they were informed that 'now it is going to be a bit different' and they were invited to continue but now to point to the one of each pair which they (*a*) had seen before (SB) or (*b*) had not seen before (NSB) or (*c*) liked (L) or (*d*) did not like (NL). The question was repeated as each page was turned. Each child looked through two books chosen randomly from the two sets of pictures, under *one* of these instructional sets. This section of the study constitutes Test 1 and forms the main part of the investigation. After *each* book was looked at, each SB and NSB subject was asked to perform Test 2, in which he was given a sheet containing the 12 pictures just seen, and asked: 'Now that we have all the pictures together, are there any which you particularly like (do not like)?' He was encouraged to indicate three likes *and* three dislikes from the sheet: i.e. to perform six categorizations without the binary-presentation constraints of Test 1. Thus the range of conditions is as follows.

Test 1	Test 2
SB	L & NL
NSB	L & NL
L	—
NL	—

In Test 1 the *cognitive* groups SB and NSB will have had their attention drawn to the novel-familiar distinction. These Test 2 groups thus form a memory-tested comparison group for the *affective* groups L and NL from Test 1.

RESULTS

The percentage of choices which were of novel stimuli are shown in Table 1. The accuracy of discrimination under the cognitive instructions SB and NSB was 93 and 90 per cent respectively. Thus the pre-exposed pictures had been remembered and the negative instruction '*not* seen before' was clearly understood and used to good effect. It is equally clear that less novel/familiar discrimination was induced by the affective instructions L and NL, and that they did not function as opposites. Under NL, 54 per cent of the choices were of novel. Under L, 67 per cent of the choices were of novel (binomial P 0·02). This supports the hypothesis that novelty induces liking. However, the hypothesis is obviously weakened by the fact that the liking was not strong enough to drive 'not like' towards choice of the familiar stimuli. The failure to select familiar under NL is most unlikely to be due to incomprehension of the negative. The results are not due to selection patterns altering over trials: when the 12 individual trials were examined no sequential effects were found at group or individual level. The affective choices of groups SB and NSB gave totally non-significant results: the effects of novelty were confined to Test 1.

The results clearly do not support the hypothesis that familiarity induces dislike. On the other hand, the connexion between novelty and liking is not a simple one since although the children liked the novelty they failed to 'not like' the familiarity, i.e. under NL they chose novelty as 'not liked' on 54 per cent of the choices. This is consonant with the results of Cantor mentioned in the introduction. However, the

Table 1. *Percentage choice of novel stimuli under four instructional conditions in 5-year-olds*

Test	Condition			
	SB	NSB	L	NL
Test 1	7	90	67	54
Test 2 (SB)	—	—	44	56
Test 2 (NSB)	—	—	51	49

SB = seen before; NSB = not seen before; L = like; NL = do not like.

Table 2. *Percentage choice of novel stimuli under four instructional conditions in 6-year-olds*

Test	Condition			
	SB	NSB	L	NL
Test 1	2	99	73	55
Test 2 (SB)	—	—	35	65
Test 2 (NSB)	—	—	37	63

SB = seen before; NSB = not seen before; L = like; NL = do not like.

margin of error, as we saw from groups SB and NSB, could be up to 10 per cent. Therefore we undertook to repeat the study on a sample of children who would be likely to achieve 100 per cent accuracy under conditions SB and NSB.

Experiment II

Method

Subjects

A class of 36 children from a Co. Durham primary school, average age 6:9, S.D. 3·8 months, served as subjects.

Procedure

The procedure was exactly as for Expt. I.

Results

The percentage choices of novelty are shown in Table 2. Discrimination under SB and NSB was nearly 100 per cent and many of the children expressed restiveness at the task being so easy. Again novelty was chosen under L to a significant extent (binomial P 0·035) and again there was a slight bias towards novelty under NL.* No sequential effects were found when the 12 individual trials were examined. The results on Test 2 show three interesting points. First, there is a liking for familiarity. Secondly, the results from both SB and NSB are in the same direction. Finally, NL acts as the opposite of L. We shall take these points in order.

The binomial test is only significant for NL (P 0·02). However, taking the results in conjunc-

* In a replication with another class, of 6- to 7-year-olds, the percentage choice of novelty was 60 per cent under L and 54 per cent under NL. This is the third finding of a weak tendency to choose novelty as 'not liked' together with a stronger tendency to choose novelty as 'liked'. This evidence, together with the findings by Cantor mentioned in the introduction that familiarity is not disliked, must cast strong doubt on the associationist model of a simple connexion between novelty and liking being operative in American and English children.

tion, their directions are clear. There is a liking for familiarity and a dislike for novelty. The difference in liking from Test 1 to Test 2 is quite striking: it is apparent that Test 1 did not serve merely as a memory test but also affected preference. It is a symptom of weakness in current theories that there is no means of making quantitative predictions about such changes. The results from SB are the same as those from NSB, indicating that the effect of Test 1 did not depend upon specifically directing the subject's attention to novel or to familiar, so much as on directing his attention to the dimension on which both lie (or to the disparity between them). Finally, under these conditions, NL is the opposite of L, a finding which was absent from Test 1. However, as the subjects in Test 1 (NL) showed a weak bias towards selection of novelty, which is here shown more strongly, it may be questioned whether the apparent opposition of NL and L may not be due simply to the altering selection-bias under L? In this case the fact that the selection-bias under NL did not alter still points to *some* degree of independence of L and NL.

DISCUSSION

The basic problems are: how well the affective responses discriminated between novel and familiar, and how far 'like' acted as the opposite of 'not like'. In the first part of the study (Test 1) the children clearly demonstrated that they could remember the familiar pictures and that they understood the negative in 'not seen before'. The results from intra-group comparisons in the second part of the study (Test 2) showed that 'like' and 'not like' were treated as opposites, and that the children were consistent in their choices.

However, when we look at the details of the affective responses to novelty and familiarity we find that the results are quite complex. In Test 1 the novel pictures were liked. However, the familiar pictures were not 'not liked'; that finding weakens the hypothesis that novelty is 'preferred' since apparently the 'preference' was not strong enough to drive the children's indication of 'not like' towards the familiar stimuli of the paired comparisons under NL (and this cannot be accounted for by invoking the four artifacts of yea-saying, incomprehension of the negative, discriminative failure or differential sequential effects, since the first is controlled for by the design of using only one instructional set per group, and the middle two artifacts do not appear in the cognitive groups tested at the same time). Indeed in both classes there was a weak tendency to select the novel under 'don't like'; and therefore, *mutatis mutandis*, one cannot hold the complementary position that familiarity arouses negative affect. The results are consonant with those of Cantor mentioned in the introduction.

However, the evidence against the connexion between novelty and liking and that against familiarity and disliking are not equally strong, and one might argue that the use of an affective response to differentiate between novelty and familiarity actually involves two processes. First, the subject's *attention* is caught by the novelty (a cognitive classification) and then he emits an *effective response* of 'like' to indicate it. When the effective response is 'not like' the subject may have difficulty in switching to the familiar stimulus. Thus the intervention of a tendency to point to the novelty may be artifactually preventing the subject from registering dislike of the familiarity.* However, that explanation has two weaknesses. First, there is evidence from Test 2 of a successful use of L and NL as opposites to indicate a liking for familiarity and a

* This problem is being investigated, and relevant evidence has emerged which is currently being prepared for publication.

dislike of novelty (very surprising on this account in group SB which had had practice in pointing to the novelty). Secondly, if an orienting reaction and an affective reaction of liking are both involved in group L on Test 1, then we could reasonably expect a greater efficiency in discrimination than is acutally observed.

The results from SB and NSB on Test 2 are identical – a liking for familiarity – indicating that the cognitive responses in Test 1 had equivalent effects (perhaps simply to draw the subjects' attention to the difference between novelty and familiarity). There is at present no workable criterion available to identify procedural factors responsible for novelty or familiarity being liked (see, for example, the hypotheses advanced by Zajonc, 1968; Faw & Pien, 1971). Hopefully the differences in liking from Test 1 and Test 2 here obtained with the 6-year-olds may help towards studying such factors within a single study instead of between studies. We do not yet know why only the slightly older children should have shown Test 2 effects. Finally, the fact that the children discriminated between novelty and familiarity more efficiently under cognitive instructions than under affective instructions is consonant with the approach discussed in the introduction, whereby cognitive classification takes precedence over affective responses.

The research was supported by a grant from the A. J. Wheeler Trust, from which the author holds a Fellowship in the University of Durham. Thanks are due to the heads and teachers of St Hild's College C.E. Junior Mixed and Infant School and Neville's Cross Junior Mixed and Infant School and their unstinting help and cooperation. D. Hargreaves, N. Bolton and A. W. Still read the manuscript and made valuable criticisms.

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(*Manuscript received* 27 *July* 1971; *revised manuscript received* 12 *November* 1971)

Br. J. Psychol. (1972), 63, 4, *pp.* 555–559
Printed in Great Britain

INDUCTIVE THINKING IN CHILDREN OF PRIMARY SCHOOL AGE: THE 'DOOR AND KEY' TEST

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One hundred and twenty primary children aged between 4:11 and 11:0 were given a test of inductive thinking capacity. A sequence of developmental stages was observed similar to that in Wetherick & Freeland (1969) in a sample that was both larger and more representative. The most important change in performance level appears to take place at about age 7–7½. It is suggested that level of performance may depend on the capacity (increasing with age) of the short-term memory store.

Wetherick & Freeland (1969) put to the test a hypothesis suggesting that adult capacity for valid inductive inference develops in stages through childhood. They predicted 'a sequence of typical performances beginning with more or less random responses arising from an inability to grasp the nature of the task, passing to responses which are correct if the instances are positive but incorrect if the instances are mixed (positive and negative) since all the instances are in fact being treated as positive, and then to responses which are correct whether the instances are positive or mixed'. They also considered the possibility that subjects in the second stage might, depending on how near they were to passing into the third and final stage, be capable or incapable of learning to make the correct response to mixed instances given knowledge of results. They obtained data which supported the hypothesis, using a sample of 44 children aged between 4:8 and 10:5 and of average to above-average intelligence. The relatively small number of subjects made it impossible to cover the full range of either age or intelligence and was in part a consequence of the fact that the apparatus employed was clumsy and difficult to manipulate whilst retaining the interest of the subject. The apparatus was subsequently used to test a small sample of adult subnormals and a sample of ESN children (Wetherick *et al.*, 1969) but it became clear that work on a larger scale would require the development of a more convenient method of presentation.

The method here adopted was suggested by Miss M. E. Nuttall in conversation with the senior author. It has the advantage that the test material is easily portable and requires only a very simple form of instruction. The testing time was reduced by half to 15–30 min. (The wear and tear on the tester was reduced by more than half!) The method employs schematic pictures of doors which are either open (positive instance) or closed (negative instance). Each door has a keyhole, a square upper panel and either two or three vertical oblong lower panels which are coloured. The subject is provided with a card on which appear nine identical schematic pictures of keys each in a different colour and is told that each key opens any door that has the same colour on it. Thus, given that a man has only one key, if two open doors are presented the key must be the one whose colour appears on both doors; if one open and one closed door are presented the key must be the one whose colour appears on the open door but not on the closed door; and so on. The problems presented in the

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apparatus originally employed can be presented more conveniently by this alternative method. A precise analogy would, of course, have required the presentation with each problem of four closed doors, the subject being required to say which of them the man could have opened granted that he could open the two given doors or could open one door but not the other. However, the door-and-key format suggested a simplification of the procedure, following which the subject was required merely to choose a key, and this simplified procedure was adopted. Nine commercially available colours were used which seemed to offer the minimum possibility of confusion.

METHOD

Subjects

One hundred and twenty children aged between 4:11 and 11:0 took part (63 boys and 57 girls). All were pupils in either the infant or the junior department of a primary school in an urban area of west Yorkshire. No test data was available for these children.

The school was organized in six age-classes and one group (of 15 children) was chosen from each age-class except the third which contained the 7- to 8-year-olds. The previous study seemed to indicate that this age might be a crucial one for the principal change under investigation and so from this age-class two groups were selected, one of younger and one of older children. The eighth group was selected from a remedial class in the junior department. Details of the groups were as follows:

Group	*n*	Mean age	Range
A	15	5:4	4:11–5:7
B	15	5:11	5:9–6:2
C	15	6:11	6:9–7:0
D	15	7:6	7:1–7:10
E	15	8:6	7:10–9:0
F	15	9:5	9:1–10:0
G	15	10:5	9:9–11:0
Rem.	15	7:11	7:1–8:11

The 120 children tested constitute, approximately, a 20 per cent sample of the school population. Within each age-class except the first, children were selected for test as follows (the object being to make the sample as representative as possible of all levels of normal intelligence):

The head teacher of the department was asked to rate all the children as either average, above average or below average. Five children in each category were then selected at random for each age-class and their ratings checked with the class teacher. If the class teacher did not accept the head teacher's rating, the child concerned was discarded from the sample and a substitute chosen. The teacher's ratings appeared to relate mainly to general alertness and verbal fluency. Testing was done in the early weeks of the autumn term and teachers had consequently had no opportunity to get to know the children in the first age-class well enough to rate them. Here, therefore, children were selected at random. However, six of the children originally selected refused to accompany the tester to the test room and so, in this group, there is an element of self-selection by willingness to cooperate. The children in the remedial class were by definition 'below average' and the group was selected at random.

Materials

The same sets of problems were employed as in the previous study. The first set comprised six problems presenting two doors, each door having two coloured panels; the second comprised six problems presenting three doors, each door having two coloured panels and the third, six problems presenting three doors, each door having three coloured panels. In each set, three problems presented positive instances only (i.e. open doors) and three mixed instances (i.e. one open door and one or two closed doors). Examples are:

Example A. Open door with light brown and red panel; closed door with light brown and yellow panel (correct key: red).

Example B. Light brown, yellow, dark brown (open); light brown, red, dark green (open) (correct key: light brown).

Example C. Light green, orange, dark green (open); light green, orange, dark brown (closed); light brown, orange, dark brown (closed) (correct key: dark green).

Procedure

The tester presented the key card saying, 'I am going to ask you some questions about doors and keys. Here is a picture of some different coloured keys.... All the keys are of different colours and each key opens any door that has the same colour on it.'

The tester then presented a card showing two open doors. 'Here are two doors. The man had a red key and so he could open this door which has red on it and this door which also has red on it.'

He then presented a further card showing one open and one closed door. 'Here are two more doors. The man had a yellow key so he could open this door which has yellow on it but not this door which does not have yellow on it.'

After presenting two more demonstration cards the tester then proceeded: 'Now I want *you* to show *me* which key the man had. Remember each time he had only one key.'

When presenting each problem the tester said: 'He could open this door and this door...' or 'He could open this door but not this door...' as appropriate. 'What colour key did he have?'

He then recorded the subject's response, getting him to point to the key as well as name the colour if there was any doubt as to his capacity to name the colours correctly.

If the subject made any errors in the first series of problems the tester said: 'Now I want you to go through them again and this time if you make any mistakes I will tell you and you can try again.' When a mistake was made the tester said: 'No, that's not the key the man had, have another try.' And so on until the subject found the correct key.

The demonstration problems were insoluble as problems, since where two doors were open they had both colours in common and where one was open and one closed they differed in both colours. This prevented subjects from learning the correct procedure from the demonstration problems. Occasionally subjects pointed out during the demonstrations that the man might have had a different coloured key but if this happened the tester merely agreed, adding: 'But each time he only had one key.'

The test problems were unequivocal; there was only one possible key in each case.

Results

Table 1 shows the results obtained. Six levels of performance are distinguished.

Level 1 indicates correct performance in the first series (without knowledge of results). The child can perform the task at the normal adult level. In the previous study, level 1 behaviour was observed in the 8-year-old group and was characteristic of the 10-year-old group. Here, half group D performed at level 1, and the proportion increased with age. As might have been expected, the larger sample included one or two exceptional younger children; one in group A (aged 5:7) made no errors and went on to explain the principle of the test to the tester; one of the two in group B (aged 6:2) made errors in the first series which appear to have resulted from confusing light and dark shades of the same colour. In the second series he lost heart and guessed at random but careful analysis makes it clear that he made no errors of inference in the first series and his performance has been classified accordingly.

Level 2 indicates correct performance in the second series (with knowledge of results) following errors in the first series. The child does not naturally perform the task at the normal adult level but can very quickly learn to do so given knowledge of results. In the previous study, level 2 behaviour was characteristic of 8-year-olds; all the children who fell into this category made what were called Type 1 errors in the

Table 1. *Age and level of performance of primary school children on the 'Door and Key' test* (n = 15 *in all groups*)

Age group ...	A	B	C	D	E	F	G	Rem. group
Mean age ...	5:4	5:11	6:11	7:6	8:6	9:5	10:5	7:11
Performance								
Level 1	1	2	–	7	9	11	12	1
Level 2	1	–	6	2	3	1	2	1
Level 3	3	1	–	2	1	2	–	4
Level 4	2	5	7	2	–	1	–	3
Level 5	–	1	2	2	2	–	1	6
Level 6	8	6	–	–	–	–	–	–

first series, they treated mixed instances as if they had all been positive. Here, level 2 behaviour is mainly characteristic of group C. Eight children so classified made Type 1 errors in the first series, five chose by position and three at random.

Level 3 indicates liability to Type 1 error in both series with no improvement following knowledge of results. Here, as in the previous study, level 3 behaviour is to be found in most groups.

Level 4 indicates choice by position, with no improvement following knowledge of results. In the previous study, level 4 behaviour was observed only in a few 8-year-olds but the format employed here offers a new semi-rational basis for choice by position in so far as one colour must necessarily be next to the keyhole. Most of the level 4 behaviour involved selection of the colour next to the keyhole in the door on the left. This kind of behaviour was mainly characteristic of groups B and C.

Levels 5 and 6 indicate non-rational choice. In the present study the child had in front of him a card on which appear various coloured keys. Level 6 indicates choice among the keys with no reference to the doors and has no exact analogy in the previous study. Level 5 indicates apparently random choice among the colours on the doors. Non-rational choice was characteristic of groups A and B and the Rem. group in the present study and of the youngest groups in the previous study.

DISCUSSION

The 'sequence of typical performances' obtained was as follows. The youngest children (groups A and B) did not grasp that there was a soluble problem before them. They were asked to choose a key and they did so. Frequently they followed an ascertainable strategy, e.g. 'Round the keys clockwise starting top left', but the strategy was related to the choice of key, not to the solution of the problem. Random choice was rarely seen and may indicate merely that the subject was employing a strategy that the tester had not grasped. The youngest children are still to some extent under the control of their environment. They respond to what is perceptually salient but already they show evidence of awareness that the environment is ordered and that ordered responses pay off. In the slightly older child this awareness results in semi-rational position choices. The child now actively tries to solve the problems and the problems could conceivably have been constructed in such a way that position choices were invariably correct. In group C position choice is the dominant

mode of response – even more dominant than may appear, since four of the six level 2 performers in this group progressed from position choice to all-correct performance with knowledge of results and there were in addition seven level 4 performers, not yet ready to learn. Children in group D and above have in the main learned to make the correct inferences, though some of them can handle sets of positive instances but not as yet sets of mixed instances. All-correct performance becomes progressively more common as average age increases. The remedial group perform, predictably, at a lower level than normal children of the same age.

This sequence differs from the hypothesized sequence (and from the sequence obtained in the earlier study) mainly in the dominance of position choices in groups B and C. This feature of the results may plausibly be related to the method of presentation adopted and is perhaps the price that has to be paid for the convenience of the method. The child of 6–7 is looking for a basis for prediction and the format suggests to him a basis which is, in fact, false. Older children are not, however, misled. There was some indication that younger children tended to regress to lower performance levels when faced with more difficult problems but too few problems were employed at each level of difficulty for this to be demonstrable.

The transition between groups C and D is abrupt, as was the transition in the previous study between 6- and 8-year-olds. Groups C and D, however, differ in average age only by about 7 months. At age 7 semi-rational position choice is the dominant mode of response, though some children are ready to learn; at age 7½ all-correct performance has become the dominant mode. It is not easy to think of any other major change in behaviour that occurs at that age. There is, for example, no sudden increase in the child's linguistic competence. Wetherick (1968) has suggested that gradually increasing short-term memory capacity may be responsible. This would imply that at about age 7–7½ short-term capacity has reached a level at which inductive inference from positive or mixed positive and negative instances becomes feasible. An explanation in similar terms has been proposed by McLaughlin (1963) for Piaget's developmental stages and by Graham (1968) in relation to language ability.

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(*Manuscript received* 9 *September* 1970; *revised manuscript received* 5 *April* 1971)

Br. J. Psychol. (1972), 63, 4, *pp.* 561–568
Printed in Great Britain

A HIERARCHICAL MEASURE OF COGNITIVE COMPLEXITY

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A new hierarchical measure of cognitive complexity based on the repertory grid is described. This measure is considered to be both conceptually and empirically an advance over previous measures. It emphasizes that the structure of a subject's construct system is important, rather than merely its degree of differentiation. In this respect, it has advantages over previous measures based on the repertory grid. Three experiments are described which test the validity and reliability of the new measure. These experiments show that the hierarchical measure is reliable and is unrelated to a measure of differentiation based on the repertory grid. The hierarchical measure is shown to be positively related to performance on Harvey's 'This I Believe' test, which has been used as a projective measure of cognitive complexity. The hierarchical measure is also shown to be unrelated to performance on a verbal reasoning task.

Much of the research on cognitive complexity has been generated within the framework of Kelly's Personal Construct Theory, the basic assumption of which is that each individual evolves a hierarchically organized system of personal constructs to interpret and predict events. The concept of cognitive complexity was introduced by Bieri (1955). Following Bieri (1966), we may define cognitive complexity as 'the tendency to construe social behaviour in a multidimensional way, such that a more cognitively complex individual has available a more versatile system for perceiving the behaviour of others than does a less cognitively complex person'.

The various attempts to operationalize this concept may be conveniently grouped into two categories. First, there are the measures based on the repertory grid, which provides a direct methodological link with Kelly's theory. The second category includes those measures based on projective techniques.

Repertory grid measures of complexity

The repertory grid is a very attractive research tool. Subjects make a list of people known to them (the elements) and are then required to make judgements about these people, using their own constructs. A detailed description of the technique and its variations, as well as a thorough discussion of the theoretical background, is given by Kelly (1955) and Bannister & Mair (1968). The major advantage of the repertory grid is that it provides a standardized judgemental framework within which the subject has the maximum possible freedom to respond. This simplifies in a realistic way the difficult task of making comparisons among subjects.

The use of the repertory grid in investigating cognitive complexity was initiated by Bieri (1955), who used a standard dichotomous judgement grid. The extent to which two constructs were used in a similar manner was ascertained by counting the number of elements having identical ratings on the construct. The total matching score for all pairs of constructs defines Bieri's measure of complexity. A subject with

a high matching score uses all his constructs in a similar manner, and so is deemed cognitively simple, while a cognitively complex subject has a low matching score. A related measure, this time involving the rating scale form of the repertory grid, was used by Tripodi & Bieri (1963, 1964). Tripodi & Bieri (1963) also showed that similar results could be obtained using provided constructs rather than constructs elicited by the subject. Another approach, used by Bieri & Blacker (1956), is to count the number of verbally different constructs used by the subject. These measures of cognitive complexity, as well as other related measures, are discussed in greater detail by Crockett (1965) and Bonarius (1965).

The measures described above should more properly be called measures of differentiation, since they are only concerned with one aspect of cognitive complexity. Thus Warr (1970) has included Bieri's measure in his category of measures of 'dimensional differentiation'. That differentiation is not a sufficient condition for cognitive complexity is indicated by the finding that thought-disordered schizophrenics have very differentiated construct systems (Bannister & Mair, 1968) and therefore would be deemed cognitively complex. These authors point out the necessity of distinguishing between complexity and confusion. A comparable criticism of the use of indices of differentiation in assessing cognitive complexity is given by Makhlouf Norris *et al.* (1970), who show that obsessional patients may have a much simpler conceptual structure than normal matched controls, even though the differentiation scores of the patients and their controls are similar. Thus we may conclude that, while the repertory grid is a potentially powerful research tool, no adequate measure of cognitive complexity has yet been derived from it.

PROJECTIVE TECHNIQUES

The second type of measure that we shall consider here involves the use of open-ended questionnaires and similar techniques. For example, Harvey's 'This I Believe' (TIB) test (Harvey, 1966) requires the subject to complete in two or three sentences the phrase 'This I believe about —', the blank being replaced by such items as friendship, marriage, myself, the American way of life. The sentences are then scored in terms of the abstractness or concreteness of their content. The more abstract subjects were found to be more cognitively complex than the more concrete subjects, using a repertory grid measure of complexity devised by Campbell (1960) which is similar to the Bieri measures described above. Schroder *et al.* (1967) have used a similar paragraph completion test, together with a refined scoring procedure. It is also possible to use their scoring system to assess the complexity of essay answers to examination questions. Nidorf (1961) and Streufert & Driver (1967) have used an impression formation task, where the subject writes a characterization of a person described by a set of potentially contradictory traits. Nidorf used a four-point scoring system which he subsequently collapsed into the two categories of integration and non-integration. Streufert & Driver attempted to score the characterizations both in terms of integration and of differentiation. They have accordingly developed a five-point scale of integration and a four-point scale of differentiation. The complex subject will be high in both integration and differentiation. A more detailed description of projective measures of cognitive complexity is given by Crockett (1965).

The major problem with these projective techniques is that, while they may adequately operationalize the concept of cognitive complexity, they are very difficult to score. Even Nidorf's four-point scoring system is difficult to use in practice. A finer scoring system, such as that proposed by Streufert & Driver, would be very difficult to use. Apart from the problem of training raters to use the system, subjects frequently write characterizations which are not scaleable, as for example the subject in one of our experiments who wrote, 'Should be a politician', in response to a request to characterize a person described by the adjectives trustworthy, sincere, honest, conceited, pompous and bigoted. No scoring system can cope with this sort of response.

The projective measures have been more successful in operationalizing the concept of cognitive complexity than the measures based on the repertory grid. The reason for this is that they have attempted to take into account the integration of the constructs used by the subject and not just the degree of differentiation. In this sense, they have managed to solve the problem of distinguishing between complexity and confusion, posed by Bannister & Mair (1968). However, as we have seen above, the projective measures are difficult to use in practice, and so an adequate means of operationalizing the concept of cognitive complexity has yet to be found.

In the present paper we shall discuss a more adequate measure of cognitive complexity, based on the repertory grid. For the reasons discussed above, an adequate measure needs to be objectively scorable in a straightforward manner. The structured task provided by the repertory grid allows the possibility of treating all subjects' responses in the same way. At the same time the repertory grid allows the subject a great deal of freedom in the judgements he makes, and also has considerable face-validity from the point of view of the subject. We have therefore chosen to modify the repertory grid measures of cognitive complexity, rather than attempting to improve on the conceptually more adequate projective measures.

A HIERARCHICAL MEASURE OF COGNITIVE COMPLEXITY

Procedure

The basis of the measure to be described is the rating scale version of the repertory grid. The size of the grid is very important: it should be sufficiently large to sample adequately both the people to be judged and the constructs used by the subject. At the same time, if it is too large the task becomes very difficult for the subject. An adequate compromise is to use a 13×13 grid, with the subject judging 13 people on 13 constructs. This generally takes just over half an hour to complete and subjects find it very interesting.

The subject first chooses 13 people he knows, including himself. A role list is given for guidance and to emphasize that the subject should choose both people he likes and people he dislikes. The subject is then given a 13×13 matrix, the columns of which are labelled from 1 to 13 and refer to the people he has chosen, and the rows of which refer to constructs. Each row of the matrix has three cells circled. The cells which are circled are selected in advance on the basis of half a balanced incomplete block design (see Cochran & Cox, 1957), to allow each pair of cells within each row a relatively equal chance of occurring together. The rows of the design are then

rearranged in random order. The subject is instructed to look at the first row and consider the three people corresponding to the circled cells, and to think of one important way in which two of these people are the same and yet different from the third. He is then asked to provide verbal labels for both sides of this construct which may be meaningful only to him, and to write these labels alongside the row. He then rates all 13 people on this construct using a five-point scale from $+2$ to -2, the positive end referring to whichever pole of the construct he wrote on the left. This procedure is followed until 13 constructs have been elicited and the people have been rated on them. The subject is asked not to use constructs referring to physical characteristics (e.g. red hair versus black hair) and to try not to repeat the same construct.

Analysis

A 13×13 correlation matrix for the constructs is drawn up, the elements of which are the absolute values of the product-moment correlations calculated between all possible pairs of rows in the grid. The magnitude of a correlation in this matrix gives a measure of the similarity of the two constructs to which it refers. A similar 13×13 correlation matrix for the people is drawn up. The elements of this matrix are the signed product-moment correlations between all possible pairs of columns in the grid. The algebraic size of a correlation in this matrix gives a measure of the judged similarity between the two people to whom it refers.

If we consider the correlation matrix for the constructs, we could simply find the average of all the intercorrelations. However, such a measure of differentiation is inadequate to convey what we mean by cognitive complexity, as Bannister's research with thought-disordered schizophrenics indicates. Cognitive complexity must be considered as a characteristic of the structure of the construct system rather than simply the average degree of interrelationship of the constructs. If we accept this point, we have two major problems to solve in our analysis of the grid. First, how can we discover the structure of a subject's constructs, and secondly, how do we quantify this?

The first problem can be solved fairly easily if, following Kelly (1955), we assume that personal construct systems are hierarchically organized. With this assumption, a natural way of analysing the grids is to use a hierarchical clustering technique. Accordingly, the correlation matrix for constructs is now analysed using Johnson's cluster analysis (Johnson, 1967), which provides a hierarchical picture of the way in which the constructs group together.

Our second problem, that of quantifying the hierarchical structure, is not so easy to solve. However, it is possible to circumvent this difficulty by inferring the importance of structure in a subject's construct system rather than measuring it directly. We first look at the relationships between the people judged by the subject, which we can assess by means of cluster analysis of the correlation matrix for people. The basic idea of the measure proposed here is to see what happens to the relationships between the people when we impoverish the subject's construct system. It is hypothesized that the fine details of the construct system will be more important for a complex subject than for a simple subject, so that impoverishing the structure will have a more dramatic effect on the relationships between the people for the more complex subject.

For this purpose, we can consider all constructs which interrelate above a criterion level* as being equivalent, and as such they may be collapsed into a single construct. We can now reanalyse the relationships between the people using this impoverished construct system, and determine the difference in the groupings of people between the first analysis and the second. If this difference is large, then the subject is deemed to be cognitively complex, and if the difference is small, the subject is deemed to be cognitively simple. The measure of difference in groupings is based on a non-parametric node count which is described in an unpublished appendix to this paper.† The appendix also discusses the above analysis in greater detail and provides two examples of the analysis from Expt. II below.

This procedure provides a rank ordering of the subjects within a sample in terms of cognitive complexity. A number of experiments have been carried out to assess the reliability and validity of this structural measure of cognitive complexity, and these are described below.

Experiment I

Method

Subjects were given Harvey's 'This I Believe' (TIB) test. Harvey proposes four major cognitive modes of functioning, which he labels Systems 1, 2, 3 and 4. (See, for example, Harvey, 1967.) For present purposes, we shall only consider the two extreme categories represented by System 1 and System 4. System 1 is the 'most concrete mode of relating to and construing the world' (Harvey, 1967), whilst System 4 is the most abstract of the four systems. We would therefore expect subjects in System 4 to be more cognitively complex than those in System 1. Two independent raters scored the TIB test for each subject, and on this basis, 13 subjects were classified under System 1 and 14 subjects were classified under System 4. These 27 concrete and abstract subjects were given 13 × 13 repertory grids to complete.

In addition to the hierarchical measure of complexity proposed above, it is also possible to compute Bieri's measure of differentiation (Tripodi & Bieri, 1964) from the grids, by comparing the ratings for all possible pairs of constructs. In comparing two constructs, a score of 1 is given for every exact agreement of the two ratings for any one person. The total score summed over all 78 possible construct comparisons defines Bieri's measure of differentiation. These scores are inverse indicators of differentiation: the lower the score, the more differentiated is the subject's construct system.

Results

We shall first consider the relations between the two measures of complexity computed from the grids. Spearman's rank correlations were calculated between these measures. The correlation between the hierarchical measure of complexity and Bieri's measure was 0·10, thus indicating that the structural measure is looking at a different aspect of the construct system from the measure of differentiation.

Having seen that the two measures are unrelated, we can now assess their relationships with the TIB test. In order to do this, Mann–Whitney tests were carried out between each of the measures and the TIB classification for each subject. These tests indicate that subjects who are cognitively complex on the hierarchical measure tend

* For the work described here we have arbitrarily chosen the 0·05 level of significance of a product-moment correlation as the criterion level beyond which to impoverish the construct system. There is no reason why any other level should not be used. We are at present investigating the feasibility of continuously varying the criterion level.

† Copies of the appendix are available on request from Chris Leach, MRC Social and Applied Psychology Unit, University of Sheffield.

to be classified as abstract on the basis of their TIB test, while simple subjects are concrete on the TIB test ($U = 50$, $P = 0{\cdot}05$). No such relationship exists between Bieri's measure and performance on the TIB test ($U = 64$).

EXPERIMENT II

This experiment is concerned with the generality of cognitive complexity across domains of content. This is an important problem, since, if generality does exist, then cognitive complexity may be considered as a personality trait, rather than a specific mode of functioning within a particular content area. For previous grid measures, a generalized notion of cognitive complexity has been found not to hold (see, for example, Bannister & Mair, 1968; Crockett, 1965). In this experiment, we shall also be looking at the relationship between complexity and intelligence.

Method

Forty-two subjects were given 13×13 repertory grids to complete in the standard way described above, using people as the elements. They were then given Baddeley's three-minute reasoning test (Baddeley, 1968), which is a test involving the understanding of sentences of various levels of syntactic complexity. This test correlates positively with the British Army verbal intelligence test ($r = 0{\cdot}59$) and is reliable and easy to administer. The subjects were finally asked to select a topic of particular interest to them, such as pop groups, authors, novels, and so on. They then selected 13 elements based on this topic, and completed an identical repertory grid using these 13 elements in place of the 13 people previously judged. The data from this experiment also allow us to repeat our test of the relationship between the two measures of complexity.

Results

For this experiment there is a significant negative correlation between hierarchical complexity and Bieri's measure (rho $= -0{\cdot}34$, $P < 0{\cdot}05$).

The hierarchical measure was found not to generalize across content domains. The correlation between the two types of grid was $-0{\cdot}04$. For Bieri's measure, however, there was a significant positive correlation between the two types of grid (rho $= 0{\cdot}35$, $P < 0{\cdot}05$).

Finally, neither of the measures correlates significantly with performance on the reasoning test. Reasoning scores correlate 0·29 with the hierarchical measure and 0·09 with Bieri's measure.

EXPERIMENT III

Method

This experiment is concerned with the reliability of the hierarchical measure of cognitive complexity. Fifteen subjects were given a 21×14 repertory grid to complete, judging 21 people on the basis of 14 constructs. The subjects returned one week later and completed a second 21×14 grid using the same 21 people as elements. This experiment provides a further comparison of the two measures of complexity, as well as a test of the reliability of each of them.

Results

For both the first and second grids, hierarchical complexity was found to be unrelated to Bieri's measure (rho $= -0{\cdot}10$ and $-0{\cdot}26$, respectively).

The hierarchical measure was found to be reliable (rho $= 0{\cdot}76$, $P < 0{\cdot}01$), as was Bieri's measure (rho $= 0{\cdot}46$, $P < 0{\cdot}05$).

Discussion

In deriving the hierarchical measure of cognitive complexity, we attempted to improve on previous methods of analysing repertory grids by increasing the content validity of the measure. We noted that differentiation alone provided only a limited means of operationalizing the concept of cognitive complexity, and we therefore tried to build the notion of structural differences between subjects into our measure. We argued that the measure derived was conceptually more adequate than the previous measures. The three studies reported above have tested the empirical adequacy of the hierarchical measure.

That a different aspect of the subject's construct system is being investigated by the hierarchical measure is indicated by the four tests of the relationships between this measure and Bieri's measure of differentiation. Three of the tests indicated a negative correlation between hierarchical complexity and Bieri's measure, although only one of these was significant. We might therefore conclude that subjects who have differentiated construct systems according to Bieri's measure show a slight tendency to be cognitively simple on the hierarchical measure.

Expt. I indicated that hierarchically complex subjects tended to be more abstract on the TIB test, while no such relationship was found for the measure of differentiation.

Expt. II suggested that the hierarchical measure of complexity did not generalize to other content areas. Bieri's measure, on the other hand, did generalize across content areas. This latter finding is, unfortunately, not very helpful, since it merely adds to the confusion already in the literature about the generality of Bieri's measure (see, for example, Crockett, 1965, for a discussion of this point). The present experiment mainly serves to emphasize the difficulty of making a completely adequate test of generalization. In this experiment subjects chose many different topics, from motor cycles and religions to earth features. It is likely that some of these areas allow the subject to use a more standardized construct system than others. For example, a consideration of the differences between earth features is more likely to lead to the use of the same constructs for different individuals than is a topic such as novels. For this reason, the test of generalization used in Expt. II leaves a lot to be desired. On the other hand, it is not possible to by-pass this difficulty by requiring all subjects to make judgements about the same topic, since there will be confounding differences between subjects in terms of interest in the topic and knowledge about the topic. At a practical level, some subjects may not be able to select the required number of elements within the given topic. The simplest way around this difficulty would be to use subjects who are matched for interest and knowledge about a particular content area.

We also found in this experiment that neither measure of complexity was related to reasoning ability. It is important to ascertain whether the concept of cognitive complexity could simply be considered synonymous with intelligence. The results of this experiment, as well as others (for example, the experiments by Mayo, 1959, Sechrest & Jackson, 1961, Rosenkrantz, 1961, discussed by Crockett, 1965), suggest that this is not the case. Obviously, at the low end of the intelligence scale we would expect subjects to be functioning in a relatively cognitively simple manner. The present results suggest that, at least for a university population, there is no direct relationship between cognitive complexity and intelligence.

Finally, in Expt. III we observed that the hierarchical measure and Bieri's measure were reliable. This latter finding replicates the previous result of Tripodi & Bieri (1963) who reported a large test–retest reliability (rho = 0·76, $P < 0·001$) using the subject's own constructs.

This work was financed in part by an SSRC grant. We are grateful to Peter Warr for his help throughout the course of this work, and to Thomas Green for discussing some ways of comparing hierarchical structures. The help of Carol Porter and Peter Warr in administering and scoring the TIB test is also gratefully acknowledged.

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(*Manuscript received* 15 *April* 1971; *revised manuscript received* 15 *March* 1972)

Br. J. Psychol. (1972), **63**, 4, *pp.* 569–571
Printed in Great Britain

EXTRAVERSION AND THE VISUAL FIGURAL AFTER-EFFECT WITH TACHISTOSCOPIC PRESENTATION

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It may be argued that the failure of previous studies to find a significant relationship between extraversion and visual FAEs lies in their use of inspection times of 'intermediate' duration, where in fact the theory predicts no difference between extraverts and mtroverts. In the present study, where very short inspection times were used, the hypothesis that extraverts show larger FAE than introverts was not supported.

Eysenck (1962) has suggested that the failure to find any significant relationship between Introversion–Extraversion and visual figural after-effects (FAE) may be explained by the hypotheses that extraverts (1) experience greater difficulty in maintaining visual fixation than introverts (Franks, 1963), and (2) develop greater inhibition of sensory input during inspection, which affects the actual strength of perceptual stimuli. A consequence of this second hypothesis is that extraverts should show greater FAE at short inspection times, introverts should show greater FAE at long inspection times, and at intermediate inspection times there should be no difference between extraverts and introverts (Eysenck, 1962). Previous studies have used inspection times in the range 30 sec. to 2 min., and it may be argued that these inspection times fall in the 'intermediate' range. The purpose of the experiments reported here is to investigate the hypothesis that extraverts show larger FAE than introverts at inspection times that are unquestionably short (500 msec.). An added advantage of using tachistoscopic presentation is that it reduces possible differential effects of difficulties in maintaining fixation.

Method

Subjects were selected from a total population of 780 first-year psychology students on the basis of their scores on the Eysenck Personality Inventory (Form A). For the first experiment ($n = 90$) nine groups of five men and five women each were obtained by sampling three levels of E and three levels of N orthogonally. In the second experiment ($n = 40$) only the two extreme levels of E and N were sampled, with a further five men and five women in each of the four groups. The three levels of E were bounded by the scores 1–6, 10–12, and 17–23; the three levels of N were bounded by the scores 2–5, 10–12, and 17–23. Thus the intermediate groups had scores very close to the mean scores of Eysenck's normative student population, and the extreme groups were located completely outside the $M \pm 1$ S.D. range for the same normative population. No subject was selected with a Lie score of more than 3, and all subjects were naive with respect to FAE.

The pattern used in both experiments was the concentric circles FAE (Usnadze effect) with an inspection figure/test figure size ratio of 1·5, where the predicted FAE is a shrinkage in the apparent size of the test figure. In Expt. I the inspection figure (IF), test figure (TF) and comparison figure (CF) circles were bright outlines (4·5 ft. l.) on a dark ground (0·025 ft. l.), presented in a three-projector tachistoscope. IF was 60 mm (3° 36′) diameter, 35 mm (2°) to the left of a fixation point, the constant size TF circle, 40 mm (2° 17′) diameter, was concentric with the locus of IF, and the variable CF circle, 35 mm right of the fixation point, varied in 2 mm steps

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from 20 mm to 60 mm diameter. In Expt. II the IF and TF patterns were black lines on matt white cards, field brightness 17·8 ft. l., presented in a Scientific Prototype GB three-channel tachistoscope. IF was 45 mm (2° 8′) diameter, 32·5 mm left of the fixation point, the constant TF circle was 30 mm (1° 26′) diameter, and the diameter of variable CF ranged from 20 mm to 40 mm in 1 mm steps.

Inspection time for IF was 500 msec. in both experiments, and the duration of the TF presentation was 100 msec. in Expt. I and 250 msec. in Expt. II. In both experiments the control condition was presentation of the TF + CF pairs alone. In Expt. I the FAE (IF followed by TF + CF) was measured with an inter-stimulus interval of 1 sec., and in Expt. II the FAE and the retroactive (R) FAE (TF + CF followed by IF) were measured with an inter-stimulus interval of 100 msec.

In both experiments subjects were required to make a forced-choice judgement on the relative size of the TF and CF circles, saying 'left' or 'right' larger. In Expt. I subjects were dark-adapted for 5 min. Four values of variable CF were selected around the approximate point of subjective equality (PSE) established by a method of limits, and these four TF + CF patterns were presented 40 times each in four control blocks and four experimental blocks of 20 trials each in the order C E C E E C E C. The mean PSEs for each condition were established by fitting straight lines to S–Z plots. In Expt. II the control, FAE and RFAE measurements were made in three separate blocks, in random order for each subject. The up-and-down method (Dixon & Mood, 1948) was used for repeated measures from individual subjects, since large numbers of trials were impractical. Fifty judgements were made, counted from the first change in the subject's judgements. Mean PSEs (and S.D.s) were calculated for each subject in each condition.

FAE size effects were obtained by subtracting the experimental means from the control means. These FAE measures were also expressed as a percentage of the control means, and all analyses were repeated with these percentage scores.

RESULTS

In both experiments mean PSEs differed significantly between experimental and control conditions ($P < 0{\cdot}0001$ in Expt. I, $P < 0{\cdot}01$ in Expt. II). In Expt. I the mean FAE shrinkage was 1·86 mm (6·52′), S.D. = 0·86 mm. In Expt. II the mean FAE was 2·06 mm (6·21′), S.D. = 1·10 mm, and the mean RFAE was 1·19 mm (3·57′), S.D. = 1·63 mm.

A factorial design analysis of variance ($3 \times 3 \times 2$) of the data from Expt. I showed no significant main effects of E, N or sex, nor any significant interactions. For Expt. II, a Type 3 mixed design analysis of variance (Lindquist, 1953) showed no significant effects of E, N or sex, nor of their interactions. The within-subjects effects of experimental conditions (FAE, RFAE) were significant at the 0·005 level, with the shrinkage effects significantly greater in the FAE condition. A similar analysis was conducted with the S.D. scores expressed as percentages of PSEs. Women were significantly more variable than men at the 0·05 level, and the experimental conditions were significantly more variable than the control condition at the 0·01 level, but there were no effects of E or N.

The analyses of the shrinkage scores were repeated with these scores expressed as percentages of control PSEs, with similar outcomes.

DISCUSSION

An adequate test of the hypothesis under consideration requires that the criterion groups are well differentiated and that the FAE measures are significant and reliable. In these experiments the extreme groups are clearly differentiated at ± 1 S.D. from

the means. The FAEs measured are significant, and at 4·87 per cent FAE and 3·95 per cent RFAE typical of those reported in the literature. For example, Ikeda & Obonai (1955) give 5 per cent FAE and 3 per cent RFAE shrinkage for similar size ratios. No direct evidence on the reliability of the FAE measures can be obtained from the data in these experiments. However, in a pilot study, a test–retest correlation of 0·82 was obtained with a 1 week interval, while in Expt. II there is a significant ($P < 0{\cdot}05$) correlation between FAE and RFAE.

Under these conditions we are justified in rejecting the hypothesis that extraverts will show smaller FAE than introverts with short inspection times. This may say more about the postulated identity of reactive cortical inhibition and the inhibitory processes underlying the FAE than about the relationship between reactive cortical inhibition and extraversion–introversion.

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(*Manuscript received* 18 *January* 1972)

Br. J. Psychol. (1972), 63, 4, *pp.* 573–574
Printed in Great Britain

BRITISH NORMS FOR THE EIGHT-PARALLEL-FORM ANXIETY BATTERY

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The equivalences of pairs of forms of the Eight-Parallel-Form Anxiety Battery have been shown to range widely, although the mean equivalence coefficients have been reasonably high (Scheier & Cattell, 1960; Bendig & Bruder, 1962; Philip, 1970). These studies used students as subjects and it was felt that an examination of form-equivalence in a more diverse population would be worthwhile. In the study reported by Philip (1970) it was observed that the normative data provided in the Manual for translating raw scores into stens (standard 10-point scales) were inappropriate. Accordingly, a further aim of this study was to provide normative data more appropriate for British use.

One hundred and seventy-eight individuals (69 M, 109 F) cooperated in this study. All eight forms of the test were completed by 22 subjects, including the students from the previous study. The remainder each completed four forms of the Battery. This gave 100 protocols for each form of the Battery, the average number of pairs for calculating correlations being 55. Those tested included physiotherapy students, technicians, scientists, housewives and others. One quarter of these subjects were aged below 20 years, a further quarter were aged 35 years or more, leaving half the group in the 20–34 year range.

Two sets of correlations are shown in Table 1. The values in the upper triangle of the matrix refer to all subjects, while those in the lower triangle are those for the non-student part of the sample. The mean raw scores for each form were compared with the values in the Manual (Table 2). Four forms show significant differences while a further two forms almost attain the 5 per cent significance level. For all forms, except 'A', the mean score of the British sample was higher than that reported in the Manual. The raw scores of each form were changed into sten scores using the area transformation method (Table 3).

The present findings agree with previous studies in respect of the magnitude and range of equivalence correlations between forms. For retest purposes some pairs of tests are to be preferred to others. The normative data in the Manual are not applicable to British subjects. Although not a representative sample the present respondents are more 'normal' than those who provided the norms for the Manual; much statistical juggling was necessary there to compensate for the high proportion of 'abnormal' subjects. Table 3 provides a working set of tables for the British user of the Battery.

Table 1. *Correlations between forms of the battery*

(Decimal points omitted.)

	A	B	C	D	E	F	G	H
A	—	597	619	689	418	658	541	571
B	637	—	645	680	715	731	644	516
C	620	641	—	708	476	709	535	656
D	680	744	675	—	538	676	628	729
E	457	823	208	608	—	611	588	506
F	865	798	773	767	623	—	691	655
G	560	647	576	669	661	773	—	647
H	620	519	345	729	454	699	674	—

Upper triangle – all subjects, $\bar{r} = 0{\cdot}628$ (range 0·418–0·731).
Lower triangle – subjects excluding physiotherapists, $\bar{r} = 0{\cdot}667$ (range 0·208–0·865).

Table 2. *Comparison of mean raw scores*

Test form	Manual data			Present data				
	n	Mean	S.D.	*n*	Mean	S.D.	*t*	
A	169	6·7	1·1	100	6·3	1·2	1·03	n.s.
B	161	6·4	1·3	100	6·7	1·3	1·86	$0{\cdot}10 < P < 0{\cdot}05$
C	175	6·4	1·2	100	6·8	1·3	2·60	$P < 0{\cdot}01$
D	235	6·0	1·2	100	6·2	1·1	1·49	n.s.
E	221	6·6	1·1	100	7·2	1·2	4·38	$P < 0{\cdot}001$
F	166	6·2	1·2	100	6·6	1·4	2·44	$P < 0{\cdot}05$
G	167	6·0	1·3	100	6·3	1·2	1·95	$0{\cdot}10 < P < 0{\cdot}05$
H	142	6·7	1·1	100	7·0	1·2	2·03	$P < 0{\cdot}05$

Table 3. *British norms for the eight forms*

($n = 100$ for each form.)

Sten	A Raw	B Raw	C Raw	D Raw	E Raw	F Raw	G Raw	H Raw
1	0–3·8	0–4·5	0–4·7	0–4·0	0–4·9	0–4·3	0–4·1	0–4·7
2	3·9–4·7	4·6–4·8	4·8–5·2	4·1–4·5	5·0–5·3	4·4–4·6	4·2–4·5	4·8–5·2
3	4·8–5·1	4·9–5·5	5·3–5·6	4·6–5·0	5·4–5·8	4·7–5·1	4·6–5·0	5·3–5·8
4	5·2–5·6	5·6–5·9	5·7–6·2	5·1–5·6	5·9–6·6	5·2–5·9	5·1–5·5	5·9–6·3
5	5·7–6·2	6·0–6·5	6·3–6·7	5·7–6·2	6·7–7·0	6·0–6·4	5·6–6·2	6·4–6·9
6	6·3–6·9	6·6–7·1	6·8–7·3	6·3–6·6	7·1–7·8	6·5–7·1	6·3–7·0	7·0–7·3
7	7·0–7·3	7·2–7·8	7·4–8·0	6·7–7·2	7·9–8·5	7·2–7·8	7·1–7·4	7·4–8·2
8	7·4–7·8	7·9–8·8	8·1–8·3	7·3–7·9	8·6–9·0	7·9–8·8	7·5–8·0	8·3–8·9
9	7·9–8·8	8·9–9·4	8·4–8·9	8·0–8·8	9·1–9·6	8·9–9·8	8·1–8·8	9·0–9·4
10	8·9+	9·5+	9·0+	8·9+	9·7+	9·9+	8·9+	9·5+

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(*Manuscript received* 12 *November* 1971)

Br. J. Psychol. (1972), 63, 4, *pp.* 575–582
Printed in Great Britain

BASELINE DEPENDENT FACILITATION: EFFECTS OF A PRE-REWARD STIMULUS

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Two groups of six pigeons were trained to key-peck on either a VI or a DRL schedule of positive reinforcement. After response rates were stable subjects were presented with one of the following combinations of a signal and free reward over several sessions. For one subgroup of three birds in each group, an auditory stimulus was paired with free reward whilst birds were prevented from making the operant response. Birds in the remaining subgroups received random presentations of the stimulus and free reward. When the conditioned stimulus alone was presented at random intervals during normal key-peck sessions for three days, no consistent effects on response rate were observed. Further respondent conditioning sessions followed, after which the effects of the conditioned stimulus were tested against ongoing behaviour, or during extinction of the operant response. Birds retested on the baseline again showed little effect. Of birds tested during extinction, those receiving pairings of the conditioned stimulus and reward showed facilitation of responding during the stimulus, whilst those who had received random presentations showed a slight suppression.

The operant approach to the study of emotional behaviour began with Estes & Skinner's (1941) study of what has since come to be known as conditioned suppression. Briefly, their procedure involved the presentation of a signal (CS) terminating in electric shock (US), independently of an ongoing baseline of behaviour maintained by positive reinforcement. More recently the paradigm has been extended to the study of CSs leading to either non-contingent negative *or* positive reinforcers.

Studies which have investigated the effect of a CS followed by a free positive reinforcement (US) upon ongoing positively reinforced behaviour have been reviewed by Strongman *et al.* (1971). Of particular relevance here, Herrnstein & Morse (1957) found that a CS of 2 min. duration, with free reward presented 1 min after its onset, facilitated key-pecking in pigeons working on a schedule which differentially reinforced a low rate of responding (DRL 5 min). Henton & Brady (1970) found that an 80 sec. CS terminated by free reward produced acceleration in monkeys on DRL 30 sec. schedules, whereas CS presentations of 20 or 40 sec. produced no effect. Azrin & Hake (1969) found CS suppression on a 1 min. variable interval schedule (VI, 1 min.) using rats, with a CS of 10 sec. duration. Meltzer & Brahlek (1970), also using rats, found a similar suppression of VI 2 min. performance with a 12 sec. CS, no effect with a CS of 40 sec., and response facilitation during a CS of 120 sec. duration. Similarly, with monkeys working on VI 45 sec., Miczek & Grossman (1971) found suppression during a short CS, but no facilitation appeared as CS duration was increased.

Interpretation of these data is hindered by at least three factors. First, the data are drawn from several species, sometimes working on quite different schedules of reinforcement. Secondly, only Azrin & Hake and Henton & Brady specifically controlled possible effects of adventitious reinforcement, so that facilitative effects found in other studies could have been caused, or at least maintained, by superstitious responding. Some procedures minimizing adventitious reinforcement make the US contin-

gent on a pause at the end of the CS. Finally, in most studies, subjects have been habituated to a neutral stimulus, and later presented with that stimulus paired with free reward. Rescorla (1967) has argued convincingly that an adequate control for any respondent conditioning procedure requires subjects in a control condition to have experience of both CS and US randomly and independently. This control may be particularly important, since it is desirable to avoid any contaminating effects due simply to US presentation (e.g. progressive satiation leading to a drop in overall response rate).

The present study, using a design first employed by Estes (1943), avoided some of the above difficulties. After acquisition of an operant response, pigeons received respondent conditioning trials 'off the baseline' such that, during this phase of the procedure, they were unable to key-peck. Thus any behaviour adventitiously reinforced by the treatment would serve to reduce the probability of the reinforced operant. Further, during this phase some birds received normal CS–US pairings, whilst others received presentations of CS and US that were both random and independent. The effect of random or contingent pairings was tested without the US present, further avoiding satiation effects. Also, birds were established on either VI or DRL schedules, such that a direct comparison could be made of the effect of a fixed number of CS–US presentations on either schedule after the same amount of training. These schedules were chosen since they had already been shown to yield differential results in similar studies (e.g. Azrin & Hake, 1969; Herrnstein & Morse, 1957). Finally, a test of the effects of CS was made not only against ongoing behaviour, but also during extinction.

METHOD

Subjects

Twelve experimentally naive male pigeons (B 1–B 12), aged approximately 18 months, were maintained at 80 per cent of their free-feeding weights throughout the experiment. Continuous access to water was provided in home cages.

Apparatus

Two Lehigh Valley Electronics Model 1519 Pigeon Test Chambers were used, white noise being fed individually to each enclosure via loudspeakers. Only the right-hand key on each panel was operable, the microswitch contacts being closed by a static mass of 20 g. Electromagnetic control equipment programmed VI schedules to one chamber and DRL schedules to the other. Reinforcement consisted of 3 sec. access to the food hopper together with 3 sec. onset of the hopper light.

During respondent conditioning sessions, two matt aluminium strips (35 × 6 cm) obscured both keys in each box. During this phase, the US (free reward) consisted of the operation of the hopper and the hopper light, *and* turning off of the houselight, for 4 sec. Free reward was thus different from reinforcement in both duration and associated stimuli. The CS was a 1 min. clicker operating at 4 beats per sec.

Procedure

Operant conditioning: acquisition and stabilization. During the first experimental day, each bird was magazine trained and hand-shaped to key-peck. For the two days, birds worked for continuous reinforcement, a session being terminated when 40 reinforcements had been presented. Following this, birds were arbitrarily assigned to one of two groups, six subsequently being established on VI schedules, the remainder on DRL schedules. On days 4 and 5, birds worked on VI 30 sec. or DRL 10 sec., the parameter values of these schedules being increased to 1 min. and

20 sec. respectively on days 6 and 7. On day 8, schedule requirements were again raised to the terminal baseline values of VI 1·5 min. (Fleshler & Hoffman, 1962) and DRL 30 sec. Birds worked on these schedules in consecutive daily 55 min. sessions for 66 days (days 8–73) until each bird had reached a stable daily rate of responding. Response rate and reinforcements were monitored for each session using counters and cumulative recorders. Similar data were taken during all operant conditioning sessions.

Off baseline respondent conditioning. Following stabilization, both the VI and DRL groups were divided into two subgroups of three birds per group, such that birds were matched across subgroups for overall rate of responding. The subgroups were DRL-C (B 3, B 7, B 9, DRL-R (B 1, B 5, B 11), VI-C (B 8, B 10, B 12) and VI-R (B 2, B 4, B 6)). Groups suffixed 'C' received normal conditioning trials, i.e. 12 pairings of CS and US per session, the pairing occurring randomly in time, but with a mean interstimulus interval of approximately 4·6 min. The remaining six birds, in the 'R' subgroups, received 12 presentations of both CS and US randomly in time during sessions of the same length (i.e. both CS and US had mean interstimulus intervals of 4·6 min. but in this condition both stimuli were programmed independently of one another). During this phase, all birds received two daily 55 min. sessions spaced 6 hr. apart for seven days (days 74–80). Thus, all birds received 168 presentations of both CS and US either contingently, as in normal respondent conditioning, or independently, as in a Rescorla 'truly-random' control procedure. During this phase, aluminium blanks fitted over the response keys prevented pecking.

Test 1

All birds were then returned to the reinforcement schedules on which they had been previously trained. On the first day (day 81), session duration was 70 min., the initial 15 min. being free of the CS to ensure that performance on the schedule had been reinstated. During the final 55 min., each bird received six presentations of the CS alone spaced randomly in time. Response rate during each 1 min. CS, and during the 1 min. period immediately preceding it (PCS responses) was recorded on print-out counters. Birds received six CS presentations during 55 min. sessions for the following two days (days 82–83).

Restabilization and reconditioning. All birds were fully restabilized on their baseline schedules for 28 days (days 84–111). Respondent conditioning sessions, the same as those already described, were then reinstated for a further 7 days (days 112–118).

Test 2

One bird from each of the four subgroups (B 3, B 5, B 6, B 8) was selected to remain on the appropriate baseline schedule, whilst the other eight birds were switched to extinction. Sessions were again of 55 min. duration. Birds thus ran under one of four conditions (VI 1·5 min.; EXT VI 1·5 min.; DRL 30 sec.; EXT DRL 30 sec.) for three days (days 119–121) after which six CS presentations were spread randomly throughout every session for five consecutive days (days 122–126). Hence, for four birds, the procedure replicated that of test 1, whilst for the remainder, the procedure constituted a test of CS effects in extinction.

Results

The effects of the CS on response rate were determined using the inflection ratio (RCS/RPCS + RCS): (Annau & Kamin, 1961). RCS indicates response rate during the 1 min. CS, and RPCS the rate during the immediately preceding minute. An inflection ratio of 0·50 indicates equal rates in the PCS and CS, and the ratio can vary between limits of 0·00 and 1·00. Facilitation of responding during the CS is indicated by a ratio greater than 0·50, a lowered CS rate by a ratio less than 0·50. When no responses were made in PCS or CS, this ratio equals 0/(0 + 0) which is mathematically undefined. However, this value only occurred when the bird's rate of responding did not change due to CS presentation. Thus, in view of the more general definition, this ratio was arbitrarily defined as 0·50. Inflection ratios were calculated for each CS presentation during the eight test days, the mean daily inflection ratio being determined for each

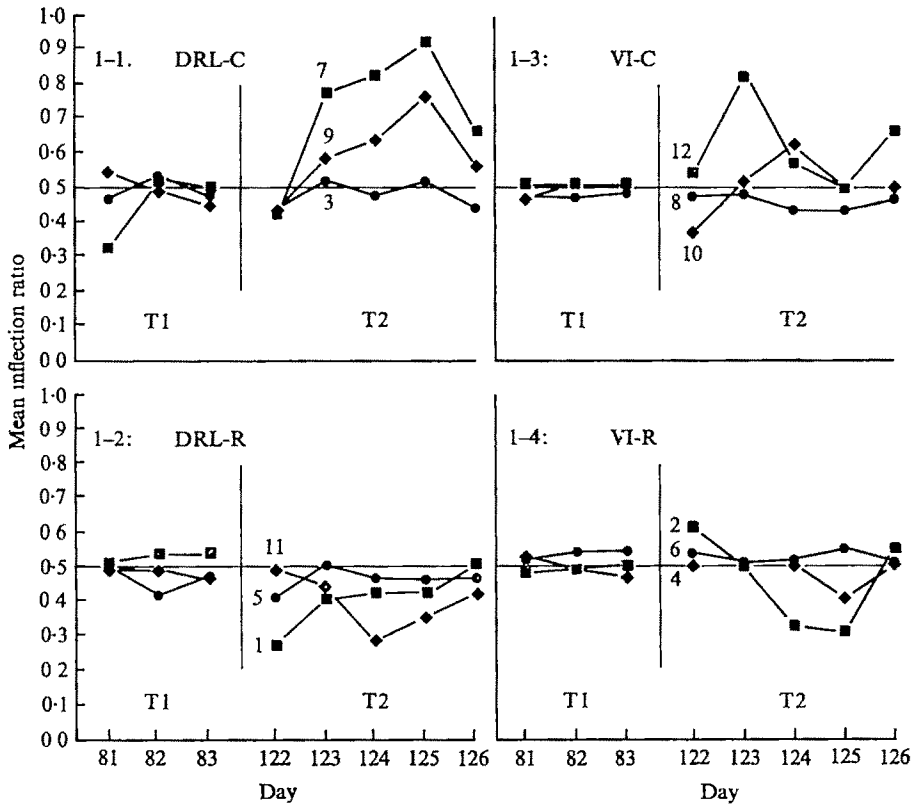

Fig. 1. Mean inflection ratios for individual birds during test 1, where birds were returned to their ongoing reinforced baseline (VI 1·5 min. or DRL 30 sec.) and test 2, where only B 3, B 5, B 6 and B 8 (circles) were returned to baseline, other birds being placed on extinction. Graphs suffixed 'C' (1–1, 1–3) contain subgroups of birds who had undergone normal respondent conditioning sessions, 'R' suffixed graphs (1–2, 1–4) contain subgroups who had been exposed to the truly random control procedure.

bird over six presentations. These mean scores are shown in Fig. 1. For birds subjected to extinction, mean response rates during the CS and the mean rate at all other times during the session were also calculated over days 122–126. These data appear in Table 1.

Test 1. Mean daily inflection ratios for each bird over three days are shown in the left-hand side of each graph in Fig. 1. Birds in both DRL subgroups showed little change in response rate during the CS, the most striking difference being the somewhat greater variability of inflection ratios for the DRL-C birds on the first day of testing (Figs. 1–1, 1–2). B 7 initially showed a marked but transient suppression on day 122, while B 3 and B 9 showed no consistent effects. Of the DRL-R birds, B 1 showed a slight consistent facilitation, while B 5 and B 11 showed slight suppression of responding. Effects on the VI subgroups were again small (Figs. 1–3, 1–4). Only B 8 (VI-C) showed slight but consistent suppression, and B 6 (VI-R) a similar facilitation during CS, effects for other VI birds being varied or imperceptible.

Overall, effects of the CS on any individual bird was meagre, and there were no obvious similarities within, or differences between, subgroups. Further, comparisons

Table 1. *Response rates during test* 2 (*days* 122–126) *for birds undergoing extinction*

(CS rate is the mean rate of responding during six CS presentations; extinction rate the mean rate during all other parts of the session.)

			Day				
Subgroup	Bird	Rate in	122	123	124	125	126
DRL-C	B7	CS	1·7	1·0	0·7	1·2	0·5
		Extinction	1·0	0·6	0·6	0·0	0·3
	B9	CS	4·7	6·8	5·2	2·2	4·2
		Extinction	7·7	5·7	2·4	1·4	2·4
DRL-R	B1	CS	2·0	1·2	0·0	0·0	0·0
		Extinction	4·2	1·2	0·7	0·2	0·1
	B11	CS	10·5	7·5	0·7	0·8	0·7
		Extinction	9·5	6·8	2·2	1·2	0·6
VI-C	B10	CS	3·5	9·0	1·5	0·0	0·0
		Extinction	2·1	4·1	0·5	0·0	0·0
	B12	CS	62·2	58·7	5·3	0·0	0·5
		Extinction	52·2	35·6	1·5	0·1	0·0
VI-R	B2	CS	7·7	4·2	0·0	0·2	1·2
		Extinction	4·6	2·7	0·9	0·4	0·1
	B4	CS	0·0	0·2	0·8	5·7	0·0
		Extinction	0·1	0·4	0·1	1·1	4·0

between birds returned to different schedules, having undergone the same respondent conditioning procedure, yielded a similar lack of effect.

Test 2. In each subgroup, the bird returned to the pre-respondent conditioning baseline (B 3, B 5, B 6, B 8) again showed only small effects similar to those found in Test 1. As before, B 6 showed a slight but consistent facilitation and B 8 a comparable suppression. The slight suppression which had developed on days 82–83 for B 5 continued, whilst B 3 continued to produce inflection ratios which varied only slightly about 0·50.

However, marked effects were found on some birds subjected to the extinction procedure. In the DRL-C subgroup, both B 7 and B 9 showed a slight suppression on the first test day (day 122), but a clear facilitation effect during the following days of extinction. Sample cumulative records from these birds are shown in Fig. 2. It can be seen that, for B 9, CS onset produced an increase in the existing rate of responding. For B 7, later in extinction, where the existing local rate was zero, CS onset was sufficient to reinstate the response.

Reference to Table 1 shows that CS rate exceeded not only the local (PCS) rate, but also the overall rate of responding throughout the session for B 7 (on each test day) and for B 9 (on each day following day 122). Extinguished birds in the DRL-R subgroup (B 1, B 11) showed considerable suppression of responding (Fig. 1–2), an effect reflected in overall response rates (Table 1). This effect may be due partly to inadequacies in the CS–US randomization procedure (e.g. CS occurrence perhaps tended to predict absence of free reward) and partly to the fact that, in extinction, the second of two successive time samples of responses will tend to be lower than the first. This mitigates against the possibility of finding a facilitation effect in extinction.

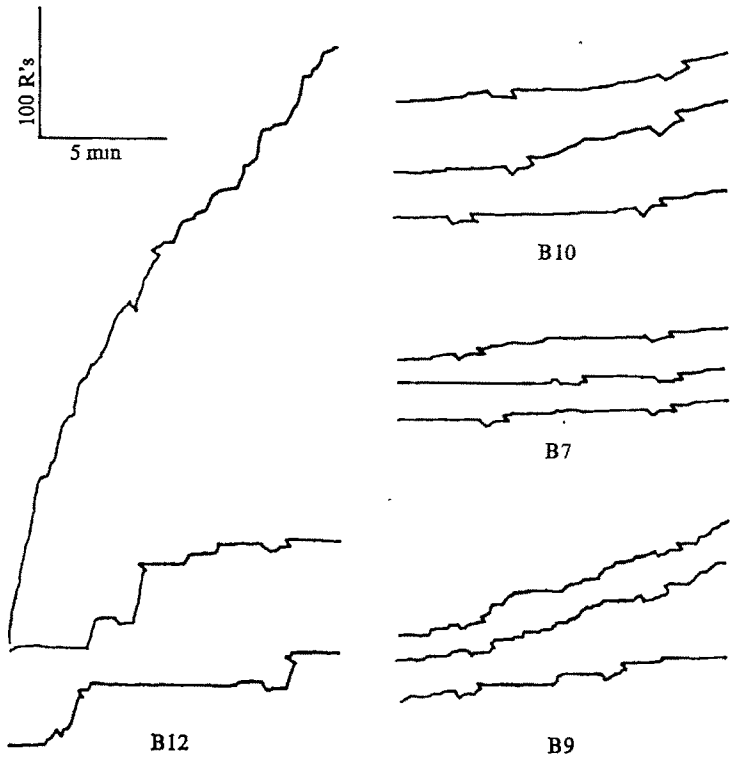

Fig. 2. Representative cumulative records of key-pecking responses for B10, B12, B7 and B9 during the first three days of test 2. In each case the uppermost record comes from day 122, the second from day 123, and the lower record from day 124. All birds had experienced normal respondent conditioning sessions, B10 and B12 then being returned to extinction of VI responding, while B7 and B9 were returned to extinction of DRL responding. The records demonstrate CS facilitation of responding in extinction, downward displacements of the pen indicating periods during which the CS was in operation.

Birds extinguished in the VI-C subgroup showed less clear effects than comparable birds on DRL schedules. B10 showed quite large variation of mean inflection ratios over days 122–124, after which complete extinction occurred (Table 1). However, Table 1 also indicates that during the initial three-day period B10 produced an overall higher rate of responding during the CS than during the rest of the session, even on day 122. The discrepancy between these data is due to the two occasions on which CS presentation stopped local responding. B12 showed a clearer facilitation effect on both inflection ratios and overall response rates (Fig. 1–3, Table 1). Compared with VI-C birds, those in the VI-R subgroup showed clear differences in patterns of responding during extinction (Fig. 1–4). B2 produced a marked facilitation on day 122, followed by three days of marked suppression. Overall response rate data show the same tendency to variability. On inflection ratios, B4 showed no effect over the days 122, 123, 124 and 126 and a slight suppression on day 125, a pattern paralleled on overall response rates but with somewhat greater variability. In summary, birds exposed to signalled free reward 'off the baseline' showed a facilitation of responding during the CS, *when presented in extinction*, the effects being similar across birds extinguished after working on VI or DRL reinforcement schedules. This effect was

consistent across measures comparing local and overall response rates. Birds exposed to random CS and US presentations tended to show suppression to the CS in extinction, but these birds showed greater variability and less consistency across the two measures. No clear effects were seen in any condition in which the CS was presented during ongoing reinforced behaviour.

Discussion

Several recent studies (e.g. Azrin & Hake; Meltzer & Brahlek) have found a suppression of responding during a short CS in the standard positive CER paradigm, the effect being known as positive conditioned suppression. However, a facilitative effect of a stimulus paired with free reward has been found in this and several earlier studies. Estes (1943, 1948) found increased response rates during a CS previously paired with free reward when the stimulus was presented in extinction of fixed-interval responding. A control group which had previously received presentations of the CS alone showed no effect. The present study adds generality to this finding, since the effect occurred during extinction following both DRL and, to a lesser extent, following VI schedules, in comparison with birds receiving a truly random control procedure.

Morse & Skinner (1958) exposed naive pigeons to a houselight alternating in colour between red and green. In the presence of one light, *free* reward was presented on a VI 1 min. schedule. The birds were then trained to peck a key illuminated by white light, but during extinction of this response, the key light alternated between red and green. An increased rate of responding in extinction was observed only during those periods in which the key was illuminated by light of the colour that had been previously paired with the free reward schedule. Hyde *et al.* (1968), using rats, paired a stimulus with food over several daily sessions. Then pursuing a discrete trials procedure, the rats were trained to bar-press for food at the onset of a discriminative stimulus. Finally, the CS was used as a generalization stimulus for the bar-press response. Rats that had received CS–US pairing showed shorter response latencies to the CS than did control animals that had received 'truly random' presentations of the same CS and US.

These findings support the general position that a stimulus paired with free reward leads to a facilitation of instrumental responding, at least in situations where respondent and instrumental conditioning sessions are temporally independent. Further, facilitation has been found in some studies where the two procedures are simultaneously in operation (e.g. Herrnstein & Morse, Meltzer & Brahlek). None of these studies, which have analysed several different kinds of data, support a 'general emotional state' explanation of CS effects (Azrin & Hake, 1969), and suppression effects found during the CS seem to be somewhat exceptional. These may be explicable in terms of the discriminative stimulus properties of the CS. Clearly, CS termination sets the occasion for reinforcement of behaviours which typically occur just before food presentation (e.g. orienting to the hopper). With a short CS, this, or some other perhaps superstitious behaviour, would come under strong stimulus control, and might compete with the reinforced operant response. This would produce an overall suppression effect. As CS duration is increased, the probability of adventitious reinforcement for the competing response is reduced and the more general facilitation effect emerges.

However, the nature of this more general effect is far from clear. In the present study, a CS which failed to produce any effect against ongoing reinforced behaviour generated a large effect in extinction. This suggests that, where the CS is presented alone after a series of respondent conditioning sessions, the overall frequency of baseline reinforcement may be an important variable, facilitation occurring when this is low or zero. But if this is the case, it is surprising that no differences emerged between birds working on DRL where frequency of reinforcement was low, and those established on VI, where the reinforcement density was relatively high. These differences were found where Pavlovian conditioning took place on the baseline.

Within the framework of the present analysis, the effects of a CS linked with free reward measured against behaviour generated by positive reinforcement depend on (1) whether the CS–US pairing takes place before or during instrumental behaviour; (2) whether the CS is presented against an ongoing baseline or during extinction; (3) the operant reinforcement schedule and (4) the duration of the CS.

This research was supported by Medical Research Council grant no. G 969/269/B to the second author.

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(*Manuscript received* 3 *February* 1972)

Br. J. Psychol. (1972), 63, 4, *pp.* 583–591
Printed in Great Britain

SAFETY SIGNALS AND THE ACQUISITION AND EXTINCTION OF LEVER-PRESS DISCRIMINATED AVOIDANCE IN RATS

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Rats learned a discriminated avoidance response more rapidly when a safety signal occurred following an escape or avoidance response. Two extinction procedures were used: complete omission of shock or delivery of an unavoidable shock at the end of the warning period. Under the latter procedure, avoidance–extinction responding was initially higher but subsequently declined to the same level as under the omission procedure. Under each extinction procedure, animals were divided into two subgroups: safety signal and no-safety signal. The safety signal did not significantly effect avoidance responses in extinction *per se*, but it did result in fewer extra responses during the inter-trial and safety periods following an avoidance–extinction response. With no-safety signal there was an increase over extinction sessions in the numbers of such extra responses, especially in the shock delivery subgroup.

Beginning with Mowrer (1939), many writers have suggested that discriminated avoidance learning involves two separate processes: stimulus association and response strengthening. It is claimed that the discriminative pre-shock stimulus acquires aversive properties because it is temporally associated with shock, giving this stimulus the functional status of a drive. A response terminating such a stimulus would be strengthened according to a drive reduction principle of response reinforcement. Alternative accounts of the learning of an avoidance response exist. Sidman (1962) and Herrnstein & Hineline (1966) proposed that responses which avoid shock become strengthened because they result in reduction of shock frequency. Another approach has emphasized that responses which result in safety signals, that is stimuli which have been consistently associated with periods free from shock, become differentially strengthened (Denny & Weisman, 1964; Dillow, 1968; Rescorla, 1969; Weisman & Litner, 1969; Bolles & Grossen, 1969). It has been suggested (Denny, 1971) that the effectiveness of safety signals is due to conditioned relaxation.

The latter interpretation focuses on the positive incentive and reinforcing properties of safety signals, rather than on the negative reinforcing properties of the warning signal. These two interpretations are not contradictory but are in fact logically complementary since both are concerned with response contingent stimulus changes. One emphasizes the stimuli present prior to performance of the response, the other the stimuli following the response. A complete theoretical analysis of avoidance must consider the characteristics of both warning and safety signals, even though much data may be adequately understood in terms of either one or the other of these interpretations.

The importance of the warning signal is demonstrated by Miller's (1948) experiment in which termination of a conditioned aversive stimulus was used to reinforce a new

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response. Moreover, a delay in termination of the warning signal produces a decrement in acquisition (Bolles *et al.*, 1966; Kamin, 1956) and more rapid extinction (Delprato, 1969). A theoretical viewpoint emphasizing warning signal termination, however, has difficulty explaining the fact that rats will not respond to avoid a conditioned aversive stimulus (Sidman & Boren, 1957), although they will terminate such a stimulus. This poses problems for a drive reduction theory of avoidance, because if a stimulus will not be avoided but only escaped the question is raised as to whether that stimulus is actually aversive. It might be more accurate in this case to think of the warning signal not as a fear-inducing or conditioned aversive stimulus, but simply as a discriminative stimulus which informs the organism that unless he responds shock is imminent.

Further experiments demonstrate that stimuli other than those preceding the avoidance response can influence its acquisition. Bolles & Grossen (1969) and D'Amato *et al.* (1968) showed that an avoidance response feedback stimulus could largely overcome the impairment in acquisition due to lack of a warning signal termination contingency. An emphasis on the stimuli following the response provides a parsimonious explanation of why the availability of a feedback stimulus in a free operant avoidance situation facilitates the acquisition of a high response rate (Bolles & Grossen, 1969; Hurwitz *et al.*, 1972) and retards extinction (Roberts *et al.*, 1970). In this case, feedback stimuli are necessarily presented when shock is temporally furthest away and thus signal the beginning of a safe period.

It should be noted that even though both the warning and safety signals are important variables in avoidance acquisition and maintenance, they are not always necessary (Sidman, 1962). Avoidance of the unconditioned aversive stimulus itself is the most basic of the variables contributing to acquisition (Bolles *et al.*, 1966).

In the present experiment the effect of a safety signal on the acquisition of a discriminated avoidance response was examined. Because there are two possible procedures for the extinction of avoidance behaviour, for completeness the effect of the safety signal was examined on each. In the first, shock was given at the conclusion of each warning signal period even when a response to the signal was made. In the second, shock was omitted altogether.

METHOD

Subjects

Twenty-four female hooded rats supplied by Spruce Farms, New Jersey, served as subjects. They were approximately 100 days old at the beginning of experimentation. The subjects were randomly assigned to the four experimental conditions described in the Procedure section.

Apparatus

A standard lever-press shock chamber as described in Dillow & Hurwitz (1971) was employed. A 1 mA constant current shock was mechanically scrambled across the grids, walls, and lever. The chamber was equipped with a buzzer mounted on the ceiling which when operating produced a signal 10 db above the ambient noise level.

Procedure

Two different discrete-trial avoidance training procedures, referred to as no safety and safety and described in the flow diagrams of Figs. 1 and 2, respectively, were employed. Under both procedures, a lever response during the warning period avoided shock and terminated the

warning buzz. For the 12 subjects in the no-safety condition, an avoidance response directly introduced the inter-trial period which was of variable duration, T (mean 45 sec., range 30–60 sec.). For the 12 subjects trained under the safety procedure, the avoidance response also introduced a 16 sec. safety signal which was indicated by the houselights being extinguished. This, in turn, was followed by a T-16 sec. inter-trial interval.

If, under either procedure, after 8 sec. of warning signal, no lever response had occurred, the buzzer remained on while a series of pulsed shocks was initiated. Shock pulses were delivered once every second and lasted for $\frac{1}{3}$ sec. Under the no-safety procedure (Fig. 1), an escape response, i.e. a lever-press in the presence of shock, terminated both the shock series and the warning buzz and introduced an inter-trial period of T sec. duration. Under the safety procedure (Fig. 2), buzz and shock termination was followed by a brief safety signal, houselight off for 1·5 sec. This in turn was followed by an inter-trial period which lasted T-1·5 sec. Lever responses during safety and/or inter-trial periods had no programmed consequences. Automatic programming and recording were accomplished by relay circuitry housed in a separate room.

Each subject was given 100 trials daily. Training was continued until a criterion of avoidance was reached which required that the subject avoid shock on 70 per cent of the trials on four consecutive sessions. Upon reaching the avoidance performance criterion, each subject was given four daily extinction sessions of 100 trials each.

At this point each group was subdivided. For two subgroups the shock was disconnected (no-shock procedure). For the remaining two subgroups two inescapable shock pulses were delivered 8 sec. after the onset of the warning signal irrespective of whether the subject had made a lever-press during this interval (shock procedure). Except for the above-mentioned changes in the shock contingencies all stimulus programming, for all subgroups, was the same as under avoidance acquisition. That is, subjects trained with safety signals received these signals in extinction, and subjects trained without safety signals did not receive them in extinction. Note that in the safety, shock group the shock was necessarily received some time during the 16 sec. safety signal on avoidance–extinction response trials, and on no-response trials the 1·5 sec. safety signal followed the shock pulses.

Results

Acquisition: avoidance response data

The rates of acquisition of the avoidance response by subjects trained under the safety and no-safety procedures were compared. The no-safety subjects took a mean of 11 sessions to reach the criterion of 70 per cent avoidance on four consecutive sessions whereas the safety subjects took a mean of five sessions to achieve this criterion. This difference in the number of sessions to criterion performance was found to be statistically significant ($t = 3{\cdot}15$; d.f. $= 13$; $P < 0{\cdot}005$) when tested with a t test for independent groups and unequal variances with Welch's correction applied to the degrees of freedom (Winer, 1962).

Acquisition: extra response data

A measure of discrimination was obtained by counting the sessional number of responses performed in the safety and inter-trial periods following the successful performance of an avoidance response. This figure when divided by the sessional number of avoidances gives the mean number of responses performed in the safety and inter-trial periods following an avoidance; for brevity this will be referred to as the mean number of extra avoidance responses. Perfectly discriminated avoidance behaviour would be characterized by an absence of extra avoidance responses. That the avoidance behaviour in the present experiment was under good discriminative control of the warning stimulus is attested to by the fact that the mean number of extra avoidance responses across all subjects on the last of the acquisition sessions was

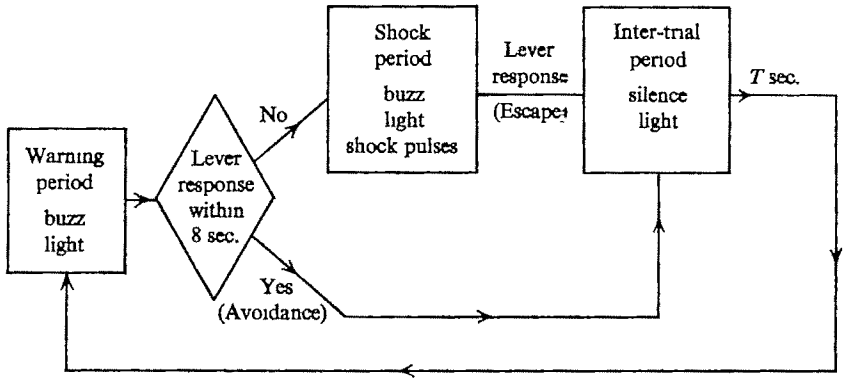

Fig. 1. Flow diagram of no-safety avoidance training procedure. The name of each state in the procedure, together with the stimuli which describe that state, is presented within each square. The arrowed lines represent transitions between the various states, and the adjacent descriptions refer either to the duration of the preceding state or to the termination of that state by a response. The diamond represents an either–or decision point in the procedure. Only one state could exist at a given time.

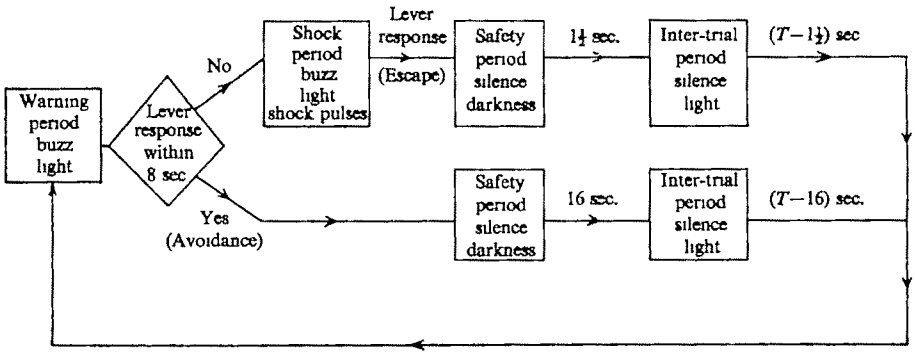

Fig. 2. Flow diagram of the safety avoidance training procedure.

0·3 responses, or approximately one extra avoidance response following every third successful avoidance.

An analysis of variance was performed on the sessional means of the number of extra avoidance responses in acquisition using a design with two between-subject variables, each with two levels (safety *v.* no-safety and shock *v.* no-shock), and with one within-subjects variable with four levels, the last four acquisition sessions. Note that the safety *v.* no-safety variable was manipulated during acquisition in that half the subjects were trained under each condition, whereas the shock *v.* no-shock variable was not introduced until the extinction phase of the experiment. The latter variable was included in the analysis to check that no differences existed between the subjects prior to their exposure to the differing extinction procedures. The analysis of variance computations followed the procedure given by Lindquist's (1953) type III.

The between-subject main effects and interactions were not statistically significant. The only significant within-subjects factor was the main effect of sessions ($F = 6{\cdot}61$; d.f. = 3, 60; $P < 0{\cdot}01$). Duncan's new multiple range test (Edwards, 1967) was applied to this latter significance. It showed that there were significant decreases

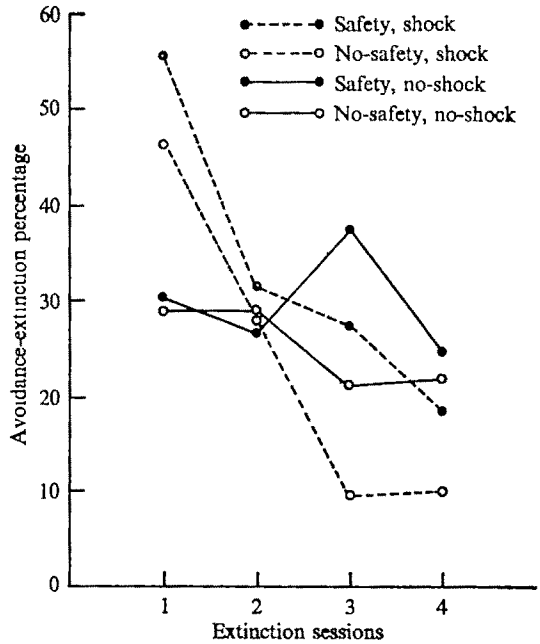

Fig. 3. The mean percentage of trials within a session on which an avoidance–extinction response was performed is plotted for each extinction session. The data from all four extinction groups are shown.

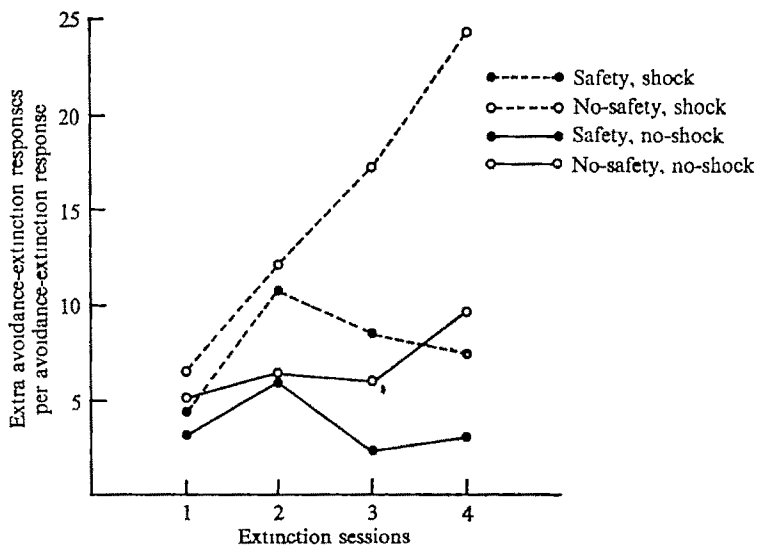

Fig. 4. The mean number of extra avoidance–extinction responses per avoidance–extinction response trial is plotted for each extinction session. The data from all four extinction groups are shown.

($P < 0{\cdot}05$) in the mean number of extra avoidance responses across the last four acquisition sessions.

Extinction: avoidance–extinction response data

The number of sessional trials on which a response was performed during the warning period was expressed as a percentage of the total number of trials per session and will be referred to as the avoidance–extinction percentage. Fig. 3 presents the group means of the sessional avoidance–extinction percentages for the safety, no-shock; no-safety, no-shock; safety, shock; and no-safety, shock groups.

An analysis of variance was performed on the sessional avoidance–extinction percentages using the same basic design as was used to analyse the acquisition extra response data. The between-subject main effects and interaction were not statistically significant. The main effect of extinction sessions was significant ($F = 10{\cdot}99$; d.f. = 3, 60; $P < 0{\cdot}0001$). The interactions of sessions × safety signal and of sessions × safety signal × shock were not statistically significant. However, the interaction of sessions with the shock factor was significant ($F = 7{\cdot}11$; d.f. = 3, 60; $P < 0{\cdot}006$).

The interaction between the shock factor and sessions was further analysed by testing the simple effects of the shock factor at each level of the sessions factor. A significant difference between the shock and no-shock groups was found on the first extinction session ($F = 4{\cdot}81$; d.f. = 1, 20; $P < 0{\cdot}04$). The two shock groups combined made 51 avoidance–extinction responses on this session, whereas the no-shock groups combined made only 30 avoidance–extinction responses. The simple effects of the shock factor were not significant on the second, third or fourth sessions.

The simple effect of sessions at each level of the shock factor was also computed. There was no significant difference between the numbers of avoidance–extinction responses performed by the no-shock groups as a function of sessions. However, there was a significant decrease in the avoidance–extinction percentage over extinction sessions for both shock groups ($F = 17{\cdot}52$; d.f. = 3, 60; $P < 0{\cdot}001$).

Extinction: extra response data

An analysis of variance was performed on the sessional mean number of responses made in the safety and inter-trial periods following an avoidance–extinction response; that is, the number of extra responses. Fig. 4 presents the sessional means of the number of extra responses following an avoidance–extinction response for the four treatment groups. The same basic design as was used to analyse the acquisition extra response data was employed. The main effect of the safety signal factor was significant ($F = 4{\cdot}42$; d.f. = 1, 20; $P < 0{\cdot}05$). Subjects in the safety groups made an average of 5·7 extra responses following an avoidance–extinction response, whereas no-safety subjects made an average of 10·9 extra responses.

The main effect of the shock factor was significant ($F = 6{\cdot}54$; d.f. = 1, 20; $P < 0{\cdot}05$). Subjects in the shock group made an average of 11·5 extra responses following an avoidance–extinction response, whereas no-shock subjects made only 5·2 extra responses. The interaction of the safety with the shock factor was not statistically significant. The main effect of sessions was significant ($F = 3{\cdot}76$; d.f. = 3, 60; $P < 0{\cdot}05$). The interaction of sessions × shock and of sessions × safety

signal × shock were not statistically significant. However, the interaction of sessions with the safety signal factor was significant ($F = 3{\cdot}44$; d.f. = 3, 60; $P < 0{\cdot}05$).

The interaction between the safety signal factor and sessions was further analysed by testing the simple effects of the safety signal factor at each level of the sessions factor. There were no significant differences between the safety and no-safety groups on the first and second extinction sessions, but significant differences were evident on the third and fourth sessions ($F = 5{\cdot}71$; d.f. = 1, 20; $P < 0{\cdot}05$ and $F = 6{\cdot}73$; d.f. = 1, 20; $P < 0{\cdot}05$, respectively). The two safety groups combined made 5·4 extra responses on the third session and 5·3 extra responses on the fourth session compared with 11·6 and 17·0 extra responses on the third and fourth sessions respectively for the no-safety groups.

The simple effect of sessions at each level of the safety signal factor was also computed. There was no significant difference between the mean number of extra responses following an avoidance–extinction response for the safety groups as a function of sessions. However, there was a significant increase in the number of extra responses during extinction sessions for the no-safety groups ($F = 6{\cdot}23$; d.f. = 3, 60; $P < 0{\cdot}01$).

Discussion

The results showed that the acquisition of a discriminated avoidance response is facilitated by a feedback stimulus additional to the inter-trial stimulus. A similar finding has been reported for learning under a free operant avoidance schedule (Hurwitz *et al.*, 1972; Grossen & Bolles, 1968). The conclusion to be drawn is that the feedback stimulus has the functional property of a reinforcer and that this property is independent of any reinforcing property attributable to the termination of a warning signal (Mowrer, 1956; Rescorla, 1969). It is probable that the reinforcing property of the feedback stimulus is due to the fact that it reliably signals a period which is free from shock. Denny (1971) has suggested that these signals come to control relaxation responses which are antagonistic to the anxiety that underlies avoidance behaviour. The manner in which the safety signal controls relaxation seems analogous to the warning signal's control of conditioned fear responses.

Two procedures were used to extinguish the avoidance response: complete omission of shock and unavoidable shock at the end of the warning signal. Even though both groups finally extinguished to a comparable level, extinction was initially retarded when shocks were always given. The heightened level of fear produced by such regular signal–shock pairings would enhance responding, as suggested by Mowrer's (1939) dual-process theory. Eventually, however, the shock would suppress such responses. This could be interpreted as a learning of 'helplessness' (Seligman *et al.*, 1971), in which an organism, although fearful, learns that there are no responses it can perform to change the situation.

Alternatively, one could invoke the punishment hypothesis which states that in the extinction of avoidance behaviour, responses consistently followed by shock are suppressed. The delay between the response and the shock in the present case would have slowed the development of response suppression. Unfortunately the design of our experiment does not allow us to determine which of these alternative explanations accounts most succinctly for the extinction results obtained.

Even though the safety signal facilitated acquisition of the avoidance response, no difference in the rate of extinction with or without a safety signal was observed. This might be explained as follows. At first the safety signal helped to identify different periods within the experimental session: the warning signal indicated that shock was impending, whereas the safety signal initiated a period during which no further shocks would be presented. During experimental extinction, in the case where shock was never again presented, the warning signal was no longer followed by shock. Therefore, it would not be functionally different from the safety signal, both stimuli now indicating periods free from shock. In the case where shock was always presented, the safety signal would again no longer be functionally different from the warning signal, in this case because both predict shock. In the absence of a response, the warning signal was followed by shock; if a response was made, shock was delivered during the safety signal. According to this interpretation both extinction procedures result in the elimination of the functional difference between the warning and safety signals upon which the reinforcing power of the safety signal depends.

However, there was a significant difference between the safety and no-safety groups in extinction, namely that the number of extra avoidance–extinction responses was greater under the no-safety condition. This difference is mainly attributable to an increasing number of extra avoidance–extinction responses over sessions by the no-safety, shock group. In our discussion of the avoidance–extinction response data, we showed how the shock-delivery extinction procedure resulted in a heightened level of fear and thus an initially higher level of avoidance–extinction responding. It appears that this heightened level of fear and the accompanying responding persisted into the inter-trial period now that the response was no longer effective in avoiding shock. The data show this to be the case in the no-safety, shock group. However, in the safety, shock group this extra responding did not persist, a result which attests to the power of the safety signal in inducing a state of relaxation during extinction, thus counteracting the energizing effect of the shock.

Even though the safety signal was not shown to reinforce avoidance–extinction responding *per se*, it was shown to control the level of extra avoidance–extinction responses under the shock-delivery extinction procedure. It is possible that the safety signal does have reinforcing properties effective on avoidance–extinction responses, but that the techniques and measures employed may not have been sensitive enough to measure the effect. On the other hand, it could be that the power of the safety signal to maintain avoidance–extinction responding is not inseparable from its relaxing effects during the inter-trial periods.

This research was supported by Predoctoral Public Health Service Fellowship no. 5-FL-MH-31, 439-02 (P2) from the National Institute of Mental Health to P. V. Dillow and by USPHS grant no. MH-12115-03 to H. M. B. Hurwitz.

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(*Manuscript received* 14 *August* 1970; *revised manuscript received* 21 *November* 1971)

Br. J. Psychol. (1972), **63**, 4, *pp.* 593–596
Printed in Great Britain

OPEN-FIELD BEHAVIOUR IN RATS: EFFECTS OF HANDLING, SEX AND REPEATED TESTING

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The effects of pre-weaning handling, sex and repeated testing on the open-field behaviour of rats were consistent with existing reports and with the interpretation of the defecation and ambulation measures as inversely related indices of emotionality. The main finding replicated a report that although handled rats increase their ambulation with repeated testing in a fashion concordant with the observed drop in defecation, non-handled rats show an initial decline followed by an increase. This latter effect is interpreted in terms of fear-motivated escape behaviour on trial one, since non-handled rats confine their activity largely to the periphery of the field on this trial and their exploration of an object placed in the centre of the field increases over trials in accordance with theoretical expectations.

Defecation and ambulation scores from the open-field test have generally been regarded as inversely related indices of emotionality or fearfulness in rodents since the pioneering work of Hall (1934). High defecation often tends to be associated with low ambulation (Broadhurst, 1957, 1960; Denenberg & Morton, 1962), but not invariably (Pare, 1964; Russell, 1970).

Whimbey & Denenberg (1967) have argued that although defecation is determined largely by emotionality, the ambulation score is influenced both by emotionality and an independent *exploration* factor. This analysis may resolve some of the problems associated with the test. In particular, the progressive decrement in defecation over a series of trials (Broadhurst, 1957; Hall, 1934) may not be inconsistent with reports of an accompanying decrement in ambulation (Pare, 1964), since the latter may be attributable to the habituation of exploratory responses.

However, interpretation of the effects of repeated testing is complicated by another factor. Whimbey & Denenberg have also found that the first trial ambulation score is *positively* related to emotionality and suggest that the initial reaction of a frightened rat may be to run in an attempt to escape from the field. Hence a drop in ambulation could represent a decrease in escape behaviour rather than a decline in exploration. Levine & Broadhurst (1963) and Levine *et al.* (1967) have found that emotional (high defecating) rats show an initial drop in ambulation from the first to second trials with a subsequent increase, whereas non-emotional rats ambulate increasingly over trials.

It is suggested that a clearer picture of the effects of repeated testing of rats differing in emotionality may be obtained by considering a longer series of trials than the usual four or five and by modifying the basic open-field apparatus. Corman & Shafer (1968) found that the introduction of a novel stimulus into the centre of the field increased the time spent there, which is attributed to exploration of the stimulus. It may be that possible escape and exploration components of ambulation can be 'separated out' by making use of this modification.

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METHOD

Subjects

Forty black hooded rats (strain PVG/C) were used, 20 male and 20 female. Half of the animals had been handled prior to weaning and half were non-handled.

Apparatus

The open field used was closely similar in all important respects to the 'standardized' version described by Broadhurst (1960) except that a small object was positioned in the centre of the floor. The object was a heavy metal cylinder 4 cm high and 4 cm in diameter with a cylindrical top 2 cm high and 2 cm in diameter; the top was white and the body of the cylinder was black with a white band 1·3 cm wide around the middle.

Procedure

Handling. At birth complete litters were assigned randomly to one of the two conditions of handling or no handling. Litters that contained fewer than eight animals at weaning were not used. On each of the 20 days between birth and weaning, the cages of the handled rats were removed singly from the rack and placed on a bench. When the mother had been removed from the cage, the pups were taken individually and at random from the nest and each placed in a separate compartment of a 'handling box'. This was an open wooden box that contained 12 compartments, each measuring 8·16 × 8·16 × 8·16 cm. When all the pups in the litter had been removed they were replaced in the same manner. The operation time for each animal was approximately 30 sec. Non-handled animals remained under normal colony conditions and were undisturbed during this period.

Maintenance. The maternity cages were of plastic and contained bedding material. At 21 days of age, the animals were weaned and placed with litter mates in wire mesh cages. At approximately 40 days they were ear-punched, weighed and caged with like-sex litter mates of the same group. During testing each rat was housed individually.

Testing. At approximately 100 days of age subjects were tested in the open field. Each animal was tested for 3 min. per day on each of 10 consecutive days. Subjects were transported individually to and from the test room in a 2 gal. white plastic bucket with a fitted lid. The number of faecal boluses deposited in the bucket prior to testing was recorded and added to the number deposited in the open field to give a total defecation score for each trial. A measure was taken of the total number of floor sections entered per trial (ambulation), with a separate count of the number of times the *centre* section was entered.

RESULTS

The results of the analyses of variance on the three measures are summarized in Table 1. Fig. 1 depicts the mean defecation and ambulation scores of the groups on each of the 10 trials.

DISCUSSION

The effects of sex and handling accord with the literature. If emotionality is characterized by high defecation and low ambulation, handling reduces emotionality (e.g. Levine *et al.*, 1967) and males are more emotional than females (e.g. Broadhurst, 1957).

Three of the four groups also show a decline in defecation with repeated testing. The significant interactions obtained on this measure result from the fact that handled females defecate little overall and reveal no appreciable decline.

On the ambulation measure, in addition to the main effect of trials, there is a strong treatment by trials interaction but no sex by trials interaction. Non-handled rats of both sexes are least active on the middle trials of the series, whereas the scores of

Table 1. *Summary of the analyses of variance for ambulation (total sections entered), defecation (number of faecal boluses) and entries into the centre section of the open field*

Source of variation	D.F.	Ambulation		Defecation		Entries in centre	
		F	*P*	*F*	*P*	*F*	*P*
Sex	1	11·61	< 0·01	7·67	< 0·01	4·52	< 0·05
Handling	1	17·67	< 0·001	9·84	< 0·01	29·89	< 0·001
Sex × handling	1	< 1	n.s.	< 1	n.s.	< 1	n.s.
Subjects within groups	36	—	—	—	—	—	—
Trials	9	7·90	< 0·001	177·75	< 0·001	51·33	< 0·001
Sex × trials	9	1·50	n.s.	33·25	< 0·001	< 1	n.s.
Handling × trials	9	2·30	< 0·05	41·62	< 0·001	1·98	< 0·05
Sex × handling × trials	9	2·74	< 0·01	15·25	< 0·001	2·60	< 0·01
Trials × subjects within groups	324	—	—	—	—	—	—

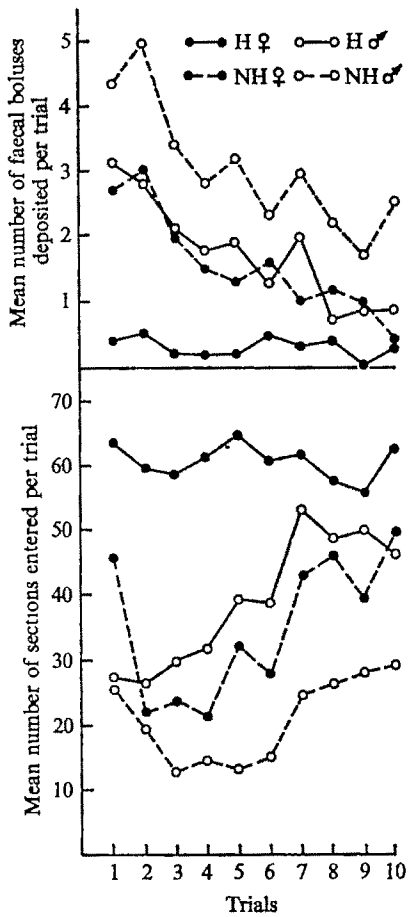

Fig. 1. Mean number of sections entered, and faecal boluses deposited, per trial as a function of treatment and sex (H = handled, NH = non-handled).

handled rats increase straightforwardly over trials (this effect is small for the highly active handled females). In general form, then, these results strikingly resemble those Levine *et al.* (1967) take as evidence of escape behaviour among the emotional non-handled rats on the early trials.

Accepting this interpretation, it may be necessary to assume that the increment in ambulation due to escape is 'superimposed' on the normal sex difference in ambulation in order to explain why non-handled females ambulate more on the early trials than the (more emotional) non-handled males.

Consideration of the centre section entries data proves favourable to the escape hypothesis. On this measure the trials effect appears as a *straightforward increase* in entries into the centre over the series for all groups. Thus, although non-handled rats enter more peripheral sections on trial one than on two, three, four and five, they enter the *centre* section less often on trial one than on trial five. If we regard entries into the centre as exploratory responses directed at the object, the data suggest that exploration increases as the suppressive effect of fear becomes less with habituation. This measure appears to be relatively 'uncontaminated' by escape behaviour, and it may be that frightened rats seek escape near the field walls rather than by running into the exposed centre.

It appears that, at least in the present experimental situation, fear inhibits exploration to the extent that as fear declines exploration increases. This is manifest as an increase in the number of entries made into the centre section of the field and, in relatively non-emotional rats, as an increase in the overall level of ambulation. The latter effect is obscured by the influence of escape behaviour on the first trial ambulation scores in the case of emotional rats.

We are grateful to Miss Julie Hall and to Mrs Gillian Bailey for their assistance in conducting this experiment.

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(*Manuscript received* 11 *October* 1971; *revised manuscript received* 3 *December* 1971)

Br. J. Psychol. (1972), 63, 4, *pp.* 597–603
Printed in Great Britain

IKONIC IMAGERY IN THE SEVERELY SUBNORMAL

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Ability to reproduce and transpose matrices was studied in 23 adult SSN trainees. These subjects could transpose the matrices at a MA equivalent to the CA at which children normally do. The ability to reproduce the same matrices was not in evidence prior to the ability to transpose them, as it is in normal children. One possible interpretation of this finding is that cognitive development in SSNs is qualitatively different from that of normal children.

Bruner *et al.* (1966) assert that the course of cognitive growth is governed by the child's increasing dependence on the symbolic mode of representation. The infant is to begin with exclusively dependent on the enactive mode, representing his world in terms of gross motor movements. With the development of imagery, ikonic representation becomes possible and during the years of early childhood such representation is the dominant mode. Symbolic representation, which is most powerfully employed in the transformation of information into verbal codes, attains dominance in later childhood, normally about age seven to eight years. All three modes are then fully available to the child and later cognitive development is marked chiefly by the elaboration of, and increasing dependence on, the symbolic mode. Typical evidence for his sequence is provided by Bruner & Kenney (1966, chap. 7) in an experiment involving the reproduction and transformation of a 3×3 matrix composed of beakers varying in height and diameter. They find that children of age five can successfully reproduce the matrix but are generally unable to transpose it till around age seven. The former task may require only effective ikonic imagery while the latter requires a symbolic transformation; their results therefore support the general hypothesis.

In an extension of this experiment, Mackay *et al.* (1970) presented children of five to eight years with three 3×3 matrices of differing task complexity. The first comprised laminas varying in shape and colour; the second comprised cylinders varying in height and colour; the third, as in the original study, comprised cylinders varying in height and diameter. The three completed matrices are shown in Fig. 1. The construction of these matrices requires, in the first case, the logical multiplication of discrete variables (e.g. squares $\times$ red things = red square); in the second case, the multiplication of a discrete variable (e.g. black) with a relational variable (e.g. short); in the third case, the multiplication of asymmetrical transitive relations (e.g. tall $\times$ narrow). These differences in complexity were reflected in the results. In terms of the number of subjects correctly reproducing and transposing these matrices, the Bruner & Kenney matrix was very much more difficult than the other two. Less than one-third of the sample of eight-year olds could correctly transpose the former while two-thirds could transpose the latter. Except in the case of the simplest matrix, significant differences were found within matrices between the numbers successful on reproduction and transposition, with more children being successful on the reproduction task. Analysis of the configuration of responses on the reproduction task showed that by far the most popular response was that of returning the matrix

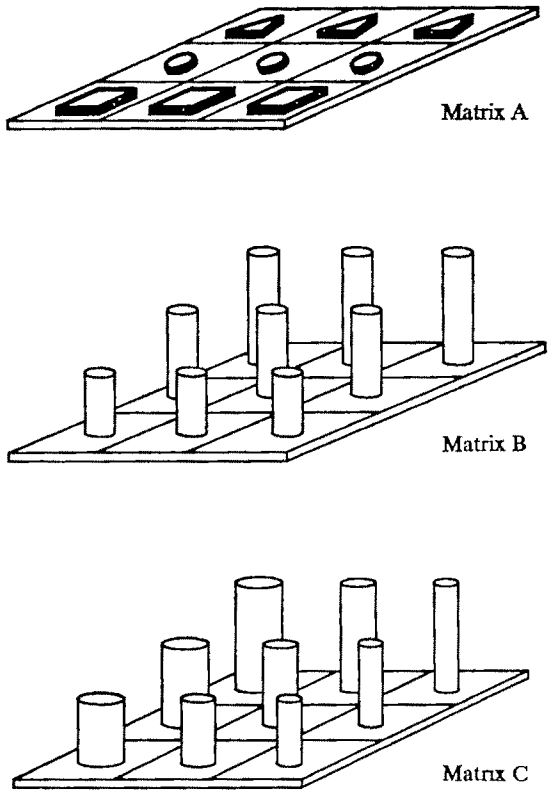

Fig. 1. The three matrices.

exactly as it had been presented, although a number of logically equivalent and hence correct alternatives was available.

Thus there is disagreement with Bruner & Kenney over the age norms for successful completion of the reproduction and transposition of a double seriation matrix. More important, there is agreement over the order of acquisition of these two abilities and in the interpretation of it. In the reproduction task, it is sufficient to reproduce the matrix from an ikonic image of the array; in the transposition task, it is necessary to be guided by a symbolic 'rule'. The former ability is in evidence earlier than the latter.

The present investigation is an attempt to ascertain whether a sample of non-institutionalized severely subnormal (SSN) adults of similar mental ages presents the same developmental pattern. The results may have implications for the retarded *v.* abnormal development controversy; particularly for the question of whether one can accept MA as an index of developmental level or merely as an index of ability level.

METHOD

Subjects

The subjects were adult SSN trainees attending a day centre in Aberdeenshire. All trainees were interviewed but a number had to be excluded from the analysis for the following reasons: absence for long periods during the experiment, gross sensory handicap, withdrawn psychotic

behaviour, apparent colour blindness, and/or a measured IQ above 60. Colour vision was tested during the experiment (see Procedure); MA and IQ were assessed on the Stanford–Binet Form L–M. The final sample comprised 23, 10 males and 13 females, median CA 20 years, range 17–52, median MA 6 years 0 months, range 3 years 4 months to 9 years 6 months.

Materials

Fig. 1 shows the three matrices used. Each matrix was based on a sectioned wooden board. Matrix A comprised ½-in. thick wooden laminas in a 3 × 3 arrangement of squares, circles and triangles painted red, white or black. Matrix B comprised nine solid wooden cylinders in a 3 × 3 arrangement of heights 6, 4 and 2 in. painted red, white or black. Matrix C comprised nine open-ended grey plastic cylinders in a 3 × 3 arrangement of heights 6, 4 and 2 in. and diameters 4, 3 and 2 in.

Procedure

All subjects were tested on the Stanford–Binet Form L–M. At a later session, they were presented with the matrices in order ABC or BAC. This allowed counterbalancing for transfer effects across A and B; the small number of subjects did not allow for further counterbalancing. Each subject was shown his first matrix and asked to complete the following tasks.

Replacement. The experimenter explained that he would remove 'things' from the board and ask the subject to 'put them back in their right places'. He then removed the top-right object and asked the subject to replace it. This was followed by replacement of the top-right and bottom-left objects and, finally, the three objects on the top-left to bottom-right diagonal. Any mistakes on replacement were noted and then immediately corrected by the experimenter.

Reproduction. The subject was told the objects would be removed and scrambled and he would be asked to 'put them all back in their right places'. If he did not reproduce the matrix correctly, the experimenter noted the attempt and then removed the objects, scrambled them and replaced them correctly, without comment, before proceeding to the next task.

Transposition. The objects were removed, scrambled and the object from the bottom-right cell was placed on the board in the bottom-left position. The subject was instructed to 'put them all back the way you think they should be when we leave this one [pointing] here'.

The same procedure was used for all three matrices. On completion of these tasks, all subjects who had failed to maintain colour consistency in the reconstruction of the matrices were presented with the following task. The three tall cylinders from matrix B were set out before the subject in the order red, white, black. Replicas of these cylinders were presented singly in the order white, red, black, white, black, red and the subject was asked to match the replica with its original. Subjects who failed on this task were excluded as being potentially colour-blind. (In fact, only one subject was excluded on these grounds.)

Results

Scoring

Replacement. One point was scored for each object correctly replaced. The maximum score on each matrix was 6.

Reproduction and transposition. One point was scored for each row and column where one of the relevant variables had been kept consistent, e.g. a row of reds, a column of triangles, a row of tall cylinders. The maximum score for either reproduction or transposition on each matrix was 6.

Individual results are set out in Table 1. Adopting the criterion of six points for a 'pass', these data are summarized for the reproduction and transposition tasks in Table 2. The data of Table 2 show that, for this sample, (1) matrix C is more difficult than either A or B; (2) matrices A and B are of equivalent difficulty; (3) Reproduction and transposition are of equivalent difficulty on matrices A and B. The numbers passing on matrix C are too small to allow any conclusion on relative difficulty but

Table 1. *Individual scores*

	S	Sex	Age (years)	MA (years and months)	Matrix A (shape and colour) Repl.	Repro.	Trans.	Matrix B (height and colour) Repl.	Repro.	Trans.	Matrix C (height and diameter) Repl.	Repro.	Trans.
Gp 1 sequence ABC	1	M	24	3–8	6	0	0	3	0	0	4	0	0
	2	M	17	5–3	6	1	3	4	3	1	3	0	0
	3	M	30	9–6	6	6	6	6	6	6	6	0	1
	4	M	52	6–9	6	1	4	6	0	4	6	6	1
	5	M	22	7–6	6	6	6	6	6	6	6	6	1
	6	M	41	7–6	6	3	3	6	3	3	6	1	1
	7	F	21	3–4	6	4	6	3	4	6	6	0	0
	8	F	18	9–6	6	6	6	6	3	3	6	1	1
	9	F	25	6–2	6	6	6	6	6	6	6	1	2
	10	F	18	7–0	6	6	6	6	6	6	6	6	6
	11	F	17	3–8	6	0	0	3	0	0	3	0	0
	12	F	17	6–0	6	4	4	6	0	4	4	3	3
		Mean = 25		Mean = 6–4									
				Totals	72	43	50	61	37	45	62	24	16
Gp 2 sequence BAC	1	M	23	7–2	6	1	1	4	4	0	6	1	3
	2	M	19	4–5	4	0	0	4	1	4	6	1	0
	3	M	20	6–0	6	2	1	6	6	6	6	0	3
	4	M	27	3–4	6	6	0	0	0	0	6	0	0
	5	M	18	7–4	6	6	3	6	6	6	6	2	1
	6	M	20	4–10	6	1	2	6	4	4	6	6	3
	7	M	31	6–3	6	1	0	4	0	0	3	0	0
	8	F	21	7–0	4	1	3	6	5	1	3	0	0
	9	F	18	8–0	6	6	6	6	6	6	4	1	0
	10	F	17	3–11	4	0	1	4	0	1	4	0	1
	11	F	20	7–6	6	6	6	3	6	6	4	1	1
		Mean = 21		Mean = 6–0									
				Totals	60	30	23	49	38	34	54	12	12
				Overall totals	132	73	73	110	75	79	116	36	28

Table 2. *Numbers correctly reproducing and transposing the three matrices*

	Matrices					
	A		B		C	
	Reproduction	Transposition	Reproduction	Transposition	Reproduction	Transposition
Pass	9	8	8	9	4	1
Fail	14	15	15	14	19	22

Table 3. *Partial scores on different dimensions*

Matrix	Dimension	Reproduction	Transposition	Total
A	Colour	33	40	73
	Shape	40	33	73
B	Colour	35	36	71
	Height	40	43	83
C	Height	20	21	41
	Diameter	16	7	23

when partially correct scores are considered (the overall totals of Table 1), there are no significant differences between performance on any of the matrices. (For matrix C, $t = 0{\cdot}90$; d.f. $= 22$; $0{\cdot}40 > P > 0{\cdot}30$.)

The correlations between total scores on reproduction and total scores on transposition for the three matrices ($r = +0{\cdot}80$), between total scores on reproduction and MA ($r = +0{\cdot}63$), and between total scores on transposition and MA ($r = +0{\cdot}55$) are all significant beyond the 1 per cent level. (For d.f. $= 21$, $P = 0{\cdot}01$ when $r = 0{\cdot}53$.)

The proportions of those successfully reproducing the matrix who reproduced it exactly as it was presented (although alternative correct reproductions were possible) were: matrix A, 6:9; matrix B, 7:8; matrix C, 4:4. The proportion of failures on the transposition task who reproduced the original matrix with only the instructed (bottom-left and bottom-right) items transposed were: matrix A, 0:15; matrix B, 2:14; matrix C, 1:22.

Table 3 presents the total points scores on the relevant variables in the three matrices. No significant preferences are found for colour *v.* shape or colour *v.* height (matrix B, colour *v.* height totals, $t = 1{\cdot}04$; d.f. $= 22$; $0{\cdot}40 > P > 0{\cdot}30$). There is a significant difference in performance in favour of height *v.* diameter on the transposition task of matrix C ($t = 2{\cdot}77$; d.f. $= 22$; $0{\cdot}02 > P > 0{\cdot}01$).

Discussion

These results suggest that the ability to reproduce and transpose matrices in SSNs is related to MA and, in the case of transposition, the MA at which the ability appears is comparable with the CA at which the ability was found in normal children in the Mackay *et al.* (1970) study. However, the ability to reproduce a matrix is not, as in normal children, an earlier acquisition than the ability to transpose the same

matrix. In this sample, the abilities appear together. This might mean that the development of representation in SSNs does not follow the pattern suggested by Bruner for normal children. There is no indication in the present results that the SSN subjects were attempting to reconstruct 'something from the template of ikonic imagery' as the children in the previous studies had appeared to do (cf. Bruner *et al.*, 1966, p. 159). It seems more likely that the method used to reproduce the matrix depended not on ikonic imagery but on some form of symbolic representation similar to that used to transpose the matrix. There are three sources of evidence for this claim. First, there is the synchrony of successful performance on the two tasks. Secondly, although the majority of subjects reproduced the matrix exactly as it was presented, the larger range of alternatives offered on the simplest matrix elicited a wider range of responses. Thirdly, the number of failures on transposition through attempting to reproduce the pattern presented with only the bottom corners transposed (a fairly frequent error with young children) was trivial.

Such evidence suggests that SSNs may use a different strategy from children of equivalent MA in attempting to solve these problems. In the case of the SSN subjects in this sample, there is no evidence of their dependence on an ikonic mode of representation. This lack of dependence may be due to poor visual imagery although it is not necessarily so. Memory impairment, inability to categorize, insufficient practice to allow registration of the array, are some of the competing hypotheses. The point being made here is that SSN adults, unlike normal children of equivalent MA, do not seem to employ visual imagery as a *dominant* mode of representation. Some support for an hypothesis of lack of dependence on visual imagery can be drawn from the well-established finding that visual discrimination in SSNs is generally impaired (e.g. House & Zeaman, 1958). Again, recognition of visually encoded material has been found to be less efficient in SSNs than is stereognostic recognition (Mackay & Macmillan, 1968). If the present findings are confirmed and the hypothesis accepted, there will be important theoretical and practical implications. The hypothesis implies that the cognitive development of SSNs is not merely retarded but is qualitatively different from normal children. This would necessitate the reappraisal of the validity of matching SSNs and normals on MA for some experimental purposes. It would also, at the applied level, support Hermelin & O'Connor's (1961) suggestion that alternative teaching methods to those favouring visual aids should be considered for imbecile children.

Finally, the fact that these subjects showed no preferences among colour, shape or height in the tasks presented but that they dealt more successfully with height than diameter is offered simply as an interesting addition to the present morass of data pertaining to the question of the relative salience of colour, shape and size as discriminative cues.

I wish to thank Dr Harold Ross and the staff and day patients of Wellwood Home, Cults, Aberdeen for their cooperation.

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(*Manuscript received* 12 *January* 1972)

Br. J. Psychol. (1972), 63, 4, *pp.* 605–615
Printed in Great Britain

THE DEVELOPMENT OF SENSITIVITY TO FIGURAL AND STYLISTIC ASPECTS OF PAINTINGS

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To determine whether pre-adolescent subjects can learn to classify paintings consistently by style or by figure, matched groups of subjects at each of two age levels (7 and 10) were given a pre-test, a seven-week training session, and two post-tests. Subjects at both age levels demonstrated the ability to sort paintings according to a consistent criterion; the older group could more readily alter the basis of its classifications when instructed to do so; style sensitivity was not found to be dependent upon concrete operational thought. Strategies used by subjects and the role of Gestalt perception and operational thought in style sensitivity are discussed.

Sensitivity to artistic style has recently been recognized as a fruitful area for investigation; the ability to place together works by the same artist is a complex form of cognition, involving objects drawn from daily experience, interesting but not overly familiar to children. Several studies have indicated that pre-adolescent children do not spontaneously speak of style, that they do not understand an instruction to sort by style (Machotka, 1966; Gardner & Gardner, 1971), and that they exhibit a strong tendency to place together paintings which feature the same dominant figure or subject matter, even when explicitly instructed to sort by style (Gardner, 1970*a*, *b*). Some investigators have accordingly hypothesized that formal operational thought is a prerequisite for style sensitivity (Machotka, 1966); others have proposed that concrete operational thought is a necessary precondition (Frechtling & Davidson, 1970).

A child's failure to sort paintings by style may be due to an inability to perceive attributes relevant to style detection or to an inability to take them into account in the face of attraction to a dominant geometrical figure or subject matter (Gardner, 1972; Rubin, 1921). If the child cannot perceive such attributes as texture or composition, because of a deficit in operational thinking, a short training programme is unlikely to result in style sensitivity; if, however, the child's difficulty is in making use of information already at his disposal, training might well reveal considerable style sensitivity. Furthermore, if the relevant attributes can be perceived directly by the child without operations upon classes, ability to sort by style may be independent of operational level; style sensitivity may be a form of Gestalt perception (Lorenz, 1966), in which all works by an artist, like all triangles or all dogs, are seen as having characteristic relationships among components. Should style sensitivity emerge after brief training, an explanation of this ability in terms of differentiation of stimulus features (Gibson & Gibson, 1955) or attention to the relevant stimulus dimension (House & Zeaman, 1963) would be supported.

To determine whether pre-adolescents can become sensitive to style and whether such sensitivity depends upon operational level, a training and testing programme

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was instituted. Style sensitivity was operationalized as the ability to place together works by the same artist in the face of competing figural and subject-matter cues. As the training was non-verbal, it was assumed there were sufficient cues within the works to permit sorting by style. Cues judged less central to style (like colour or size) were eliminated or minimized; it was then assumed that composition, expressiveness and, above all, texture (i.e. microstructural regularities which extend across figure and ground; Gibson, 1950; Gardner, 1971*c*) would be the most important cues in style detection.

The plan of the study was as follows. During the first week all subjects took a pre-test in which their natural classificatory tendencies were elicited. Over the next seven weeks, subjects were given weekly practice in sorting paintings either by figure or by style. After the training, subjects took four additional tests at weekly intervals: a post-test to determine whether learning had occurred; two transfer tests whose results are discussed elsewhere (Gardner, 1971*c*); a repetition of the post-test with instructions to sort according to the alternative criterion. Finally, younger subjects were given four tests of concrete operational thought.

The study sought answers to the following questions: (1) Can pre-adolescents learn to sort painting reproductions consistently by style or consistently by figure? (2) What are the learning trends at two age levels? (3) Does exposure without differential reinforcement to sets of paintings enable the individual trained in one way to sort in the other as well? (4) Is stylistic sensitivity dependent upon operational level? Evidence was also sought on sex differences; strategies used by subjects; the relationship of the findings to earlier studies of style sensitivity.

METHOD

Subjects

Subjects were selected at random from second and fifth grades in an elementary school in a Boston, Massachusetts suburb having a mixed, though predominantly middle-class population. The group which received training consisted of 48 individuals: 12 males and 12 females at each of the two grade levels. The modal ages of the subjects at the time of the first post-test were 7 and 10.

Materials

Following Gardner & Gardner (1970), 230 sets of painting reproductions were devised, each set consisting of reproductions of four paintings, two by one artist (A), two by another (B). One painting by A and one painting by B had strong figural properties in common; the other paintings by A and B also had common figural aspects but always ones markedly different from those shared by the first pair. The reproductions represented a wide range of schools and periods; paintings were trimmed to prevent sorting on the basis of signature; sets in which colour, size, medium, or other salient cues would coincide with style were avoided. The sets were shown to three judges who eliminated ones inconsistent with the guidelines. Then twenty sets were chosen for the pre- and post-tests and seven other groups of 20 sets were assembled for the training, making a total of 160 sets to be used in the study. Some sets which were difficult to group by figure were deliberately included in the middle and latter weeks of the training, in order to maintain the interest of the figure group and to encourage subjects to consider carefully the figural aspects of the paintings. For each set three groupings were possible: by style (or painter), by figure (or subject matter), and a third anomalous way which cut across the other classifications. This third way was important in determining whether subjects were becoming sensitive to style, or simply learning not to group by figure.

Procedure

At the start of the first week subjects were shown the first set of paintings and told: 'Here are some pictures. I want you to look at them carefully and then put together those paintings which you think look most alike. Make two piles with two paintings in each pile.' Subjects were not allowed to proceed on this or any other test until they correctly paraphrased the instructions. No feedback was given but the subjects were generally encouraged and received a prize at the end of the session.

After pre-testing the population was divided into two groups: a figure group (FG) which would be reinforced verbally for grouping by figure and a style group (SG) which would be reinforced verbally for grouping by style. The FG and SG at each age level included six males and six females and had approximately the same proportion of high (5–8 style responses) and low (0–4 style responses) scores. So that the groups would also be roughly equivalent in intelligence and motivation, the lists were reviewed with the teachers who suggested a few modifications.

On the second week subjects were told: 'I want you to make two piles of paintings again but this time there will be a special way of doing it. Try to figure out the special way. I will tell you whether you are right; and I will show you the special way if you try it another way. We'll be doing this for some weeks and if you try your best to learn the special way, you will get a prize'. Similar procedures were followed during the remaining six weeks of training. On the ninth week subjects were told: 'Now I'd like to see how well you've learned the special way'. The pre-test was administered without feedback as a post-test; afterwards all subjects received a prize.

Three weeks later subjects were once again given the pre-test. They were told: 'Today I'd like you to match the paintings another way'. The experimenter demonstrated the 'other way' (figure for SG; style for FG) on three practice items and subjects practised with correction on two additional items. Thereafter, there was no feedback. Immediately after this testing, all younger subjects were given four tasks of concrete operations: conservation of substance (Piaget, 1965); seriation of sticks; and two class-inclusion problems (Inhelder & Piaget, 1964). These tasks were selected because they represent the range of concrete operational thought, with seriation among the easier tasks, conservation of substance among the more difficult ones, and class-inclusion problems varying in difficulty depending on the classes being compared and the amount of training (Sigel & Hooper, 1968). It was hypothesized that if style sensitivity depended on operational level, there would be a positive correlation between performance on the style tasks and the operational tasks, particularly the class-inclusion ones.

Following Piaget, subjects were rated from stage I (pre-operational) to stage III (operational) on each problem and an overall score of operativity was computed.

Two experimenters conducted the study, each working with half of each subject group throughout the study. Relevant variables, such as order of the items in each test, were counterbalanced.

Results

Training of figure and style sensitivity: the first post-test

Success in learning the two classificatory modes can be ascertained by a comparison of a subject's scores on the pre-test and the first post-test. A series of non-independent, three-factor repeated-measures analyses of variances was performed on these scores, with age and sex as between-subject variables and test form as within-subject variable. The changes in number of figure (f), style (s), and other (o) responses given by the two groups after training will be reviewed first.

s responses. The figure group (FG) decreased its number of s responses ($F = 26{\cdot}93$; d.f. = 1, 20; $P < 0{\cdot}01$): the style group (SG) increased its number of s responses ($F = 62{\cdot}07$; d.f. = 1,20; $P < 0{\cdot}01$).

f responses. The FG increased its number of f responses ($F = 16{\cdot}51$; d.f. = 1,20; $P < 0{\cdot}01$): the SG decreased its number of f responses ($F = 117{\cdot}36$; d.f. = 1,20; $P < 0{\cdot}01$).

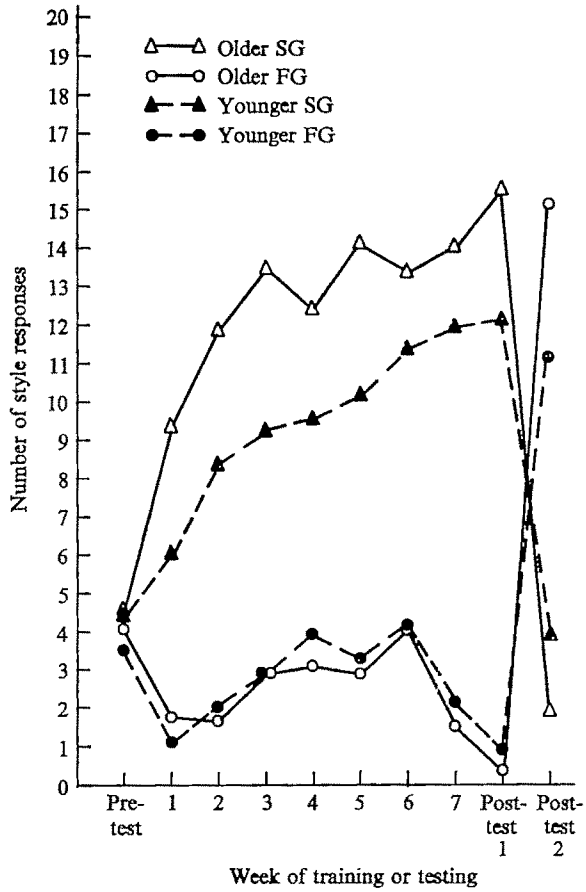

Fig. 1. Mean number of style responses given by subjects during training and testing sessions.

o responses. The FG did not change the number of anomalous (*o*) responses ($F < 1$; d.f. = 1, 20; n.s.): the SG increased its number of *o* responses ($F = 7{\cdot}92$; d.f. = 1, 20; $P < 0{\cdot}025$.) The increase in *o* responses must be interpreted with caution, however. The absolute number of *o* responses was very small, with one child accounting for nearly half of the *o* responses of the fifth grade population in the post-test (13 of 27).

Since there was an increase in *o* responses given by the SG, the number of *o* and *s* responses given by these subjects on the first post-test was compared. All the younger and 11 of 12 older subjects gave more *s* than *o* responses on the post-test ($P < 0{\cdot}01$, binomial distribution). This result, in combination with the above findings, refutes the possibility that SG subjects were simply learning not to sort by figure.

Interactions and other effects. Further analyses showed an age effect on the *s* responses of the SG ($F = 4{\cdot}40$; d.f. = 1, 20; $P < 0{\cdot}05$) and an age effect on *f* responses of the SG ($F = 6{\cdot}80$; d.f. = 1, 20; $P < 0{\cdot}025$). These effects indicate that older subjects were more sensitive to the demands of each test. As no sex effects or interactions of sex with other variables were significant, the sex variable was dropped from subsequent analyses.

Additional aspects of these results can be readily seen in Figs. 1–3. Fig. 1 points up the slight increase in *s* responses given by the FG when the *f* items became more

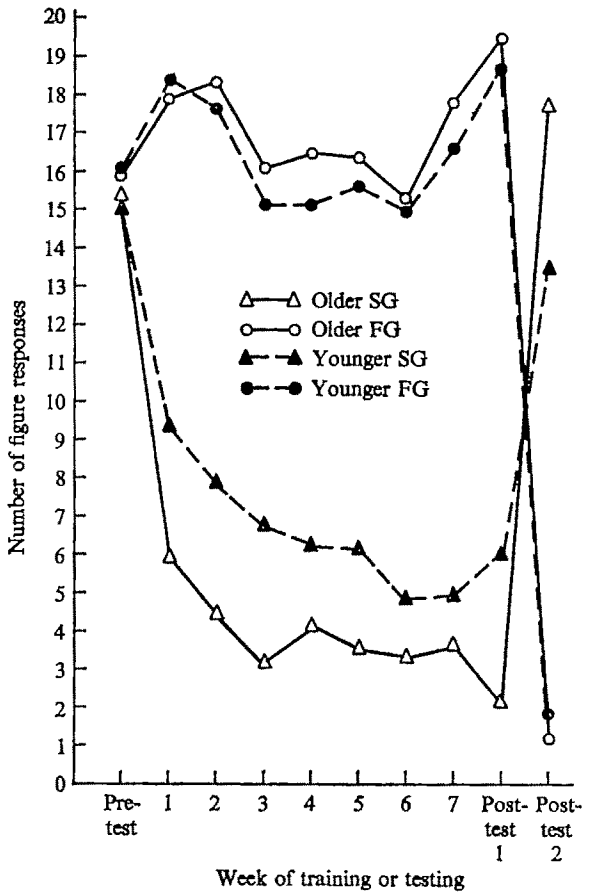

Fig. 2. Mean number of figure responses given by subjects during training and testing sessions.

challenging; the rapid learning of the *s* grouping by the older SG subjects; the gradual improvement of the younger SG subjects during training. Fig. 2 documents the rapid fall of *f* responses for both age groups in the SG condition; the persistent tendency of the younger SG subjects to make somewhat more *f* responses under contrary instructions; the steadily high scores of FG subjects throughout training. Fig. 3 reveals the lack of appeal of the *o* method and the initial experimentation with it by members of the SG.

Effect of instruction change: the second post-test

Whether individuals trained to sort paintings on one basis can also sort by the opposite criterion was investigated in the second post-test. A series of non-independent, two-factor repeated-measures analyses of variance, with age as the between-subject variable and test form (post-test 1 *v.* post-test 2) as the within-subject variable yielded the following findings.

s responses. The number of *s* responses given by the style-trained group decreased ($F = 98{\cdot}55$; d.f. $= 1, 22$; $P < 0{\cdot}01$); the number of *s* responses given by the figure-trained group increased ($F = 238{\cdot}47$; d.f. $= 1, 22$; $P < 0{\cdot}01$).

f responses. The number of *f* responses given by the style-trained group increased

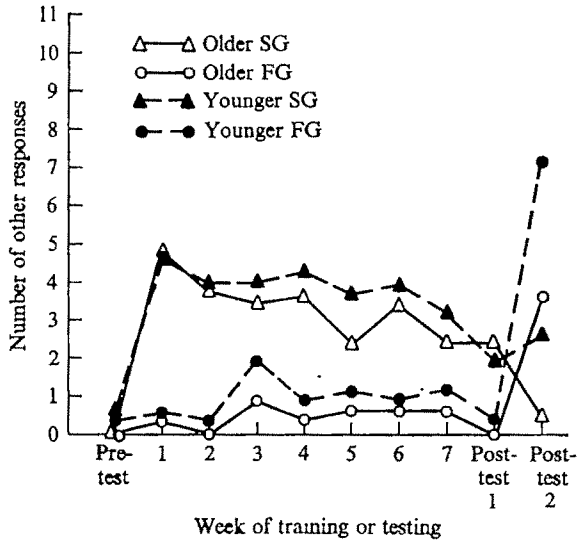

Fig. 3. Mean number of other responses given by subjects during training and testing sessions.

($F = 139{\cdot}13$; d.f. $= 1, 22$; $P < 0{\cdot}01$); the number of *f* responses given by the figure-trained group decreased ($F = 904{\cdot}77$; d.f. $= 1, 22$; $P < 0{\cdot}01$).

o responses. The number of *o* responses given by the style-trained group did not change ($F < 1$; d.f. $= 1, 22$; n.s.); the number of *o* responses given by figure-trained subjects was higher ($F = 55{\cdot}32$; d.f. $= 1, 22$; $P < 0{\cdot}01$). Clearly the figure-trained subjects were more tempted by the anomalous choices than were the style-trained subjects, who reverted easily to a figural strategy.

Interactions and other effects. The age factor alone was significant in two analyses: the number of *s* responses by figure-trained subjects ($F = 7{\cdot}74$; d.f. $= 1, 22$; $P < 0{\cdot}02$) and the number of *o* responses by figure-trained subjects ($F = 6{\cdot}27$, d.f. $= 1, 22$; $P < 0{\cdot}05$). The interaction between age and test was significant or approached significance for the number of *f* responses by the style-trained subjects ($F = 17{\cdot}02$; d.f. $= 1, 22$; $P < 0{\cdot}01$), the number of *o* responses given by style-trained subjects ($F = 3{\cdot}99$; d.f. $= 1, 22$; $P < 0{\cdot}07$), the number of *s* responses given by the figure-trained subjects ($F = 8{\cdot}06$; d.f. $= 1, 22$; $P < 0{\cdot}01$) and the number of *o* responses given by figure-trained subjects ($F = 5{\cdot}19$; d.f. $= 1, 22$; $P < 0{\cdot}05$). Each of these effects and interactions is consistent with the interpretation that older subjects attended more carefully to the instructions and the paintings, thereby furnishing more appropriate responses.

The results indicate that older FG subjects could sort by style as well as SG subjects and that younger figure-trained subjects gave almost as many *s* responses as did the SG when each was instructed to sort by style ($t < 1$; d.f. $= 22$; n.s.). However, upon examination of further indices, a more revealing pattern emerges among the younger subjects. The SG gave an average of 1·83 *o* responses on the first post-test, while the FG gave an average of 7·08 *o* responses on the second post-test ($t = 5{\cdot}65$; d.f. $= 22$; $P < 0{\cdot}01$, two-tailed). Furthermore, the SG had a mean of 6·08 *f* responses on the first post-test, while the FG had a mean of 1·83 *f* responses on the second post-test ($t = 3{\cdot}45$; d.f. $= 22$; $P < 0{\cdot}01$). Finally the number of *s* responses given by the FG

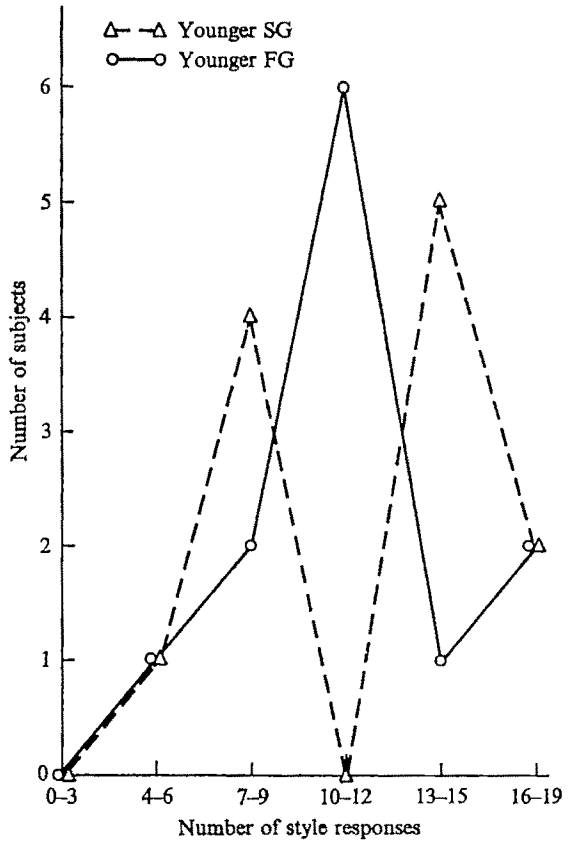

Fig. 4. Number of younger subjects giving a certain proportion of style responses when instructed to group by style.

on the second post-test tended to cluster around the mean, while the *s* responses of the SG on the first post-test had 2·05 times the variance and possessed characteristics of a bimodal distribution (cf. Fig. 4). The bimodal distribution of the SG and the clustering tendency of the FG is demonstrated by the fact that 8 out of 12 of the FG but only 1 out of 12 of the SG gave either 9, 10, or 11 *s* responses in the relevant post-test ($\chi^2 = 9{\cdot}6$; d.f. = 2; $P < 0{\cdot}01$). These results suggest a more complex response pattern among the younger SG subjects. Some subjects had mastered the *s* sorting, while others lacked a consistent basis for sorting; hence the large number of *f* responses and the bimodal distribution on the first post-test. The FG on the second post-test simply rejected the *f* grouping and divided their responses among the alternatives.

To have a sensitivity criterion stricter than mere improvement, two arbitrary performance levels were defined. Subjects classifying 12 or more sets by *s* or *f* were considered 'probably sensitive'; those sorting 15 or more consistently were considered 'definitely sensitive'. Over half the subjects at both ages in both conditions were definitely sensitive to figure. Two-thirds of the older subjects in both condition were definitely sensitive to style but of the younger subjects only those in the SG showed appreciable style sensitivity (four out of 12 at the definite level; three additional subjects at the probable level).

Other findings

Operational level. Seven of the second grade subjects (4 SG and 3 FG) were diagnosed as concrete operational (all responses at level III); eight (4 SG and 4 FG) as pre-operational (all responses at level I); the remaining subjects were somewhere in-between. There was slight evidence for horizontal décalage among the tasks, with the seriation problem proving easier than the others, but no sign of a positive correlation between the class inclusion and the style tasks. A variety of analyses were done to detect a connexion between degree of operativity and style sensitivity but no suggestion of a link could be shown. For example, one III FG subject went from 19 *f* sortings on the first post-test to 16 *s* sortings, but one I FG subject went from 20 *f* sortings to 17 *s* sortings; a number of operational subjects never learned to sort by style. It was concluded that concrete operations are neither necessary nor sufficient for stylistic sensitivity.

Strategies. Information on the subjects' strategies was obtained from subjects' spontaneous comments and from the experimenters' observations of various behavioural indices, such as hesitation and surprise. The FG did not need to devise strategies, as they were asked only to exhibit their natural way of grouping. About half of the older subjects explicitly noted alternative ways of classifying and a number experimented with a newly discovered way. The older SG soon realized that *f* groupings was never rewarded and began to search for another basis of classifying. A few subjects knew at once that manner of representation was a crucial variable; by the third week, most of those who would eventually sort consistently by style were already relying on brushstrokes, texture, or an overall judgement of similarity.

Many of the younger subjects, on the other hand, were shocked to learn of alternative ways of grouping and expressed disbelief when the 'special way' was shown. Most initially embraced less subtle hypotheses, e.g. position, colour, size, brightness, familiarity, realism. Three approaches proved especially difficult to abandon: relating the pictures by a story; placing together nudes or religious pictures: assuming one picture was embedded in another.

Those subjects who did not remain with an unproductive strategy frequently arrived at correlated hypotheses (Dulany, 1961) which, though not isomorphic with style, nonetheless improved their performances. The most popular strategy was to think of the pictures as belonging to a family; subjects would indicate that a man and woman 'look like they are related' or that two pictures 'make a family'. Subjects also grouped pictures because they belonged in a certain setting (a church, subway or book); reflected similar vintage (from olden times); or featured pigment distributed on the surface in characteristic ways (bumpy, drippy, smeary, thick). Though such approaches were not devoid of figural aspects, they enabled subjects to converge on aspects more germane to stylistic classification: the occasion of production, manner of representation, regularities across the painted surface.

While individual patterns varied, successful subjects tended to keep track of their hypotheses, move the pictures around, and ponder the variety of similarities. Less successful subjects seldom moved their eyes, focused on the experimenter, and failed to study corrections.

Discussion

The results leave little doubt that pre-adolescents can sort paintings in a consistent manner. At both age levels there were dramatic increases among the SG in sorting by *s*, as well as a solidification of figural tendencies in the FG. Furthermore, there was no evidence that operational level is a principal determinant of style sensitivity. Most of the younger subjects had not yet reached concrete operations; yet when reinforced for sorting by style, a majority of the younger subjects performed at a high level.

What steps appeared to engender style sensitivity? In all cases subjects had to focus on the material within the picture, disregard the most obvious figural aspects, and attend to corrections. The various correlated hypotheses were helpful, since they directed the subject's attention to more relevant factors, such as the way figures were represented or the family resemblances among an artist's work. Incidental comments suggested two particularly effective approaches. In one case, subjects looked at all the pictures and, disregarding figural aspects, made an overall judgement of similarity. Though this approach may seem primitive, connoisseurs often base their judgements on such global impressions (Gardner, 1971*b*). Other subjects determined that the way the picture would feel, if one could touch it, was crucial; they grouped together pictures which were rough, smooth, or pasty. This approach, together with the results on the transfer test, confirmed the contribution made by texture to style sensitivity.

Both approaches may rely upon a mechanism which Lorenz (1966) terms 'Gestalt perception'. This capacity to focus on the essential attributes of a class of stimuli is viewed as a constancy which develops to a remarkable degree in man, enabling preschool children to make fine discriminations among species, materials, or vehicles. If such a capacity exists in children, they should be able to recognize specific artists by noting the recurrent regularities in their works. Similarly they should develop sensitivity to textural aspects, given sufficient exposure to a range of textures. The present findings support the hypothesis that pre-adolescents can form Gestalten for certain artists, styles, or textures. Indeed Gestalt perception seems crucial in style perception, a task requiring the subject to classify in only one way: his ability to shift classificatory bases or reason hypothetically is not at issue, except in so far as he must suppress a previously dominant mode of classification. The findings are also consistent with the view that the ability to sort by style results from attention to the relevant dimensions of a display (House & Zeaman, 1963) or from adequate differentiation of information contained within the stimulus (Gibson & Gibson, 1955). Style sensitivity may be more closely related to discrimination learning or to the discovery of distinctive features than to class inclusion or conservation.

An unanticipated finding was that subjects in the FG could sort by style when so instructed. While some older subjects in both groups may already have learned to sort by style, this explanation cannot account for SG subjects who never learned the *s* sorting, or whose performance gradually improved. Therefore it seems probable that some older FG subjects were learning the alternative classification during training. Evidence in favour of latent learning comes from two sources: experimentation during training with 'another way', of which subjects had recently become aware;

the fact that figure-trained subjects performed better on the second post-test than the style group had performed during the initial weeks of training. Younger figure-trained subjects, on the other hand, gave little evidence of latent learning. When asked to sort by style, they divided their responses fairly evenly between the *s* and *o* sortings. It seems likely that older FG subjects had surplus processing capacity, while younger subjects devoted all their attention to the given task. The developmental changes which occur during the pre-adolescent period would appear to make a contribution to style sensitivity.

The findings supplement those of previous studies on sensitivity to style. Earlier suggestions that pre-adolescents might be sensitive to style (Walk *et al.*, 1970) have received persuasive support. It has been shown that, as in the literary and musical realms (Gardner & Gardner, 1971; Gardner, 1971*a*), children have a strong tendency to sort on the basis of figural aspects; a training paradigm can alter the basis on which they group works of art; style sensitivity is not dependent on operational level but may reflect the capacity to detect recurrent textures or Gestalten in a range of works.

This article reports part of a dissertation supported by the National Science Foundation through Thesis Grant GS-2625 to Harvard University, and also by the National Institute of Mental Health, and the Office of Education through a grant to Harvard Project Zero. I would like to thank Professor Roger Brown, Professor Marshall Haith, and Dr Judith Gardner for their wise counsel throughout the study; Professor Rudolf Arnheim, Professor John Kennedy, and Dr David Perkins for their cogent advice on the design and write-up; Jeff Lister and Ellen Bussard for their expert running of the subjects, the staff and pupils of the Underwood School in Newton Massachusetts, for their hospitality throughout the study.

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(*Manuscript received* 8 *October* 1971)

Br. J. Psychol. (1972), 63, 4, *p.* 617
Printed in Great Britain

PERCEPTUAL CONSTANCY OR PERCEPTUAL COMPROMISE: A NOTE

By ROBERT H. THOULESS*

The typical experiment in 'perceptual constancy' is one in which an unchanging physical object is observed under changing stimulus conditions (variation of inclination, distance or illumination), and the phenomenal character is found to change less than does the stimulus character, i.e. to remain relatively constant. The opposite experimental situation, in which the phenomenon changes although the stimulus remains the same while the 'real' character of the object changes has been relatively neglected. A typical experimental situation of this kind in shape perception is provided by a subject sitting immediately under a projector which throws a circular image on to a screen. This appears as circular when the screen is at right angles to the line of vision, but if the screen is rotated about 45° round a vertical axis, all subjects report the apparent shape of the projected image as ellipitical with long axis horizontal, although the retinal image obviously remains circular. Measurement of the ellipticity of the phenomenal figure shows that it is a compromise between the shape of the elliptical image on the screen and the circular retinal image. Such observations suggest the inadequacy of a description of such perceptual effects in terms of a tendency for phenomena to remain constant under changing stimulus conditions.

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(*Manuscript received* 27 *October* 1971)

Br. J. Psychol. (1972), 63, 4, *pp.* 619–620
Printed in Great Britain

A NOTE ON PERSONALITY CORRELATES OF MOTION SICKNESS

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Reason (1968*a*, 1969) has suggested that sufferers from motion sickness (MS) may have nervous systems that are specially 'receptive' to stimulation. In support of this he found that those who recalled more experiences of MS also reported a bigger increase in the duration of the after-effect of a rotating spiral as the time for which they were exposed to the spiral increased. They also increased their estimates of the loudness of tones at a faster rate as the intensity of the tone increased. Reason suggests that this dimension of 'receptivity' of the nervous system may be related to Eysenck's (1955) dimension of introversion–extraversion, greater receptivity being similar to greater introversion.

The experiment reported here examined the relation between reported frequency of MS and the introversion–extraversion and neuroticism scales of the Eysenck Personality Inventory. Reason would predict a positive correlation between reported frequency of MS and introversion. A pilot experiment with 22 subjects obtained positive rank order correlations between reported MS frequency and extraversion ($+0{\cdot}14$, not significant) and between reported MS frequency and neuroticism ($+0{\cdot}49$, $P = 0{\cdot}04$ by a two-tailed test). The present experiment tested these relations further.

Reason's (1968*b*) Motion Sickness Questionnaire for experience of MS since the age of 12, and the Eysenck Personality Inventory, Form B, were completed as a class experiment by 60 members of an Intermediate Vacation Course in Psychology for London external degree students, including a wide range of ages of both sexes. A few were familiar with the EPI but none knew the purpose of the experiment before completing the questionnaires. Spearman's rank order correlation coefficient was computed between reported MS score (RMS), calculated in the way described by Reason, and E score on the EPI (high scores indicating high extraversion) and between RMS and N score on the EPI (high scores indicating high neuroticism).

The rank order correlation between RMS and E was $+0{\cdot}043$ (not significant) and that between RMS and N was $+0{\cdot}457$ ($t = 3{\cdot}91$, $P < 0{\cdot}001$ by a one-tailed test, used in view of the positive correlation in the pilot experiment). Since the results were clear-cut, no correction for tied ranks was carried out.

The correlation of neuroticism with RMS is possibly at variance with the results of Stephens (1970), who found that the reported increase in the loudness of tones as their intensity increased, which Reason found to be positively correlated with RMS, was not significantly related to neuroticism or introversion as measured by the Heron two-part personality inventory (Heron, 1956), but only to Alpert & Haber's (1960) measure of test anxiety. He does not, however, report the size of the correlations obtained with the Heron inventory.

To explore this possible discrepancy, Part I of the Heron inventory was later given

to the same subjects. The results must be regarded with caution since the subjects now knew the purpose of the experiment and twenty did not return the Heron inventory, so the sample obtained was a self-selected one. There were, however, no significant differences between those who did and those who did not return the Heron inventory on RMS, E or N score. For those who did return the Heron inventory the rank order correlations were:

RMS and N (EPI)	$r = +0{\cdot}38$, $t = 2{\cdot}53$, $P < 0{\cdot}02$ (one-tailed test);
RMS and N (Heron)	$r = +0{\cdot}26$, $t = 1{\cdot}78$, not significant;
N (EPI) and N (Heron)	$r = +0{\cdot}60$, $t = 4{\cdot}62$, $P < 0{\cdot}001$ (one-tailed test).

The results suggest that Heron's measure of neuroticism is a less reliable predictor of RMS than the EPI measure. It may be that Stephens' failure to obtain significant correlations between reported increase in the loudness of tones and introversion or neuroticism reflects a weak relation in general between Heron's measure and Reason's 'receptivity'. Substitution of the EPI for Heron's measure in Stephens' experiment might prove instructive.

The low correlation between RMS and E on the EPI refutes Reason's hypothesis that receptives (who recall more MS experiences) are related to Eysenck's introverts. If the E scale does measure excitability of the nervous system in the sense postulated by Reason, clearly this is not related to experience of MS. It is the more neurotic individuals, as measured by the EPI, who report more MS experiences, though whether they have actually experienced more MS requires additional evidence. It seems reasonable to suggest on the available evidence that MS experience is related to general anxiety level.

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(*Manuscript received* 20 *September* 1971)

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Printed in Great Britain

PAPERS TO APPEAR IN FORTHCOMING ISSUES

(Not previously listed)

Asso, D. & Wyke, M., Department of Psychological Medicine, National Hospital, Maida Vale, London. Verbal descriptions of spatial relations in line drawings by young children.

Bennett, S., Davies, P. & Earnshaw, R., School of Psychology, University of Bradford. Constancy of mnemonic capacity as a function of trials in free recall learning.

Craske, B. & Crawshaw, M., Department of Psychology, University of Southampton. Latency of autokinesis and convergence of the eyes.

Csikszentmihalyi, M. & Getzels, J. W., Committee on Human Development, University of Chicago, Illinois. The personality of young artists: an empirical and theoretical exploration.

Dale, H. C. A., Department of Psychology, University of Hull. Short-term memory for visual information.

De Montpellier, G. & Nuttin, J. R., Department of Psychology, University of Louvain, Belgium. On 'Michotte's experimental methods' and 'Michotte's ideas'.

Fellows, B. & Thorn, D., Department of Social Studies, Portsmouth Polytechnic, Hampshire Terrace, Portsmouth. A test of Piaget's explanation of the Müller-Lyer illusion.

File, S. E. & Jew, A., Sir John Cass School of Science and Technology, City of London Polytechnic. Syntax and the recall of instructions in a realistic situation.

Fried, P. A., Department of Psychology, Carleton University, Ottawa, Canada. The septum and hyperreactivity: a review.

Gibney, N., Purdysburn Hospital, Belfast. Delayed auditory feedback: changes in the volume intensity and the delay interval as variables affecting the fluency of stutterers' speech.

Gregson, R. A. M. & Baker, A. F. H., Department of Psychology, University of Canterbury, Christchurch 1, New Zealand. Sourness and bitterness confusions over sequences of taste judgements.

Grieve, R. & Wales, R. J., Department of Psychology, University of St Andrews. Passives and topicalization.

Hoffman, H. S., Newby, V. & Stratton, J. W., Department of Psychology, Pennsylvania State University, University Park, Penna., U.S.A. Reinforcement of distress vocalization by presentation of an imprinted stimulus.

Kennedy, R. A., Department of Psychology, University of Dundee. Associative encoding of words in passive sentences.

Kniveton, B. H., Department of Psychology, University of Nottingham. The effect of rehearsal delay on long-term imitation of filmed aggression.

Russell, P. A., Department of Psychology, University of Aberdeen. Open-field defecation in rats: relationships with body weight and basal defecation level.

Salter, D., Department of Psychology, University of Newcastle upon Tyne. Capacity and delayed speech.

Br. J. Psychol. (1972), 63, 4, *pp.* 623–651
Printed in Great Britain

BOOK REVIEWS

Personality and National Character. By R. LYNN. Oxford: Pergamon Press. 1971. Pp. vii + 200. £3.75.

This book, entitled *Personality and National Character*, proves to be far more limited in scope than its title implies. It also suffers from some peculiarities in definition and logic which impair its value as a scholarly effort.

To summarize the content: Professor Lynn's study is statistical in character. It reports the incidence of mental illness (hospitalization for), calorie intake, suicide rates and alcoholism rates for some 11 countries, and then develops the thesis that variations in these rates are a function of variations in anxiety level in the countries concerned. The 11 countries were rank-ordered according to degree of anxiety by integrating the results of three separate investigations: a study by Cattell and Scheier (1961) which assessed anxiety level in six countries using an anxiety questionnaire; a study by Cattell which assessed anxiety level in students in seven countries, utilizing the 16PF Personality Factor test, from which it is possible to derive an anxiety score; and additional data collected by Professor Lynn from a population of Irish students, also using the 16PF. The result of this procedure is to identify France, Japan and West Germany as the three top-anxiety countries, while Canada, the U.S.A. and the U.K. occupy ranks 9, 10 and 11. After applying correlational techniques, Professor Lynn concludes that a high level of anxiety in the population of a country is associated with a high rate of suicide and alcoholism and a low rate of mental illness and calorie intake. The inverse relationship also obtains. Professor Lynn then goes on to examine how other population characteristics might fit into this pattern. He finds that high-anxiety countries have a high incidence of vehicle accidents, a low incidence of coronaries and low tobacco consumption. The incidence of duodenal ulcer and hypertension is found to be unrelated to anxiety level. Finally, Professor Lynn tests the possibility that other population characteristics might be involved, and successively rules out relative affluence, urbanization, political instability, the strength of Roman Catholicism, and rate of economic growth. Two factors regarded as in some degree responsible for differences in anxiety level are climatic factors and race.

In evaluating this study, one comes first to several problems in definition. One has to do with the meaning of the term 'national character'. In his summary of his argument, Professor Lynn says: 'four measures of national character are taken, namely calorie intake and rates of suicide, hospitalized psychosis and alcoholism' (p. 179). This implies that national character is some entity which is expressed by or defined by these four factors. But these are population characteristics, quite different from what is usually meant by national character, i.e. modal personality structure which is specifically Japanese, American, German, etc. The problem of definition does not end here, for at other points Professor Lynn does seem to refer to a modal personality. On page 63 he suggests that his own term, 'anxiety', puts on a scientific basis views of the Anglo-Saxons as unemotional and phlegmatic and the French and Italians as excitable and emotional. Similarly, in commenting on Sir Cyril Burt's views, Professor Lynn says: 'he described the Mediterranean peoples as "vivacious, impulsive, fickle, choleric, alternatively melancholy and gay, light-headed and hot blooded". In contrast, the Nordics are "reserved, sanguine ... and cold blooded". These differences look like the anxiety differences with which we have been concerned' (p. 171). Here it appears that national character is not being defined by population characteristics but by a variable called by Professor Lynn 'anxiety' and measured by other means altogether.

A second major area of confusion has to do with the term 'anxiety'. (This is the only personality variable dealt with in the book, so for 'personality' read 'anxiety'.) Professor Lynn defines this term as follows: 'As used in this book, the term anxiety is intended to convey both the implications of worry and strong motivation. Which is predominant depends on its intensity. At a low level of anxiety an individual is exceptionally emotionally stable and free from all kinds of worry and nervous tension. In its middle range, anxiety manifests itself as nervous energy or strained or solicitous desire, and it is here that it is used in the sense of a person being anxious to achieve something. This usage has a slight connotation of worry, but the chief implication is

one of strong motivation. It is only when worry becomes intense that it takes on the Freudian associations of angst and becomes a neurotic condition which both induces unhappiness and interferes with endeavour' (pp. 17–18).

This definition suggests that anxiety is a characteristic which varies in quantity, i.e. it can be placed on a continuum which involves degrees of intensity, or *more* or *less* anxiety. But the definition also implies that anxiety has a different *quality*, depending on its quantity. Professor Lynn discusses at some length the views of others about anxiety, and points to the fact that others have identified uncorrelated subfactors. By an analogy with intelligence, he justifies using the term 'anxiety' as a general factor, despite the fact that subfactors can be identified. In the rest of the book, the term seems mostly to be used to refer to a single entity varying in intensity or quantity. But just what this single entity is varies from time to time through the book. At times the term 'anxiety' seems to be used to indicate emotional expressiveness (South Europeans as against North Europeans); at times it refers to anxiety as a neurotic trait; at times it is used in the meaning of commitment or involvement. Most often it seems to be used in the sense of sympathetic tone and reactivity. The failure to provide a clear definition of anxiety and to use it consistently pervades the book. Examples appear in some of the quotes in the rest of this review.

The data for this study are statistical in nature, i.e. what can be demonstrated is that certain population characteristics tend to covary in certain ways. Inferences about which of these variables causes which others should be made with caution. Professor Lynn presents the following argument: that the degree (or highness or lowness) of anxiety is *responsible* for the incidence patterns reported, and that high or low anxiety in its turn is influenced by climatic and racial factors ('... the concentration of electromagnetic long waves would seem to be the most significant factor affecting national levels of anxiety' (p. 180) and 'A case can also be made for a racial factor in the determination of national anxiety levels'). It would be hard to defend using such terms as 'responsible' and 'determination' when one's data are strictly correlational in character. Consider the following example: 'It is known that university students in the U.K. are somewhat more anxious than other young people of the same age (Lynn, 1959) and since one must pass examinations to become a student, this is prima facie evidence that anxiety is a factor in examination success' (pp. 152–3).

One could argue just as easily that examinations are a factor in the higher anxiety levels of university students. But, in fact, the data do not admit of *any* interpretations about causality, or about which comes first. Similar interpretations about causality appear from time to time throughout the book, sometimes with reference to the main argument, sometimes (as with the above quote) with reference to incidental matters. In consequence, one comes to distrust the leap from data to interpretation.

There are also some specific problems with measurement. Professor Lynn says: 'Hospitalized mental illness is virtually synonymous with psychosis ...' (p. 24), an assertion which certainly can be challenged. Anxiety level itself is based on measures taken of male university students only, but is then ascribed to the total country.

One interpretation in particular contradicts the more usual view, and therefore may warrant special comment. This is Professor Lynn's argument that psychosis is a manifestation of low anxiety. In explaining this, Professor Lynn first says that 'the majority of patients are characterized by low anxiety', and later goes on to say: 'In psychotic depression the patient has feelings principally of despair and loneliness. These are sometimes regarded as signs of anxiety. But it should be noted that one of the most prominent symptoms of psychotic depression is the lack of energy, which is often so pronounced that the patient sits doing nothing for long periods of time. This inertness can be regarded as a lack of anxiety, and has frequently been interpreted in this manner' (p. 25).

In this quotation, the first time that anxiety is mentioned it appears to be used in the sense of pathologically high levels of tension, worry and unease; the second time it is mentioned it appears to be used in the sense of low response level or reactivity. To say that the inertness of (some) psychotics *is* a lack of anxiety is at least an oversimplification of the situation, and fails to consider that inertness (not-feeling, immobility) may be a response to unbearable levels of *experienced* anxiety – that is, anxiety used in another of its meanings.

There is a problem more basic than any of the above, and it is both conceptual and methodological in nature. One can argue with justification that anxiety is a universal human experience

(i.e. anxiety in its meaning of tension, dread, unease), but the way in which this universal feeling is expressed, its overt signs, the situations which arouse it, are very much culturally influenced. When a psychologist sets out to *measure* anxiety, he uses a number of specific indices; he cannot measure the experience directly. Thus Cattell reports that his high-anxiety factor includes high loadings on such items as 'more common frailties admitted', 'more tendency to agree', 'more emotionality of comment', and, of course, many others. But this is true of an American population. Would it be equally true, or would these characteristics mean the same thing, for other populations?

This point can be made most succinctly by referring to the work of Morris and Jones (1955) which Professor Lynn cites as supporting his own rankings of particular countries as high or low in anxiety. Morris and Jones asked university students from a number of countries to rank in order of preference 13 descriptions of alternative 'ways to live'; Cattell and Scheier later suggest that two of these ways imply anxiety. They are: (1) 'Constant activity, striving for improved techniques to control nature and society ... We have to work resolutely and continually if control is to be gained over the forces of nature which threaten us'; and (2) 'A person should let himself be used ... One should be humble, constant, faithful, uninsistent. Grateful for the affection and protection one needs, but undemanding.' It would be more prudent to say that these ways of life may imply anxiety *to an American*. It is not too surprising that these ways of life were most favoured by the Indians and Chinese, and then by the Japanese, and least favoured by Americans. The assumption that these two life styles imply anxiety does not take into account, for example, that a dependent mode is more acceptable in the Japanese culture, where it is built into both family and work structure in ways which do not occur in Western cultures. Further, Japanese students might well choose the first of these two ways of life because of the high value, in their culture, placed on commitment and exerting one's utmost effort. There is no reason to suppose that anxiety level has much to do with these selections. (And, in fairness to Morris and Jones, there is no indication from Professor Lynn's account that they themselves interpreted their data in this way.)

What this point adds up to is failure to take sufficiently into account the complexities and subtleties of cultural differences and, associated with this, too little critical examination of the problems of measurement.

These comments have been negative in character, pointing to certain deficiencies in definition, measurement, and interpretation. If one abstracts from this study its various deficiencies and their consequences, what remains? Essentially, what remains is a set of data pertaining to population variables, and to climatic, economic and racial factors, with information about which of these covary and in what ways. These covariances are in themselves intriguing and worthy of attention. Professor Lynn has included certain variables which are often disregarded (e.g. race, climate) but which nevertheless deserve to be considered. From the data thus assembled, it would be possible to define a number of hypotheses about how it is that these phenomena intercorrelate in the ways that they do. Personality variables, including anxiety in its various possible meanings, may prove to be involved. Professor Lynn's book, however, cannot be considered a definite explanation of the relationship he has identified.

DOROTHY STOCK WHITAKER

Woodworth and Schlosberg's Experimental Psychology. By J. W. KLING and LORRIN A. RIGGS. London: Methuen. 1972. Pp. xiv + 1279. £9.50.

Woodworth's classic *Experimental Psychology* of 1938 was radically revised by Woodworth and Schlosberg 16 years later in 1954. After a further 18 years it has now undergone an even more radical alteration at the hands of Schlosberg's successors at Brown University. The contributors, under the leadership of Kling and Riggs, are to be warmly congratulated upon the completion of this massive and scholarly work. With its most obvious competitors now somewhat long in the tooth, it seems clearly destined to become a central influence in the experimental psychology of the 1970s.

The chapters with their authors are as follows: Introduction (Riggs and Kling); Psychophysics: Discrimination and Detection (Engen); Psychophysics: Scaling Methods (Engen); Basic Mechanisms of Neural Function (C. F. Stevens); The Cutaneous Senses (Kenshalo); The

Chemical Senses: Taste (L. Bartoshuk); The Chemical Senses: Olfaction (Mozell); Audition (Thurlow); Vision (Riggs); Colour Vision (Boynton); Effector Mechanisms in Vision (Alpern); Perception: Colour and Shape (Hochberg); Perception: Space and Movement (Hochberg); Learning: Introductory Survey (Kling); Positive Reinforcement (Kling and Schrier); Aversive Behaviour (Church); The Discriminative Control of Behaviour (Blough and Lipsitt); Motivation (A. K. Bartoshuk); Properties of Verbal Materials and Verbal Learning (Cofer); Theoretical and Experimental Approaches to Human Learning (Millward); Transfer, Interference and Forgetting (Postman). Having studied some sections where I am knowledgeable and others where I am not, I am sure that teachers and students alike will be delighted to have these clear and authoritative surveys.

At the same time, the reviewer is not entirely happy about the accumulated changes which have taken place in Woodworth's famous book. The alterations which the present third edition makes in relation to the second edition, when compared with the alterations which the second edition made in relation to the first, are similar in direction but markedly greater in extent. The increase from one to two authors, and then to 19 authors, is accompanied by an increase from 800 to 900 pages, and now to 1200 much larger pages. The price rises from about £1.75 to £3.50 and now to £9.50, while the weight goes up from 42 to 46 ounces and now to 83 ounces. Unfortunately these changes are not trivial. The first two editions were sufficiently cheap and handy to become the student's constant and invaluable companion. The third edition is so expensive and cumbersome that I am afraid it may be relegated to the position of a work of occasional reference.

A similar pattern of accelerated change is shown in the more fundamental matters of the contents and their order. The original 1938 edition was conceived primarily as a survey of human experimental psychology. Its 30 chapters ranged widely, covering not only the established topics, but also venturing into such outlying fields as aesthetics, feeling, and creative thought. There was relatively little on animal behaviour, and virtually nothing on the physiological background. It began with topics of immediate human interest such as memory, and only later asked the student to consider the technical questions of psychophysics and sensory perception. The second 1954 edition was reduced to 26 chapters. Three excellent chapters on emotion remained, but the sections on feeling and aesthetics were dropped. Instead, there was increased coverage of animal studies, and more on scaling. At the same time, the material on memory began to move towards the end, bringing psychophysics and sensory perception nearer the beginning. The present 1972 edition is further reduced to 21 chapters. Emotion, reaction time, attention and problem-solving seem to have largely disappeared, and there is even greater stress on animal work and quantification. A new chapter on physiology has appeared. The book now begins with psychophysics and physiology, progresses through sensation and perception, and concludes with learning in its multifarious forms.

These alterations are no doubt in many ways defensible, but they constitute a departure – I would say a retreat – from Woodworth's original conception. Woodworth wanted to present experimental psychology as a study which had already thrown light on a remarkable variety of areas of human behaviour, and which might hope to achieve still broader scope in the future. He mentioned child development and maladjustment as challenging problems, and wrote that 'we are inclined to claim for experimental psychology a scope as wide as that of psychology itself . . .'. Today, with the growth of social psychology and personality, it might have been hoped that the experimentalist would be moving forward, however cautiously, to extend his range. Instead, it seems that this book expresses, and will encourage, a withdrawal. Despite the increase in size, the topics considered in human psychology are less varied and less adventurous; and the centre of gravity of the book is shifting. It is moving towards a conventional presentation of biological foundations rather than towards wider human concerns which Woodworth envisaged; and like so much psychology conceived in this style, it is in danger of becoming an obstacle course for anyone with an interest in human nature. My admiration for Woodworth's prodigious breadth of knowledge and interest grows as I see it slipping from the grasp of his successors.

R. B. JOYNSON

Fact and Fantasy in Freudian Theory. By PAUL KLINE. London: Methuen. 1972. Pp. x + 406. £5.00.

The aim of this book, the author states hopefully in his introduction, is 'relatively simple'. It is 'to establish what parts of Freudian theory have been confirmed or, at least, could be confirmed by objective, scientific psychological research. In short, to establish psychoanalysis as a true science'. With this aim in mind the author begins the book with an excellent statement on the nature of scientific method and how it may be applied to a study of psychoanalytic theory. Four chapters are given over to the Freudian theory of psychosexual development and its implications for personality types. In dealing with this and other psychoanalytic notions Kline gives the reader a brief but adequate (and well-documented) outline of the theory. He next discusses the importance of the theory in psychoanalysis, which means looking at its broader implications. After this he teases out of the theory any hypotheses which are open to empirical validation. The sources of these hypotheses often take the author away from Freud's writings to those of other psychoanalysts, such as Jones, Glover, Abraham and Reich, to mention but a few. In the next stage Kline presents the 'objective evidence' for the validity of the particular hypotheses. There is a useful description of each of the studies plus critical comments and conclusions by Kline himself. After an exhaustive review of the studies pertaining to a particular theory, Kline draws what conclusions he can about the status of the various hypotheses. Each section ends with some discussion of the theoretical and empirical restrictions which must qualify these conclusions.

The other theories that Kline examines include: the Oedipus and castration complexes; the structure of the mind; the mechanisms of defence; Freudian dream theory; and (a section which the author admits is extremely difficult to study empirically) psychoanalytic theories of neurosis, psychosis, and other psychogenic disturbances.

Kline also has a chapter in this book called 'The Effects of Psychoanalytic Therapy', which is more balanced than the usual discussion of this controversial topic to be found in the relevant literature. There is also a chapter on the neo-Freudian and other analytic psychologies.

The final chapter, headed 'The Status of Freudian Theory in the Light of the Evidence', suggests that any blanket rejection of Freudian theory as a whole simply flies in the face of the evidence. This well-supported conclusion is a timely reminder to those would-be experimental clinical psychologists that psychoanalysis is not one theory, but several theories; and that they are as deserving of a critical examination as any other set of theories. Many contemporary clinical psychologists have thrown the baby out with the bath water; this is a great pity because many of those who are doing behaviour therapy are rediscovering painfully some of the insights that Freud and other psychoanalysts commented upon many decades ago. Sigmund Freud deserves the respectful and scholarly examination that Paul Kline has given his work. Even the alleged arch-enemy of psychoanalysis, Professor H. J. Eysenck, said of Freud that 'the brilliance of his mind has opened doors which no one now would wish to close again, and his keen insight has given us a store-house of theories and hypotheses which will keep researchers busy for many years to come'.

The author of this praiseworthy book has carried out a much-needed task of clearing the tangled undergrowth in that forest of theories, notions, fantasies, myths, and half-baked ideas which constitute psychoanalysis. The person who is ambivalent about psychoanalysis – as I suspect many psychologists are – can no longer justify his ambivalence on the grounds that he cannot see the wood for the trees. Paul Kline's book throws that platitude out of the window. In all, the author lists 15 major and minor concepts which he feels are satisfactorily verified. For the most part these concepts are cardinal points of Freudian theory. Many concepts must be severely delimited, given the findings of objective studies. There are few good experiments which actually refute psychoanalytic theory. Many areas which have not been confirmed have not been put to the test. The point about 'good' experiments is an important one. Many of the studies, ranging from factor analytic investigations and psychometric studies to surveys and laboratory experiments, are methodologically unsound. Kline attempts to evaluate the status of these studies, and there is room for disagreement with some of his assessments about their worth. The author raises the fundamental question as to whether the verification of rather important Freudian concepts means that psychoanalytic theory should be retained or incorporated into the body of established psychology. To this end, the author examined those psychoanalytic pro-

positions that were 'verified' in the light of predictions made from theories of learning. He comes to the conclusion that in most cases the Freudian phenomena could be fitted into a model derived from theories of learning, but that these theories on their own could not have predicted the clinical observations. He argues that psychoanalysis remains a vital instrument for the study of human behaviour. Like many theoreticians before him he regards learning theory models and psychoanalytic theory not as opposed, but as closely related. Freudian theory supplies many of the details of the learning procedures, illuminating in particular the nature of 'reinforcement'. He concludes that psychoanalysis should be retained not as a whole but only after rigorous objective research has revealed what parts are correct or false, or in need of modification. Kline's book goes a long way toward doing this for us.

MARTIN HERBERT

Perceptual Processing: Stimulus Equivalence and Pattern Recognition. Edited by PETER C. DODWELL. New York: Appleton-Century-Crofts. 1971. Pp. xii + 535. $16.50.

One of the most important developments in recent years in the field of shape recognition has been the rise of the model builders. Using ideas derived from computer technology on the one hand and hints from neurophysiological structure on the other, the theorist attempts to construct a working model of a given organism's performance. The parameters of the model are set by the experimental evidence on discrimination learning and the observations of ethology. Once the organism's observed capacities have been successfully simulated, predictions can be made of its behaviour in novel stimulus situations.

Professor Dodwell, one of the acknowledged leaders in this field, has followed his earlier monograph on the subject by editing the present selection of readings. In his choice of material, Dodwell has specifically rejected the comprehensive overview in favour of putting forward the case for his own conception of the problem and its mode of solution. Some 30 articles are reprinted from a wide range of sources and each is prefaced with a critical and explanatory comment. The book is divided into five sections, each of which has a brief introduction by the editor. The first is concerned with the historical antecedents of the modelling approach and includes a previously untranslated paper by Von Holst as well as the better known statements of the problem of stimulus equivalence by Lashley, and Pitts and McCulloch. Curiously, no extract by Hebb is included in this section, despite his pervasive influence on the editor's own ideas. The second part is concerned with theory and reproduces lucid statements of the positions of Deutsch and Sutherland as well as a more technical presentation of Dodwell's own two-stage model of shape recognition. The third portion deals mainly with experimental work relevant to the latter theory, though an effort has been made in the selection of papers to give some conception of the variety of experimental paradigms open to the shape-perception theorist. In addition, two rather difficult papers are included which explore the possibility of the presence of orientation and contour detectors in the human visual cortex. The fourth section, dealing with the neurophysiological basis of pattern recognition, is in many ways the most satisfying in the book. By skilful selection, the editor brings out the increasing complexity of stimulus-analysing mechanisms as one ascends through the animal kingdom from crab to man. Clearly, the days of the grand-scale universal theory are over and in future much greater attention will have to be paid to information from comparative studies in model building. The book concludes with a selection of papers on computerized pattern recognition. In this section the editor has chosen to emphasize, quite correctly in my opinion, the dangers of generalizing too readily from machine to man.

Dodwell is to be congratulated on making available to psychologists a range of primary sources outside their own discipline and for emphasizing their relevance to the problems of shape perception. Unfortunately, such eclecticism of sources does not always make for easy reading. The editor makes creditable efforts to elucidate specialized vocabulary but none to soften the mathematics which figures prominently in some of the papers. For these reasons it seems unlikely to enjoy great popularity among the undergraduate audience for which it was originally produced, unless read in conjunction with the editor's *Visual Pattern Recognition*, the early chapters of which the present volume follows fairly closely. However, it will be a useful addition to the bookshelf of the specialist or academic who requires a handy, if selective, survey of the literature on stimulus equivalence and pattern recognition.

G. M. DAVIES

Inner Speech and Thought. By A. N. Sokolov. New York: Plenum Press. 1972. Pp. ix + 283. $22.50.

Of intellectual skills, such as mental calculation or silent reading, we can say the following. Skill originates in a matrix of overt actions and its long-term development involves qualitative changes in organization which are marked by interiorization and abbreviation of performance; yet however much performance comes to be 'in the head', it never seems fully to rid itself of its overt ancestry and, under conditions of difficulty, is liable to resume its explicitly spelled out character. Can we say likewise of thinking performances in general, on the assumption that they develop in a matrix of language which is, originally, acquired in overt social interaction? The thesis of this book is that we can say this and that much supporting evidence comes from the study of inner speech. 'Inner speech' differs in three main ways from the external, and developmentally prior, speech of social discourse. It is private speech for oneself, occurring while a person is considering, on his own, some issue or problem; it is reduced in intensity to a whisper or even to completely inaudible muscular movements of the vocal apparatus; and it is typically abbreviated, like a cryptic, personal, verbal shorthand. Such covert but detectable speech has long been recognized and, since the turn of the century, has been recorded by mechanical and, later, by electrical techniques. Since 1940, A. N. Sokolov of Moscow (who is not to be confused with E. N. Sokolov) has experimented about inner speech and in 1968 he published a book which surveyed his work. The present volume is a translation of that book, and a welcome addition to the growing body of literature which is making Soviet psychology more fully available to English readers. The book is in three parts. Part 1 is introductory and historical. Part 2 reports how various tasks are affected by articulatory interference, i.e. by mechanical restraint on vocal movements or by preoccupying the vocal apparatus in reciting unrelated verbal matter. Such interferences have variable and sometimes dramatic effects but, in the nature of such experiments, it is a subtle problem to identify the functional level of the interference effects. Part 3, which is the best part of the book, deals with electromyographic records of speech movements (and with GSR and EEG records) taken from people who are carrying out tasks such as translating English passages into Russian or solving Raven's matrix problems. Overall, the book reports, and alludes to, a vast range of issues, but we might try to give its gist by pinpointing three questions. First, in what kind of mental tasks and under what conditions do what kinds of people exhibit recordable inner speech? Briefly, inner speech is greatest in children and in adults who are least skilled or practised in the task in hand, and when the difficulty or novelty of the task is greatest; inner speech is least when the person is skilled and when the task has a large, but not too taxing, visual component. Second, what functions might inner speech serve? Some may be mere overflow; some, like general muscular tonus, may have a diffuse alerting function; and some almost certainly act in a specific way and as a means of analysing and co-ordinating difficult phases of the task. Third, what does inner speech tell us about the organization of thinking performances? With the range of tasks here examined, the general picture is as follows. 'The actual thought process is always – whether the solution of verbal or concrete problems is involved – associated with language, although at individual moments or phases of this process verbal actions may be absent . . . We have no ground . . . to infer the existence of thought without language . . . since the thought process as a whole cannot be replaced by one of its phases. For the same reason thought should not be identified with speech, for thinking contains not only a verbal but also a non-verbal (concrete or object-operational) phase of action, usually accompanied by a certain inhibition of speech processes' (p. 238).

The book has many merits. It studies an attractive diversity of task situations and people. It ranges over philosophy, neurology, the language–thought problem, and the social and biological forces which shape development. It gives finely qualitative accounts of individual performances. It acknowledges individual differences and does not attempt to wish them away by statistical jugglery. There are also some demerits. The discussion is prolix and could well be made briefer and less ambiguous, e.g. 'inner speech' sometimes refers, not to recorded events, but to more central and unrecorded events. There is proneness to give conclusions backed by illustrative instances and to say little about the representativeness of the instances. And there is often insufficient information about the exact conditions of experiments. Nevertheless, the book enables some clear conclusions to be drawn and it adds substantially to previous knowledge as sum-

marized, for example, by the chapter on 'Thought and Motor Reaction' in G. Humphrey's 1951 book *Thinking*. As regards the translation (and its price), mention must be made that it is workaday in intention. The English is adequate but scarcely polished, and no editorial attempt is made to elucidate the frame of reference within which Sokolov is writing. Thus the volume's main value is for readers who already have some acquaintance with Soviet work and with skills research and who want to learn more about Sokolov's detailed technique and findings. But what of a reader who is not in this specialist category yet who wishes to know more about the genuinely exciting Soviet work on cognitive organization and development? He or she will get much greater value out of *A Handbook of Contemporary Soviet Psychology*, edited by M. Cole and I. Maltzman and published in 1969. The *Handbook* contains a chapter by Sokolov which is a shortened version of *Inner Speech and Thought*, plus surveys by other Soviet psychologists about a variety of issues, and an extremely useful introduction to psychological approaches which have, after all, developed in relative isolation from Anglo-Saxon traditions. Without some such orientation, a British reader is likely to be confused, bored and misled by much which the present volume contains.

I. M. L. HUNTER

Cognitive Studies. Volume 2: *Deficits in Cognition*. Edited by JEROME HELLMUTH. New York: Brunner/Mazel. 1971. Pp. x + 363. $15.00.

The 15 contributions to this volume are introduced somewhat ambiguously by Elkind as being 'of a pioneering sort'. Renaissance of cognitive studies or not, at the price this book is offered one expects achievement as well as ambition.

A few of the papers are very good. Kagan's succinct and neatly reasoned review of research on 'reflexion–impulsivity' indicates a basis for understanding and modifying some of the error-making and other behaviours related to deficient problem-solving. The considerable research on overinclusive thought disorder is given a detailed, judicious review by Payne, who concludes that the concept has limited usefulness. Rohwer and Levin reveal that paired-associate learning studies support their hypothesis that individual differences in cognitive activity (production or use of elaboration) are related to proficiency in learning. Slaughter presents evidence relating dependency–independency to school backwardness and offers several well-formulated proposals for the revision of school programmes to better serve the needs of children.

The remaining papers are variously inadequate. Gillis is on the side of the angels in his advocacy of ecological relevance of research, but his exposition of Brunswik's concepts lacks sufficient clarity to be useful, despite the illustration by his own interesting study of thought disorder. Gyr and Willey intend to compare favourably constructive theory of cognitive process with a behaviouristic approach, but their style and use of terminology render their work obscure, except possibly to those already familiar with Piagetian concepts and methodology. Similarly, Fowler's case for the application of the concept of 'competence' in place of 'intelligence' for consideration of cognitive deficit lacks sufficient clarity of formulation to do justice to the idea. Critchley simply offers a medical-style description of 'dyslexic' children with no evidence for his generalizations and no references to any reports which do. Three papers deal with additional aspects of cognitive style: Witkin *et al.* differentiate blind from sighted children on measures of the global–articulated dimension; Barsch proposes that mismatches between individual pupil and school-dominant 'mode of information processing preference hierarchies' determine many learning problems; and Hertzig attempts to trace the development of class and ethnic group related deficiency in task-orientated verbalization with its consequent deficit in school performance. The last three papers report small studies by Sapir of a deficit-centred training programme, by Weiner of children with discrepantly low WISC Verbal IQs, and by Ball *et al.* of the use of physiological reaction measures to assess sensory functioning in persons with neurological impairment.

On the whole the book is disappointing. The several valuable papers do not really compensate for the low information value and awkward prose of the remainder. The work lacks integration attainable by organization and editorial introduction of contributions. Clearly the papers were not all written to the same brief. The result is an uneven collection which fails to provide an adequate, comprehensive review of the area of cognitive deficit. Nor is it a stimulating sample of illustrative research.

DAVID S. HART

The Cultural Context of Learning and Thinking: an Exploration in Experimental Anthropology. By MICHEL COLE, JOHN GAY, JOSEPH A. GLICK and DONALD W. SHARP. London: Methuen. 1972. Pp. xx + 304. £3.50.

Comparative studies of subjects drawn from groups having but little ethnic or cultural contiguity are not new in psychology, and can be found masquerading under such labels as studies of *racial* differences and *cross-cultural* comparisons. The former term is now out of vogue and has been entirely displaced by the latter, although neither of them would bear a close scrutiny. Cole *et al.* subtitle their book: 'An exploration in experimental anthroplogy' – a happy but, one fears, a belated choice, since not only do we now have a *Journal of Cross-cultural Psychology* but also an Association of that name is about to be formed. The whole area of studies, by whatever name it be called, is attracting increased attention, probably not unconnected with the growth of teaching and applications of psychology in the countries in which hitherto the subject was unknown and the consequent need for comparative studies. This makes this book a very timely piece.

There are various approaches to such studies. A narrowly defined problem can be pursued in a multitude of groups of subjects whose background is investigated only in so far as it is thought to affect the investigated behaviour. This technique was used, in the now classical study of geometric illusions, by Segall, Campbell and Herskovits. Alternatively the net can be cast less widely but the groups concerned investigated in greater depth. The latter mode is used in this work, which concerns itself primarily with learning and thought and is an outcome of a collaboration of several disciplines. Such interdisciplinary enterprises have a notorious tendency of failing to satisfy any of the disciplines involved; when successful, however, they provide an ideal forum for interdisciplinary communication and a goal for further work. Cole's book has, in my view, succeeded.

An introductory chapter reviewing the relationship between culture and thinking is followed by a brief chapter on ethnology of Kpelle (Guerze), a tribe inhabiting parts of Liberia and Guinea. The Liberian Kpelle and the American cultures provide samples whose behaviour is investigated and contrasted in the course of the studies reported in the further chapters dealing with classification, learning, memory and thinking. One is struck by the variety of methods employed in this investigation and the persistent attempts to adapt these to the specific environmental conditions whilst at the same time retaining scientific vigour. In investigating language and classification, for example, the methods range from an adaptation of Metzger and Williams' technique to construct a seŋ chart, to sentence substitution, free association, and sorting of objects, the adaptations of various 'western' techniques being such as to permit comparisons between Kpelle samples drawn from various subpopulations differing in the extent of westernization.

The findings are lucidly presented, though some may find it irritating that part of the methodological information has been placed in the appendixes, and the manner in which the results are quoted in the text may occasionally irk more pedantic readers. If, however, these devices are seen as necessary to make the book acceptable to the interdisciplinary readership they are, in the view of the present reviewer, justified.

In the final chapter of the book the authors present a résumé of findings on a broad background of relevant published work ranging from speculative theories of anthropology (Lévi-Strauss) to the observations of other cross-cultural workers. Here too the views are stimulating if not always generally acceptable, as in the veiled suggestion that because one would not think of instructing one's testees in a language which they do not understand one should not measure their performance on performance tests derived from a culture with which they are not familiar. If the response to an intelligence test is an indicator of cultural familiarity as well as of the underlying mental process one cannot accept that an adaptation of a test which by use of more familiar materials leads to an improved performance is of necessity yielding a better measure of the process. Such a score may, however, be better for a particular purpose, such as evaluation of a person's performance in a clinical setting (for, presumably, a person who does badly relative to other testees from the same culture on materials which are familiar in his culture must be regarded as deficient in his functioning), but not for selection for training on tasks derived from another culture. For this purpose a test developed in the culture in which the training will take place probably offers a superior alternative. On such a test some would score high because of their

familiarity with an alien culture, some because of their superior ability independent of such, and some because of the fortuitous combination of these two factors. If it can be shown that such familiarity cannot be easily acquired (as suggested, for example, by the observations of W. Hudson), then the test is surely a meaningful tool.

The authors do not deal *in extenso* with some of the possibly essentially perceptual problems which are encountered in some cultures when alien tests are administered (e.g. Shapiro's finding that apparently normal unwesternized subjects tested in Central Africa rotated their reproductions of Kohs blocks by amounts unencountered in Western normal subjects). It's a pity. One would like to see what light such a productive and ingenious team could throw on those issues.

It is to be hoped that the book will find its way not only into the hands of anthropologists and social psychologists but also, and above all, into the hands of experimentalists and convince them of the fertility of the cross-cultural field.

J. B. DEREGOWSKI

Vigilance: the Problem of Sustained Attention. By C. M. STROH. Oxford: Pergamon. 1971. Pp. vi + 106. £2.95.

At the risk of sounding sour and blimpish, I must begin with the conclusion that this is really a bad book. Three years ago there was clearly some need for a review of the vigilance literature. Two very competent surveys have now been published by Mackworth (two volumes, 1969 and 1970) and Davies and Tune (1970). One is therefore obliged to ask why this volume by Stroh was thought to be necessary. (One reason may be that he has never heard of these books; he never refers to them.) This particular series of monographs has produced some well-known (and, I suspect, successful) books. The two volumes by Lynn on the orientation reaction and personality and national character exemplify the two types of monograph in the series: the one is a lengthy review and evaluation of material not previously presented in one volume; the other gives an extensive account of the author's own research. Stroh's volume is neither of these, or at best is an unhappy attempt to provide a superficial combination of both. In 90 pages of text he skims through his own and previous work in a manner which leaves us bewildered and confused. This is a great shame, because at times Stroh has some very sensible things to say, and possibly some interesting data of his own to present. One therefore wishes that someone had sat over him and goaded him into ensuring a fair hearing for himself. The book looks very much like a telescoped Ph.D. thesis. There are six chapters: (I) Introduction (4 pages), (II) Factors influencing vigilance performance (38), (III) Physiological correlates (10), (IV) Theories (15), (V) Related theories and phenomena (8), and (VI) Conclusions (12). Chapter II is possibly the best part of the book, since the author attempts to reconcile discrepant findings in the literature. The condensed manner of presentation makes it difficult to read. It is only fair to point out that since there are *many* such discrepancies, anyone reviewing the field will experience difficulty. In examining each of the variables which have been demonstrated to have positive, negative or no effects upon performance, Stroh tosses us rapidly back and forth, first favouring one view and then another. Each section ends with an overall appraisal of the effects of the variable in question, but it is rarely easy to discover why or how he reaches the conclusion he does reach. Frequently, the conclusion is prefaced by a brief report of his own work. But it is not made clear why he did it, how he did it, what exactly he achieved, and how it bears upon the earlier work. This is regrettable, because there is an implication that his own work resolves certain issues and makes sense of earlier evidence. A trick which Stroh uses a lot is to claim that a task which produced the 'wrong' results wasn't *really* a vigilance task after all (particularly where he examines tasks of short duration or those having an exceptionally high signal frequency). But if they aren't tasks involving sustained attention, what are they? And why can't the various theories of vigilance cope with their intransigent data? Unfortunately, when later he moves on to personality variables (extraversion, psychoticism) or to physiological correlates, all his insistence upon rigorous definition and precise criteria for vigilance tasks is thrown to the wind, and tasks which would not be classified as 'true' tasks in Chapter II are reborn as vigilance tasks once again. The study of physiological correlates of vigilance has led to considerable effort and to considerable disappointment, with a very poor significant:non-significant results ratio. Stroh fails to get to grips with the difficulties involved in such work, although he himself has clearly experimented with physiological measures. For example, he says, quite rightly, that studies employing

pulse rate have been particularly non-productive, but he makes no attempt to explain why this is so, in spite of the fact that problems of pulse rate measurement have been worried to death in the literature. Again, he says some very funny things about the EEG: '. . . with the eyes closed, an increase in alpha usually indicates an increase in arousal; on the other hand, with the eyes opened, an increase in alpha usually indicates a decrease in arousal'! (p. 44). This peculiar and false assertion, for which no evidence is provided, is then taken as a basis for an elaborate explanation of some non-significant results. It is Elisabeth Groll who apparently has found the philosopher's stone. She alone is the person to obtain those results which everyone else would love to obtain. Groll has been able to distinguish omissions and detections in terms of EEG and EDA responses and also relates some aspects of the EEG to reaction time. Since this work is published in German and is barely touched upon by Davies and Tune and neglected entirely by Mackworth, a much more detailed account of Groll's procedures and findings would be appreciated by the reader. (I presume she would forgive Stroh the sex-change she undergoes for this book.) Why can't we have some explanation of why *her* results should be superior to those of other workers? Such a discussion is important, particularly because arousal theory interpretations of vigilance performance are given such an airing and because, presumably, physiological measures provide the best operational measure of arousal theory. A reviewer surely has some obligation to explain why and where other people went wrong. Chapter IV provides a very brief run-down on theories. It ends with the two most disappointing and frustrating pages in the book. Stroh presents his own eclectic theory in the form of a complicated diagram, with a mere two pages of explanatory text. He just doesn't give himself a chance; presumably the reader has to work it all out for himself. The final chapter is the most irritating I have ever come across. We are presented with a list of 30 numbered findings derived presumably from an over-ambitious Ph.D. project. *Then the book ends*. There is no evaluation of the findings whatsoever. And results are presented in a very strange way; for example, Item 10 reads: 'Intercorrelations of personality variables revealed eighteen statistically significant correlations'. So what? How many correlations were there in all? What exactly were the correlations between? What was the direction and magnitude of the findings? Your guess is as good as mine. Even where he does say what he obtained, there is no attempt to relate the findings to earlier work. He calls this approach a 'New Approach'. This long catalogue of unrelated and unrelatable data is alleged to demonstrate the advantages of the 'multifactorial approach'. In other words, chuck in willy-nilly every dependent and independent variable which has ever been shown to have some bearing upon a vigilance task, present your findings (or some proportion of them) and leave the reader in the air. This mode of presentation is just bad manners. The final words of the book are: 'Now is the time for vigilance researchers to change from being exuberant explorers and trail blazers to being plodding, meticulous, and sceptical scientists'. If this volume is an example of the product of meticulous and sceptical science, I know what sort of psychologist I would like to be. This, then, is the sum total of Stroh's contribution: (*a*) a few simple-minded criticisms of some rather tired-looking data, (*b*) a diagram representing his own theory (without any explanation worthy of the name), (*c*) an indigestible flood of findings (without any explanation at all), and finally (*d*) a fervent recommendation that future work should follow his exemplary model! It is, I suppose, an abuse of privilege to be so harsh about a book when asked to review it. Yet one has some responsibility, surely, to the potential reader or purchaser? I just simply cannot recommend it. But it is a slim volume and I am willing to eat it (rather than my hat) if any other reviewer does recommend it.

ANTHONY GALE

Mechanisms of Animal Discrimination Learning. By N. S. SUTHERLAND and N. J. MACKINTOSH. New York: Academic Press. 1971. Pp. xi + 559. $18.50.

This book provides – in the authors' own words – 'a relatively exhaustive review of the field of animal discrimination learning, with excursions into other areas, such as generalization, partial reinforcement and some aspects of comparative psychology'. The review itself is thorough and non-doctrinaire; and the framework for discussion is that of the authors' own view of the nature of discrimination learning and related phenomena. It is contended that the concept of selective attention is necessary for the understanding of learning in animals. More specifically, discrimination learning is said to involve two processes, namely (*a*) 'learning to which aspects of the

stimulus to attend' and (*b*) 'learning what responses to attach to the relevant aspects of the stimulus situation'. The actual theory of selective attention, which is presented in the form of a model, emerges in the first place from the consideration of experimental data obtained in studies of rats solving discrimination problems in a modified Lashley jumping stand. The essential features of the model are summarized under the heading 'Rules for the Operation of the Model'; these are set out in Chapter 2 and also, for the reader's convenience, on the inside front cover of the book.

It turns out that the two-process theory in its present form sheds fresh light on – among other things – the so-called continuity–non-continuity dispute among students of learning; it also copes well with the findings of experiments in which learning occurs with more than one relevant cue; and the theory accounts satisfactorily for the results obtained in various types of experiment involving a transfer of learning. Studies concerned with the so-called overtraining reversal effect are analysed in detail, and the authors' two-process theory is shown to make good sense of the known facts relating to reversal learning. Many other learning phenomena are examined in the light of the theory, and a variety of other approaches to the understanding of certain kinds of animal learning experiments – approaches such as the frustration theory and the stimulus after-effects theory – are discussed. It is argued that these other theoretical formulations are compatible with the authors' own approach.

Chapter 12, entitled 'Some Comparative Psychology', is of particular interest to this reviewer. The authors are, of course, fully aware that many animal learning phenomena are not susceptible of explanation in terms of the two-stage model of selective attention. Furthermore, the authors recognize that their model runs into some difficulties when it is required to explain the learning of animals other than the rat. It would appear, however, that when certain changes are made to the parameters of the model, then it can cope with learning data obtained from many different animal species. The authors are comparative psychologists in that they are interested not only in the discrimination learning of the rat but are also much concerned with inter-species similarities and differences in the mechanisms of learning. Their approach to the study of behaviour and that of the ethologists are, of course, very different; but, as the authors apparently see it, and as the reviewer certainly does, these two approaches to research are complementary rather than conflicting. This is an impressive book in the thoroughness with which it deals with its subject-matter. It is also an important book in that it provides not only an excellent review of experimental work in many areas of animal learning but also one which is presented within a viable theoretical framework.

W. SLUCKIN

Non-verbal Communication. Edited by ROBERT A. HINDE. London: Cambridge University Press. 1972. Pp. xi +443. £5.50.

This book is the result of the deliberations of a Royal Society study group set up under the chairmanship of W. H. Thorpe to examine non-verbal communication in animals and man. It forms a natural successor to the symposium on 'Ritualization of Behaviour in Man and Animals' published previously under the same auspices. The proceedings of the current symposium have been edited by Robert Hinde, who also acted as a focus for an interchange with the contributors.

It should be said at the outset that this is an important work. The authors are all distinguished in their field, and each makes some substantial point or successfully illuminates some new facet of the topic under review.

And what of the topic? Although 'non-verbal communication' is a rag-bag term and although this book includes contributions from widely different disciplines, something substantial has been presented. In fact, a Leviathan emerges from the pages which had not until this point assumed a truly tangible form except to the specialist and the expert. There are antecedents of course. Sebeok's major work on animal communication is one of them, but this book not only stands as a symbol of the final emergence of the topic but also traces through the delicate strands of argument about definitions, functional significance, evolutionary origins and the relationship of non-verbal to verbal behaviour. In respect of the contributors Thorpe presents the latest work on the communication patterns of birds and other animals and also provides a chapter which challenges the notion of the uniqueness of language in man. He points out that many of the design features of human language are present in systems of animal communication.

The difficulties of an evolutionary approach are illustrated in the chapter by van Hooff, who claims that human laughter has its origins in the bared-teeth display of primates, but we may doubt whether evidence of the kind presented will ever be sufficiently strong to support the case without equivocation.

This same difficulty is evident in Eibl-Eibesfeldt's chapter. We are given a mixture of genuine insight and quaint improbable hypothesis, and this chapter suffers from the fact that the material has been well publicized elsewhere.

The psychologist will of course be familiar with the work on eye-contact and gaze described by Michael Argyle. He may be less familiar with recent studies of non-verbal communication in children described by Blurton Jones, and in the mentally ill by Grant.

Edmund Leach, in a chapter which deserves serious consideration, is critical of current notions of the ethology of man. He touches a sensitive nerve and this chapter will no doubt spark off much debate.

The whole topic is set in an ornamental surround by a chapter on communication in the theatre written by Jonathan Miller and one on action and expression in western art by E. H. Gombrich.

The editor's comments are lucid and well argued. Reason is on his side. In so far as there is any criticism, it is that the summing up in the short final epilogue is too pessimistic. It is stated, for example, 'It is both fashionable and satisfying to the inquiring mind to establish unifying principles for an area of study, but it will be clear that within this area we are far from doing so'. In that Hinde fails to point out the positive achievements of the book he is not completely fair. There is much disagreement certainly amongst the contributors but this book has an internal strength. It conveys the feeling that a powerful discipline is here being forged and that something of real importance has been achieved.

STUART J. DIMOND

Early Learning and Early Experience. Edited by W. SLUCKIN. Harmondsworth: Penguin Books. 1971. Pp. 416. 70p.

Professor Sluckin has organized 28 readings into seven sections, working from early learning through to early experience. Of particular value are the general introduction and list of additional reading, which extend the scope of some sections and provide recent references. The sections, which are considered in turn, start with one on early conditioning. The four readings all report the successful application of a technique which had been thought not to work at this age, but fail to answer the question of whether conditioning plays an important role in early learning. Alternatives are provided in the next two sections. Part 2 consists of four papers on what the editor calls 'classical imprinting'. Within this constraint they form a useful and coherent selection, while the scope is readily extended by use of the eight further references in the general introduction. Part 3, on learning and development, is the most theoretically orientated section, leading from a classic piece of Hebb, via two detailed comparative studies of the development of learning, to a Caldwell article which, by challenging some popular assumptions about the optimal learning environment for the young child, tends to the conclusion that human infants should spend a considerable amount of time in institutions being cared for by well-trained professionals. At the very least this section should provide the basis for some lively discussions. Part 4, on restriction and enrichment of early experience, is less satisfactory. The first reading suggests a discrepancy between human and animal findings, but the enormous body of human enrichment studies of the past 15 years receives no mention. The selection of two of the four papers may also be questioned on the grounds that they are concerned with the series of experiments showing that rats subjected to almost any kind of increased stimulation in infancy show improvement in a variety of adult functions, including learning, viability and reduced emotionality. On the basis of a very elegant series of experiments, Schaeffer (in Newton and Levine, *Early Experience and Behaviour*, 1968) has claimed that the effects can all be accounted for by the decrement in emotional response, while the common factor in all of the treatments was a lowered body temperature. When temperature is maintained, the effects disappear. If valid, these conclusions reduce an enormous body of work and speculation (such as Readings 15 and 16 here) to merely historical importance. The paper may have appeared too late to be included, but such simplifications are rare enough in psychology, and it is a pity that it was not mentioned in the introduction. Part 5, the development of special traits, consists of four readings representative of the animal work on

early, non-social factors, with the Freudian influence mentioned by the editor being shown by a concentration on oral stimulation. Part 6, parental deprivation in infancy, is really a specific aspect of the previous topic. Two excellent accounts of rhesus monkey experiments are followed by a tantalizingly brief excerpt from Yarrow's (1961) review. This is one of the shortest sections, and could perhaps have been usefully extended by the inclusion of a human study. The title would also seem to justify some mention of the father, but here it would have been much more difficult to find a suitable publication. The final section, on early socialization in animals and man, provides a strong finish with five readings covering a wide range of species and theoretical levels.

In a book of readings theoretical integrations and evaluations can be provided either by elaborate linking comments by the editor or, where available, by the inclusion of appropriate review articles. Neither of these tactics has been used to any great extent in the present work, the theoretical contributions generally being of earlier vintage than the empirical studies. The readings provide excellent coverage of a wide range of species, and it is perhaps merely prejudice to wish that there had been more empirical studies of humans than of dogs, rats, chickens and monkeys. The overall effect is of a lot of valuable basic material in a form which cuts across traditional categorizations within psychology, while requiring that the reader should draw out the implications for himself. This (and the price) make it an excellent basis for individual undergraduate work.

PETER STRATTON

The Ontogeny of Vertebrate Behaviour. Edited by HOWARD MOLTZ. New York and London: Academic Press. 1971. Pp. 500. $22.50.

Two eminent American students of animal behaviour, D. S. Lehrman and J. S. Rosenblatt, set the tone of the book in the first chapter, which is entitled 'The Study of Behavioural Development'. That chapter consists of both a restatement of the authors' approach to the study of the ontogeny of behaviour and a survey of selected research. The former expresses a sentiment against 'dichotomous formulations of experience against structure' as well as one for 'respect for diversity in nature and in the activities of our fellow scientists'. The review comprises studies concerned with the role of experience in the development of basic sensorimotor functions and emotion/motivation, and also studies pertaining to social development which includes species recognition, individual recognition and mother–young synchrony.

The next chapter, by M. Hershenson, deals mainly with the development of visual abilities in human infants. Thereafter the book is primarily, but not solely, devoted to animal behaviour. Thus B. Meyers surveys the effects of restricted and complex early environments on later problem-solving abilities of animals. The ontogeny of emotional behaviour is dealt with at length in an exhaustive manner by D. K. Candland. A chapter by W. I. Welker is devoted to the development of exploratory behaviour and play; it concerns itself in the main with problems of definition and conceptual framework for research and provides an encyclopaedic bibliography. The ontogenetic origins of sexuality and of maternal behaviour are considered respectively by R. E. Whalen and by Moltz himself, the editor of the volume.

Mother–infant relations in macaque monkeys are described and discussed by L. A. Rosenblum. There is an admirable chapter by P. P. G. Bateson which both surveys the present-day factual knowledge of imprinting and soberly considers the nature of the process. Vocal learning in birds is treated exhaustively in an excellent chapter by Marler and Mundinger. The final chapter takes us back to human beings; in it J. Church deals in a trenchant manner with theories of language development and with ways of studying the ontogeny of language. All in all, this is a really worth-while book. Students and research workers in the field of developmental psychology will find it very useful.

W. SLUCKIN

Minnesota Symposia on Child Psychology. Volume 5. Edited by JOHN P. HILL. Minneapolis: University of Minnesota Press. (London: Oxford University Press). 1971. Pp. xi +216. £2.40.

Based on the 1970 Minnesota Symposium on Child Psychology, this is the fifth volume in a series which began life in 1966, and it marks the end of John P. Hill's five-year term of office as organizer and editor of the symposia. The policy of the Minnesota Symposia has been to represent the diversity of 'good programmatic research' by inviting contributions from investigators at that point in their programmes where an integrative review of their findings would be of value to themselves and to others. Six reviews of this type are included in the present volume.

In the first contribution Goodnow discusses modality-specific views of information gathering in the light of the proposition that the effectiveness of an exploratory method is a function of the way it samples the stimulus complex. Criticizing the modality-specific view because of its tendency to lead to the conception of a hierarchy of modalities – usually with vision as the dominant modality – and its tendency to carve up the literature on the various ways of gathering information, she offers an alternative non-modality-specific, stimulus-sampling framework which, she claims, is more flexible and frees experimenters from restricting preconceptions. Empirical studies leading to the development of this alternative approach are described.

The second paper, by Hilgard, offers evidence in support of the thesis that certain dissociative, childlike qualities rather than regressive, childish qualities are preserved in the hypnotizable adult. Childlikeness is interpreted as 'the qualities of childhood which, when they continue into adult life, are highly regarded', and childlike qualities such as imagery, aesthetic interest in nature, and creative expression are all shown to be related to hypnotic susceptibility in young adults. Developmental evidence is provided to show that susceptibility to hypnosis rises during childhood, peaks at adolescence, and then declines in later years. The decline is related to an increased reality orientation but it is hypothesized that when developmental circumstances are appropriate, childlike qualities and heightened hypnotic susceptibility can be retained into adulthood. The paper also reports on some of Hilgard's earlier findings on the development of hypnotizability.

In the third contribution, by Hunt and Bayley, recent investigations employing the newly released revision of the Bayley Scales of Infant Development are reviewed with the aim of establishing their predictive value. Preliminary reports are given of two different analyses of the scales, both of which are 'attempts to pinpoint specific items in the test which illustrate differences in cognitive development in infancy and to relate these to later intellectual status'. Examples of past and ongoing research are described which indicate that this item-analysis approach has considerable promise, though the need for more definitive evidence from longitudinal studies is stressed. It is also pointed out that adult mental abilities are 'the result of complex, interacting variables of traits and experiences' and that the Bayley Scales may be of use as a predictor of change rather than constancy, permitting 'assessment of the relative contributions of a number of influences on the individual during his life span'.

The fourth paper, by Patterson and Cobb, provides a dyadic analysis of aggressive behaviour. The rate of aggressive behaviour over time in intrafamilial dyads is related to preceding and subsequent behaviour of both the aggressor and the victim. A series of laboratory, observational and field studies are described which, taken together, 'suggest that in a few highly selected situations, the social environment is programmed to provide positive social reinforcers for certain types of aggressive behaviours'. The data reported are interpreted as providing support for a coercion theory of aggression.

The fifth contribution, by Schaefer, discusses the development of hierarchical, configurational, conceptual models of parent behaviour and child behaviour. In this paper Schaefer reviews 'methodology for analysis and synthesis, or differentiation and integration, of behaviour concepts' in the hope that it will lead to 'more effective and efficient analyses of psychological domains'. Factor analytic evidence in support of a two-dimensional, circular model for parent and child behaviour is discussed, together with the development of a three-dimensional, spherical model. The paper concludes with a discussion of further applications of the model to other

dyadic relationships and suggests that it could be particularly valuable in reducing 'the ambiguity of personality language'.

The final paper, by Wapner, Cirillo and Baker, is an extensive review of empirical findings on object localization, egocentric localization (apparent body position) and autokinetic motion. The findings are generated from an organismic approach as represented in Werner and Wapner's sensory-tonic field theory 'in which perception is characterized as a reflexion of equilibratory relations between two poles of the organism–environment system', and from a developmental approach as represented in the orthogenetic principle 'specified in terms of comparison between systems differing in the degree of distance between the organism and its environment'. The authors, with some justification, state that their organismic–developmental approach has been heuristically fruitful, stressing in particular 'the involvement of the organism as a total system in the formation of the perceptual world', but they question the value of some of the explanatory models that have been formulated using the approach and point to ways in which the power of the theory could be increased.

This book would be a useful addition to the library. The papers it contains are of undoubted value as up-to-date statements of the authors' empirical and theoretical positions at the time of publication. However, the highly specialized and often technical nature of the contributions would make them compulsive reading only for those planning research in closely allied areas.

ROBIN PRIESTNALL

The Nature of Adolescent Judgment. By E. A. PEEL. London: Staples Press. 1971. Pp. 184. £2.25.

The theory of intellectual development in the secondary-school age range is dominated by Inhelder and Piaget's *The Growth of Logical Thinking from Childhood to Adolescence*, which book is itself preoccupied with experimentation in physics. We cannot for long be satisfied with the theory as long as it remains so confined, and (in this country at least) we turn principally to Professor Peel and his students for their work in extrapolating or expanding Piagetian ideas into the fresh but murky woods of arts and social sciences. This work has been impossible to appreciate satisfactorily in the forms in which it has so far been published, and any further publication is therefore likely to be welcomed by anyone who has even a slight inclination to admire it.

Professor Peel begins with the (optimistic?) assertion that adolescents and young adults are driven to reconcile the inconsistencies which they apprehend between the actual and the possible. His purpose in the present book is to examine the intellectual processes in the capacity to evoke possibilities and set them alongside actualities; also to report fresh empirical results.

Examples of the grading of judgements are presented. Testees are given a passage of written material and are asked questions to which they have to make written responses. Three basic grades of response are suggested: (i) 'restricted': tautological, premise-denying or irrelevant; (ii) 'circumstantial': bound by the content of the given material; (iii) 'imaginative–comprehensive': involving the invocation of independent ideas. In a test concerning the erosion of soil, it seems the pupil cannot get the highest rating unless he has in pre-test experience become aware of the effect of trees against erosion; there is an important implication here that rated maturity of judgement may be dependent upon a pupil's specific experience.

Level of judgement is shown to be related to age and to mental age. A correlation (p. 49) between social class and level of judgement is reported but this is based on an inflated N (more than one score per pupil); it is therefore doubtful whether the correlation is genuinely significant as it is later (p. 113) claimed to be. However, I may mention that D. H. Cherrington (unpublished M.Ed. thesis at Leicester University) obtained a significant correlation of 0·23 between social class and Peel-type score. Peel presents interesting data on the effect of varying the form of the test; here again, inflated N's are to be noted. How should we interpret the finding (p. 59) that pupils recognize higher-level judgements even when they cannot produce them? If it is easier to make judgements about other judgements than to produce the other judgements themselves, where does this put the Piagetian idea that second-order propositions are harder than first-order ones? Also, is the notion of 'levels' of judgement really justified, implying a scale, or are there a number of correlated but distinct dimensions being tested?

Chapter 4 presents a view of the process of making a judgement, involving curiosity, cognitive conflict, equilibration as conflict reduction, and the formulation and selective elimination of hypotheses. The earlier emphasis on independent ideas is obscured in this analysis. Chapter 5 is rather metaphysical. 'The balance of nature is in dynamic equilibrium . . . Historical change is seen as a succession of causal shifts between states of more or less stable balance between the human forces operating over the period of change . . . Awareness of the relation between such equilibria and changes is the mark of historical insight' (pp. 96–7). I wonder: are these statements testable hypotheses about the world, or about how pupils come to think of the world, or about how Peel wishes pupils to come to think of the world?

Peel's stress on hypothetico-deductive thinking in history, geography, etc., is easy for a psychologist to accept. But my impression is that many teachers of history reject this type of thinking. And how far is the hypothetico-deductive model applicable to literary judgement? Where it is applicable, we can feel we understand the nature of the skills which the pupil is to acquire; but where the required judgements appear to be more intuitive, what then? We may agree with Peel's assertion of the applicability of hypothetico-deductive thinking to arts subjects, without necessarily asserting the universal applicability of this type of thinking through all arts. Peel himself does not (so far as I know) assert the universal applicability, or deny it.

A chapter reports research on the promotion of mature judgement; another chapter, on concept formation, includes a distinction between generalization and abstraction. Both these chapters are valuable but too brief. As a whole, the book is unpretentious, and takes the psychology of intellectual development farther along in important directions.

W. S. ANTHONY

Ethological Studies of Child Behaviour. Edited by N. BLURTON JONES. London: Cambridge University Press. 1972. Pp. x + 400. £7.00.

The usefulness of an ethological approach to human behaviour is often judged by the popular accounts which have appeared in recent years, mainly notable more for the uninhibited use of theories derived from the observation of animals than for careful studies of human behaviour. Blurton Jones in his editorial introduction is rightly critical of these accounts, and attempts to restore our confidence by emphasizing the vast amount of detailed observation which is needed before comparative studies can usefully be made.

To a cautious scientist those over-facile generalizations which have been made in the name of ethology must be an embarrassing precedent, and both Blurton Jones and others in this book are consequently perhaps too defensive on behalf of ethology, and correspondingly over-critical of what they term traditional approaches to the investigation of human behaviour. Some of the criticisms are misleading; it is not true, for example, that all observational studies before ethology was invented used complex categories of behaviour. Some earlier studies could be more reasonably criticized for attempting to answer questions which could not be answered by direct observation. A large proportion of the many observational studies in nursery schools which were carried out in America in the 1930s consisted of attempts to study individual differences in personality. As Goodenough observed in a sad review in 1949, these attempts foundered on the difficulties of sampling representative behaviour, and defining the relationships between presumed personal traits, observed behaviour and the environment in which the behaviour was observed. In contrast, the aims of the studies reported in this book are frankly descriptive. Ethologists, it seems, scorn hypotheses. Description, it is argued, should precede speculation, and observational data will itself generate theories to be tested. The weakness of this approach is that the act of observation carries with it implicit hypotheses. For example, the concentration on movement, gesture and facial expression appears to imply that non-verbal communication is more important than verbal communication. The hypothesis remains to be tested.

Similarly, the restricted situations in which the subjects are observed and the restricted sampling of behaviour imply the hypothesis that behaviour is independent of the broad structure of its environment. This assumption is most grave when the environment in which observations are made is very unusual. For example, a play-group which is the context of three studies admitted 70 per cent of the children before the age of three years and over 50 per cent before 30 months. Preschool experience of organized groups is still a minority experience for children in most Western cultures, and rare at so early an age. The limitations which observations in

unusual environments impose would not be so important if the environment were better reported, but in only one of the studies of children in preschool groups has a reasonable attempt been made to described the playroom, its facilities and the behaviour of the adults present. Such omissions make it possible that conclusions drawn from these studies may be excessively ephemeral and culture-bound. Of course, this limitation is shared by much work conducted under a nominal allegiance to psychology, but it is particularly important for ethologists because, in using phylogenetic explanations to interpret behaviour, they are necessarily implying that the behaviour may be found throughout the population sharing a common gene pool, which probably extends far beyond cultural boundaries. Unhappily, describing the environment is even more difficult than describing behaviour if one sticks to the 'micro-categorization' which is the ethologist's recommended technique. It seems more reasonable to bring together information collected at a variety of levels and with a variety of methods, as Richards and Bernal have in their study of mother–infant interaction (chapter 7).

Perhaps the aggressive posture on methodology so frequently adopted in this book provokes an aggressive response too easily. Despite these reservations, there is much that is both useful and challenging to any student of child behaviour. Hopefully, the next phase of research will be a collaboration rather than a confrontation of psychology and ethology, and there is evidence enough in the studies described to prompt a careful reappraisal of much work that is by now near to being accepted as dogma by psychologists.

The book is excessively expensive, the more so because there are few illustrations and, surprisingly, no photographs.

A. J. COSTELLO

Biofeedback and Self-Control 1970: An Aldine Annual on the Regulation of Bodily Processes and Consciousness. Edited by THEODORE BARBER, LEO V. DICARA, JOE KAMIYA, NEAL E. MILLER, DAVID SHAPIRO and JOHANN STOYVA. Chicago: Aldine-Atherton. 1971. Pp. xxi +546. $12.00.

The editors of this volume have selected a number of 1970 papers which cannot be said to fall easily under any one rubric. Rather, they bear a family relationship to each other. Their most salient feature is the use of the technique of biofeedback in gaining voluntary control over autonomic and electroencephalogram processes. Biofeedback is a much-used neologism. It is a term that is applied to techniques that supply a learner with knowledge of the results of his efforts to control specific bodily processes. Without such artificially supplied knowledge it is impossible to acquire voluntary control over heart rate, blood pressure, EEG patterns and other processes that we would otherwise receive no information about. With such knowledge, it is an open question whether humans and other animals can learn selectively to modify the states of their viscera and brains. Many of the studies included in this book indicate clearly that such control does occur with many kinds of autonomic and brain responses. This is not new, nor is it particularly surprising. What is both new and surprising is the evidence that such learning can be very highly specific and that control of autonomic responses may be exerted directly without mediation by the somatic motor system. Most of this work has been carried out on rats by Miller, DiCara and their co-workers, although Shapiro *et al.*, in a paper reported in the book, describe an experiment in which they succeeded in differentially conditioning heart rate and systolic blood pressure in humans. Miller developed the technique of conditioning the autonomic responses of rats by giving them brain-stimulation reward when they were massively curarized, precisely in order to demonstrate the possibility of instrumentally modifying such responses without any mediation from the skeletal system. His results are very impressive and their theoretical importance is great, although Obrist *et al.*, whose paper is included in the volume, do not accept that it has been demonstrated that the instrumental conditioning of heart rate is independent of somatic changes.

The papers are divided into seven sections and there is a helpful preface by Barber which summarizes the contents of the volume. Section 1 contains review material and provides a convenient introduction to the field of biofeedback research; for example, there is a paper by DiCara on learning in the autonomic nervous system. The next two sections cover the topics of hypnosis, meditation and psychedelic drugs. Both hypnosis and meditation have been of interest for a long

time because they seem to indicate the possibility of a much greater degree of control over bodily processes than most people think they possess. This suggests two questions. Firstly, is it possible to substantiate the claims made for hypnosis and meditation, and secondly, if it is, is the increased control demonstrated achieved in the same way as that acquired by biofeedback? Most of the papers are concerned with the first question and none deals with the second directly. However, there are a couple of papers, one by Spanos and Chaves, the other by Barber, which are critical of the widely held hypothesis that there is such a thing as the hypnotic state or trance. Many hypnotic phenomena can be observed without the preamble of a hypnotic induction procedure. This position is polemical and another paper by Engstrom *et al.* reports a successful attempt to increase hypnotic susceptibility by EEG alpha training using biofeedback.

The next three sections are the heart of the volume, comprising many experimental studies involving biofeedback with the conditioning of EEG, cardiovascular, electromyographic, electrodermal and orienting responses. It is probably these that the specialist will find most useful, although the final section, which is general, also contains some interesting material, including Kimble and Perlmuter's seminal article on the experimental approach to the problem of volition.

In order to complete the annual as soon as possible, all papers are reprinted exactly as they originally appeared, which leads to a gamut of print ranging in size from the Lilliputian to the Brobdingnagian. There is a name and a subject index and the origin of the articles is shown at the front of the book. This volume and its sequels should save both students and specialists a lot of time previously spent sniffing around library shelves.

ANDREW MAYES

Handbook of Psychotherapy and Behaviour Change: an Empirical Analysis. Edited by ALLEN E. BERGIN and SOL L. GARFIELD. New York and London: Wiley. 1971. Pp. xviii + 957. £8.50.

Until the late 1950s the knowledge base of psychotherapy was drawn largely from clinical experience, 'authoritative' sources and various theoretical schools. Since then a strong movement has developed towards the empirical study of psychotherapy and its integration within a general psychology of behaviour change. This book is offered as a review and appraisal of the outcome of the movement so far.

Urban and Ford open the first part by noting the considerable broadening which has occurred in the scope, methods, theories and practitioners of psychotherapy, and they adopt a problem-solving framework in an attempt to encompass this heterogeneity and to evolve a possible synthesis. Their original and interesting overview provides a framework for those concentrating on particular aspects and developments in the field to place them in a wider context. It is followed by useful chapters on experimental design, psychophysiological methods, laboratory interview analogues, social psychological approaches and clinical innovation in psychotherapy research and practice. This last contribution by Lazarus and Davison offers a corrective to the over-exclusive reliance on laboratory research as a source of therapeutic advance and stresses the heuristic value of clinical experience.

The hare started by Eysenck's 1952 review of the outcome of psychotherapy is pursued with increasing sophistication by Bergin, who concludes provocatively that while there are efficacious influences in traditional therapy, their average effects are weak and produced by a minority of practitioners, whose methods therefore should be a focus for research as well as emulation by their less successful colleagues. There follow two chapters on client and patient variables respectively, the latter reviewing the exceptionally interesting work by Truax and Carkhuff on the therapist's empathy, warmth and genuineness as core conditions for the achievement of beneficial change in clients. Other contributions in this part of the book deal with content-analysis studies, quantitative research on psychoanalytic therapy, placebo effects, the treatment of children, and finally there is a review by May of the therapeutic use of drugs which many psychologists may find a valuable source of reference in a less familiar field.

The third part, on behaviour therapy, is likely to be the most familiar ground and represents what is probably the spearhead of the empirical movement in therapy. Leading exponents contribute chapters on the treatment procedures based upon the principles of counterconditioning (Eysenck and Beech), operant conditioning (Krasner) and modelling (Bandura), and there is a more general examination of the nature of learning in the traditional and behaviour therapies.

Another interesting and potentially very important development in psychotherapy is its extension from the consulting room to the natural environment of the client, often using non-professionals as therapeutic agents. The work of Patterson and his co-workers in Oregon exemplifies this development, and it is represented in the book by his chapter on changing the management practices of parents and teachers as an indirect means of modifying problem behaviour in children. This theme is also among those discussed by Roen in his contribution on the difficult task of evaluating large-scale community mental health programmes. Research on psychotherapeutic groups and on training groups in human relations is notoriously unsophisticated and a good deal of cohesion and rigour is introduced into these two areas by Bednar and Lawlis, and Gibb respectively. The book ends with reviews of research on educational and vocational counselling, and on the teaching and learning of psychotheraphy, the last including discussion of Truax and Carkhuff's promising methods of training lay and professional people as therapists in relatively short periods.

As with all handbooks of this size and number of authors, it is difficult to offer any overall evaluation, except to say that it is reasonably comprehensive, contains many well-selected and interesting contributions, and constitutes a valuable source of reference for researchers and practitioners.

The whole volume is an impressive testimonial to the feasibility and value of applying an empirical approach to the once almost mystical field of psychotherapy, and of integrating it within a general psychology of behaviour change.

DEREK JEHU

The Effects of Psychotherapy. By S. RACHMAN. Oxford: Pergamon Press. 1971. Pp. 186. £3.75 and £2.50.

Medicine, and psychiatry in particular, has a strong historical affinity with magic, and it is only in comparatively recent times that evidence for the efficacy of medical treatments has been sought. The practice of psychiatry is still, for the most part, based on articles of faith rather than on firm evidence of clinical worth, but the need to validate therapeutic procedures is becoming more and more widely recognized. This is as it should be. In view of the enormous social and economic costs of ineffective treatments the results of outcome research should be a central concern of every clinical practitioner.

Rachman has set out in his monograph critically to evaluate evidence bearing on the efficacy of insight-orientated psychotherapy. His review of the relevant literature is perhaps the most extensive yet available and provides a valuable source of reference. His sobering conclusion is no more encouraging than that reached by Eysenck in his pioneering 1952 critique and in two subsequent reviews – that at best the case for the efficacy of psychotherapy is not proven – and it is clear that in Rachman's view it is not likely to be proven. He also conducts a similar but briefer analysis of evidence bearing on the behaviour therapies (now treated extensively in a number of texts), and arrives at a considerably more optimistic conclusion, to the point where: 'The danger of a premature and uncritical celebration of this advance in clinical psychology appears to have passed'.

Rachman's treatment of his subject is cogent and scholarly. He laments the paucity of well-controlled psychotherapy outcome studies and reveals the inadequacies of the few studies that make positive claims. With Eysenck, he argues that in the absence of satisfactory controlled studies it is up to psychotherapists to demonstrate that their success rates exceed the rates of 'spontaneous remissions', and this they fail to do. Proposals by Bergin and others that the disappointing results of outcome studies may arise from an averaging of the treatment effects of good and bad therapists also fail to survive Rachman's scrutiny. Separate chapters devoted to psychoanalysis, psychotherapy with children, and psychotherapy with psychotics all point to the same grim conclusion. Only Rogerian therapy is accorded a slightly optimistic note. In contrast, the clinical effectiveness of behaviour therapies, particularly desensitization, is found to be well established.

There is no question that the emergence of the behaviour therapies in recent years has been greeted in some quarters with considerable hostility, and it is not surprising that a number of behaviour therapists have responded aggressively or defensively as preachers of a new faith. However, neither the cause of objectivity nor the interests of patients in distress are furthered by the

blows and counterblows of warring factions. The need for dispassionate inquiry is paramount. To Rachman's credit there is much evidence in his book of his determination to meet this need, and it would be gratifying to report that he had succeeded in his task. Sad to say, however, he has not – and perhaps it is too much to expect that one so committed to the behaviour therapy cause could succeed in such an undertaking. Indeed one of the dangers of his monograph for the inexperienced clinician or the undergraduate psychologist is that its evangelical zeal is concealed under a polished veneer of apparently rigorous objective inquiry. To the more experienced reader it will be apparent that the monograph is inspired by the conclusions it presents and the evidence is then neatly slotted into place. This is a pity, both because Rachman has many interesting and valuable observations to make and because his conclusions may well be correct. It may be that insight-orientated psychotherapists do little more than comfort patients while they are 'ill', whereas behaviour therapists have the resources to induce positive change, but if so it will take a less proselytizing assessor to make out a convincing case.

Rachman is at his best when his critical standards are high, and his demolition of claims for the efficacy of psychotherapy is thorough and for the most part incisive. Frequently, indeed, the critiques are so detailed that constant reference to the original studies under consideration is mandatory if they are to be seriously appraised. His greater tolerance for the shortcomings of studies that reflect negatively on psychotherapy does not really weaken his case. It is when he turns to the behaviour therapies that the lapsing of his critical standards becomes serious, and here the tone is set by his citation of Richard Crossman's assessment of behaviour therapy as a 'real, genuine scientific breakthrough'. What place a politician's assessment of therapeutic outcomes has in a scholarly work is unclear. To the reader who has earlier been persuaded by his stress on the importance of controlled outcome investigations using a no-treatment control group, it comes as a surprise to discover that Rachman can find only one such clinical study to support his claims for the efficacy of behaviour therapy – and even here there is no mention of the author's stress on the dispensation of reward in a desensitization procedure. Negative results in controlled clinical studies lacking a non-treatment group are explained away as attributable to factors such as 'severity of illness', and Rachman finds it appropriate to make such observations as 'the patients who were treated by behaviour therapy had improved slightly more (but not statistically so) than the other group'. Again, the earlier stress on spontaneous remission rates is forgotten, when, for example, he interprets Lazarus' claim of a 62 per cent improvement rate among severely neurotic patients as evidence that 'behaviour therapy is probably effective in treating complex neurotic disorders'. It comes as a surprise, too, to find that in a comprehensive listing of analogue studies of systematic desensitization it is precisely those studies that pose the most serious questions for the usual theoretical accounts of the desensitization process that are omitted. Examples that spring immediately to mind include the report by Leitenberg *et al.* (1969) that reinforcement and/or therapeutic instructions rather than graded hierarchy and relaxation appear to be the major determinants of behaviour change, and studies such as those of Valins and Ray (1967) and Marcia *et al.* (1969), which suggest that theoretically nonsensical procedures are as effective as systematic desensitization in producing behaviour change. Rachman in fact does not appear to entertain the possibility that what goes on in a patient's head may be critical in determining his response to therapy.

The lingering final impression of the monograph, despite its many good qualities, is that it is disappointing. It is difficult indeed to resist the suspicion, compounded by its many typographical errors, that it was rushed into print as a Maudsley rejoinder to Bergin's chapter on therapeutic outcomes in the *Handbook of Psychotherapy and Behaviour Change* and, while Bergin may have got his come-uppance, we are not a great deal better off than we were before. Not only do we need a treatment of the subject that is free of subtle and perhaps unwitting polemic and bias; it is also imperative that any treatment begins with a consideration of fundamental issues on which Rachman, save for a brief concluding remark, scarcely touches: the goals of therapy, the lack of agreement among outcome criteria, the unreliability of diagnosis, and so on. Hopefully Rachman's will not be the last word.

JOHN D. DAVIS

Behaviour Modification in Mental Retardation: the Education and Rehabilitation of the Mentally Retarded Adolescent and Adult. By WILLIAM I. GARDNER. Chicago and New York: Aldine–Atherton. 1971. Pp. vii +379. $11.75.

The application of behaviour-modification techniques to the mentally retarded has grown rapidly during the past decade, receiving considerable interest from all sections of the professions dealing with these individuals. The diversity of training and activity of these specialists raises at the outset the question of the audience at which Gardner's book is directed. His own target is 'the rehabilitation personnel involved in the evaluation, training and vocational adjustment of retarded clients and for those special educators who are involved in the secondary level school programme and the programme of transition from the school to the work rehabilitation setting; however, the psychologist, the psychiatrist, the social worker, the training and sheltered workshop supervisor ... etc.'. Doubt must be raised as to how far one book can adequately communicate to such a diverse set of people. For the non-psychologist the book appears to be heavily academic, namely, a section in the introductory chapter stringing 21 references together (pp. 22–3). For the interested psychologist there is little new regarding learning theory or behaviour modification that will not have been encountered in the literature, including Gardner's own excellent work. Indeed, his review of punishment procedures receives a re-presentation in this book.

Linked to this question of target audience is that of presentation of the material. Gardner claims he is using a technique of 'reinforcement through repetition' (p. vi) involving repeated statement and restatement of various concepts. It is difficult to see what is supposed to be reinforcing what in the situation in which a reader reads the book. Elsewhere (p. 256) Gardner comments that repetition *per se* is not reinforcing. The style of the book does not seem to stem from any valid translation of operant principles into the form the material takes and is to be contrasted with a more successful attempt by Ferster and Perrott in *Behaviour Principles*. Its effect in the present book is to make it overlong and sometimes tedious by constant repetition.

Within the behaviour-modification movement there are basic differences of opinion as to what information is relevant to the development of remedial programmes. At one extreme there are advocates of an approach that starts and ends with a ongoing evaluation of the relevant behaviour, rejecting information from other areas of psychology as simply irrelevant. In contrast to this position, other behaviour modifiers, while obviously rejecting ontogenetic determinism, are looking to psycholinguistics and developmental psychology for remedial content. Gardner's position appears to be closer to the former view. This has both strengths and weaknesses. He rightly hammers home the argument that if assessment does not lead to remediation then it is irrelevant. On these grounds he rejects a variety of types of personality assessment. His position, however, fails to take into account the possibility that personality assessment of a behavioural kind might be highly pertinent to remedial activity. This is somewhat surprising in that Gardner himself has helped to develop such a possibility in his earlier work. The issue of personality assessment is given simply as an example of Gardner's position. The interested reader should be aware of the fact that behaviour modifiers are not all as unwilling to relate their techniques to findings from other areas of psychology.

The need for such a *rapprochement* is certainly considered desirable by the present reviewers. So is a clarification of the relation between (*a*) the remedial objectives of behaviour modifiers and educational philosophy, and (*b*) the relation of demonstrably effective techniques and the justifiability of their use. To Gardner's credit he is explicit with regard to (*a*) if not detailed in his consideration. The aim of rehabilitation, and assumedly education itself, is to enable the individual to function effectively in a competitive, free-enterprise society. Successful adaption to an institution can also be an intermediate objective, as much of his work shows, but social rehabilitation is the overall goal. Both these objectives are open to debate, though Gardner gives no critical assessment of them. Regarding (*b*), Gardner's position is again explicit and again open to objection. It is most clearly stated in his discussion of punishment. Gardner distinguishes between punitive action and punishment as a therapeutic technique. He comments that 'In general it appears that punishment is frequently rejected as a treatment procedure, not on the basis of an objective evaluation of scientific data, but rather on the basis of ethical, philosophical, and socio-political considerations' (p. 281). What Gardner implies is that, if as a non-punitive, therapeutic technique punishment is effective, then objections from the quarters noted are

simply irrelevant. It may be argued that this view is intrinsic to the pragmatic nature of behaviourism, but again such a position is open to debate even among behaviourists.

In this review we have concentrated on problems of presentation and philosophy. Within the limitation we have noted Gardner presents a mass of material with which interested specialists should be acquainted. This material stretches from excellent discussions of behavioural evaluation and functional analysis (chapters 8 and 9), through a variety of types of progammes for the retarded, to a description of a total behaviour-modification environment by Screven, Straka and LaFond (the only chapter by other authors). We are not aware of any other source so comprehensive as this, and despite our reservations would commend the book to all those entering this area or in contact with those who work with the retarded.

JAMES HOGG, TOM FOXEN

Behaviour Modification in Clinical Psychology. Edited by C. NEURINGER and J. L. MICHAELS. New York: Appleton-Century-Crofts. 1970. Pp. viii + 261. $7.50.

Behavioural techniques are now widely applied to human problems. The breadth and vigour of this development in applied psychology are apparent in this collection of papers edited by Neuringer and Michael. The contributions are by some of the most eminent pioneers of behaviour modification, and were originally presented at the Ninth Annual Institute for Research in Clinical Psychology in 1967, sponsored by the University of Kansas Department of Psychology. The Institute was held to consider the role and place of behaviour modification in clinical psychology. Probably as a consequence the papers appear to have been directed at an average clinical psychologist who presumably has read little of behaviour modification. The approach is exclusively operant, but this shortcoming is insignificant when one considers the variety of problems reported to be amenable to the solutions developed.

The opening paper by Neuringer discusses criticisms of behavioural clinical psychology with a brevity highly suited to the Institute and acceptable in this volume, since the issues – 'symptom substitution', for example – have been dealt with in detail elsewhere. However, some readers will object to the absence of any references.

Baer and Wolf describe four cases in which they assisted a preschool children's teacher to systematically use her normal attention-giving activities to develop and maintain satisfactory levels of social interaction.

A concise, useful review of aversive conditioning is provided by Kushner. He adds descriptions of five cases and briefly refers to such recent procedural variations as patient-controlled shocking.

Michaels deals imaginatively with the problems of rehabilitating disabled adults. Most penetrating are his analyses of the typical and often debilitating interactions between disabled persons and others, such as members of their families, the institutional staff and friends.

Krasner gives an interesting account of the characteristic virtues and implications of behaviour modification. Behavioural procedures are not 'dehumanizing'; they help individuals to be more free through increasing their repertoire of behaviours and gaining greater control over their own environment. He also believes that behaviour modification must develop by incorporating knowledge from experimental work on social influence (e.g. modelling).

We will progress towards understanding of human aggression, according to Ulrich and Favell, by using procedures which have revealed environmental control of aggression in other species. They report two of their studies as examples. But they believe that scientists need to find ways of studying control of aggression using natural groups such as whole hospitals, even cities.

An ingenious analysis of the social development of deviant behaviour is illustrated by Patterson and Reid with an account of their programme for the family of a delinquent boy. The boy's 'coercion' (the maintenance of reinforcing behaviours of another person by the withdrawal of an aversive stimulus) in the home, as well as his wandering and fire-lighting, decreased satisfactorily following direct training of the family in 'reciprocity' (the maintenance of reinforcing behaviours in another person by positive reinforcement).

Kanfer describes support for his notion that self-regulation behaviour can be modified and has a function in performance. He and others have reported success in training patients to modify their own behaviour and to learn self-control.

There is a brief account by Michael of three papers by Ayllon, Lovaas and Goldiamond, unfortunately not included in this volume because of previous commitments, and an appendix in

which Baer argues for the therapeutic use of punishment when the individual's total suffering is thereby reduced.

Lindsley's method in using the Institute summary as an appropriate consequence of paper presentation may be deduced from his conclusion that 'the most highly rated make two points, a half a slide and a laugh per minute!'

This book has little that is new for those who have read most of the published work on behaviour modification. Nor will it satisfy those seeking an exhaustive, critical review. But it succeeds very well as an introduction to behaviour modification for anyone in the helping or training professions. The writing is clear and informative, and the few technical terms are well defined. A certain excitement is engendered by the repeated achievement of solutions to heretofore intransigent problems. Also impressive is the confidence that the application of behavioural analysis is bringing us steadily closer to scientific solutions of serious social problems. However, there is ample respect for the difficulties of the enterprise. This is a realistic but very hopeful book.

DAVID S. HART

The Social Psychology of the Child with Epilepsy. By CHRISTOPHER BAGLEY. London Routledge & Kegan Paul. 1971. Pp. xii +307. £3.25.

This fascinating and careful account of a multidisciplinary study of 83 epileptics with some behaviour disturbance, of their matched controls, and of 35 normal epileptics deserves wide attention. The epileptic subjects were gathered from the 183 seen at Maudsley over a three-year period. The controls were drawn from a north-west London child guidance clinic. The data that are presented about them cover developmental, environmental and familial factors, and relate in detail to the forms of the actual behavioural disturbances. The methods that were used covered a broad spectrum, and involved interview and other material gathered from the parents and their children by neurologists, psychiatrists and social workers, the results of formal psychological testing and some information about the children at school. One wishes that there were more studies like this, although the great cost is an obvious impediment.

Part 2 (pp. 15–113) surveys the literature since 1947, considering the factors that are likely to be implicated. So there are chapters on diagnosis, epileptic equivalents, the effects of drugs, body build, social environment and so on. The last part presents the results of the present study: the main method of data analysis was a non-parametric dichotomizing of each main variable against the others. There is also a parametric analysis of the data, based on factor analysis.

The whole study is deliberately integrative, the purpose being to discuss material relevant to sociology, biology and psychology to find what underlies the behavioural disorders of these epileptic children. Due (and often repeated) recognition is given to the fact that disturbed behaviour is multiply-determined and becomes a complex interaction of the separate components. Parental attitudes and minor motor fits are identified as the most potent factors in the behaviour disturbance, with environmental hazards and low intelligence the other main influences.

The primary psychological interest lies in the detailed analysis of the correlates of behaviour disturbance in these children, in the ways that it depends on social background, and in the analysis of aggressive and anxious behaviour as the two fundamental styles of response. But the book finishes as a sociologist's account and in the final chapter an integration of the disciplines is made through the concept of role performance. The task set out for the future is to study the constraints on role behaviour. This interpretation is rather dissonant with some of the earlier material, and there is no explicit account of the ways in which the epileptics learn or have any particular role behaviours forced on to them. But this is a minor criticism of an encyclopaedic survey of the literature on epilepsy, and of a highly efficient reduction of a wealth of data, the broad conclusion from which gives an emphasis to the social or environmental factors that precipitate behavioural disturbances, even in those with a neurological malfunction. At the very least this study must serve to reduce some of the unfortunate prejudice that epileptics have been heir to.

L. B. BROWN

Psychology at Work. Edited by P. B. WARR. Harmondsworth: Penguin Books. 1971. Pp. 460. 75p.

Warr has set out to present 'an up-to-date record of psychologists' thinking and research into behaviour and experience at work'. There are 17 chapters, all by individuals well known in their particular field. Although it is recognized that students and practitioners of many disciplines, such as industrial relations, business and management, would find material of interest, the book is aimed explicitly at undergraduate and graduate students of psychology. In his introduction Warr states the reasons for the choice of readership. Briefly, they relate to what Warr regards as the present over-concentration of academic psychology, as taught in the universities, on the laboratory experiment, characterized by methodological rigour and conceptual purity but bearing little relationship to behaviours as they occur outside the laboratory. Warr argues for a psychology which, while scientific and scholarly, is nevertheless involved in day-to-day problems. Both students and the public at large demand such an involvement – this is the request for 'relevance'.

It is a view which I share and I was pleased to find myself agreeing very strongly with Warr and hoping that the contents of the book would embody his aims and intentions. To a considerable extent they do; the problems reviewed are generally those which arise in industrial settings, and many of the writers show a commendable impatience with psychological theories which have been set up to account for laboratory findings as an end in themselves. Unfortunately, it is striking that *all* the contributors are listed as working either in academic departments of psychology or in research units; none are, currently at any rate, employed as practitioners of occupational psychology. If Warr is seeking exemplars to encourage undergraduates to enter applied occupational psychology, at least a smattering of scientist-practitioners would have been of benefit. This is a point which is now being grasped by clinical psychology. The old Maudsley model of the backroom scientist as hero is becoming redundant. Instead, it would be remarkable to come across an analogous compilation of clinical applications of psychology which did not include authors who were engaged, for at least part of their time, in the *practice* of clinical psychology. This omission – which probably reflects the present state of development of occupational psychology rather than any deliberate intent on the editor's part – is related to the only serious general flaw. Inevitably, few of the undergraduates attending courses in occupational psychology will become university teachers or researchers; many more will enter applied settings. An introduction to the difficulties they are likely to encounter as applied psychologists, methodological and otherwise, presented either as a separate chapter or as a section within each chapter, would have considerably strengthened the admirable overall level of the book.

There are very good chapters by Broadbent (an overview), Kay (accidents), Griew (ageing), Drenth (selection), Sime and White (decision-making), Stephenson (negotiating) and Payne (organizations). The contributions by Wilkinson (cyclicity), Annett (complex learning), Warr (judgements – particularly good on interviews) and Fineman and Warr (management training) are all of similarly high quality. Two chapters relating to the motivational aspects of the work situation (both by Blackler and Williams) are much less successful and seem to be written more for managers than final-year undergraduates (the usual time for the 'occupational option'). Poulton (skill and stress) similarly undershoots his audience. While I agree with Singleton in his strictures about the desirability of cooperation between engineers and psychologists, in his contribution (man–machine systems) he throws out the baby with the bath-water – the phrase 'no previous knowledge of psychology' being particularly apposite.

In sum, excellent value for money, and a very good text for an undergraduate course in occupational psychology, particularly when taught by a present or former practitioner.

PHILIP FELDMAN

The Psychology of Commitment: Experiments Linking Behaviour to Belief. By CHARLES A. KIESLER. New York: Academic Press. 1971. Pp. xii + 190. $9.95 and $4.65.

Kurt Lewin stressed the role that public commitment plays in 're-freezing' attitudes after the 'unfreezing' initiated by guided group discussion. Kiesler suggests that the impact of group decision on individual behaviour stems from subjects' impressions that they are not forced to behave in this particular way. Their 'commitment' is correspondingly high, commitment being defined as 'the pledging of the individual to behavioural acts'. If the acts contradict beliefs, the latter will change to support the pledge. Congruent acts are said to increase an individual's resistance to subsequent attacks on belief.

Kiesler's experiments concern the latter assertion. The openness of acts, their number, a subject's freedom to choose, and other factors are used to give substance to his concept of commitment. The results broadly confirm expectations, and throw some light on the 'boomerang' effect observed in previous studies by a number of different workers. For example, a well-constructed field experiment demonstrated that women who signed a petition in favour of giving birth control information to school children were subsequently more likely to respond to subsequent attacks on their beliefs by increasing their willingness to do voluntary work for pro-birth control agencies than were non-attacked but equally committed subjects. In contrast, uncommitted subjects yielded to the onslaught, and were subsequently less charitably inclined than their non-attacked counterparts. This divergent effect of attack on 'committed' and 'uncommitted' subjects is repeatedly demonstrated: Kiesler's experimental work is a welcome clarification of an interesting phenomenon.

Naturally, Kiesler must discuss his work in the light of dissonance theory and other related theoretical approaches. Harold Kelly's recent contribution to attribution theory informs the most useful of these ventures. For the most part, Charles Kiesler's theoretical sorties are a frustrating mixture of common sense, muddle and myopia. The hitherto unpublished experimental work does, however, ensure this work a place in well-stocked psychology libraries.

GEOFFREY M. STEPHENSON

Social Stress and Chronic Illness: Mortality Patterns in Industrial Society. By DAVID L. DODGE and WALTER T. MARTIN. Notre Dame and London: University of Notre Dame Press. 1970. Pp. xxvi + 331. $9.95.

This is an infuriating book to review. Indeed, I was tempted at first attempt to return it to the editor on some spurious grounds such as denying its relevance to readers of the *Journal*. Further thought, however, persuaded me that in the interest of interdisciplinary communication I should bring its existence to the notice of those sufficiently intrigued by the title to pause and look. Let me first despatch it as a *book*. It is printed in electromimeograph type – in other words printed typing – which is not conducive to reading and if it is presented this way then far better to have economized on the text and had it properly type-set. Secondly, it lacks an index, which immediately debars it from being a serious book for serious readers. Perhaps the authors would argue that the chapter synopses are detailed enough, which is not true, and even if it *were*, in a book of this degree of sophistication an index is still vital.

Now to return to it as a piece of scholarship. It is a serious attempt in an important area to try and understand the dynamics of chronic disease prevalence and social structure. The method adopted is a respectable one of building up a theoretical model of a possible system and then adducing evidence from morbidity and mortality statistics to support the model. Its importance for psychologists interested in this area is that it extends the psychosomatic hypothesis in directions in which this hypothesis has clearly been unsatisfactory. Whether the authors have *succeeded* in their task of matching model and reality I am quite unable to tell. Struggle as I might through the turgid prose I only got more and more confused, and a concluding chapter entitled 'summarization' might have warned me that no help would be forthcoming there. These reading difficulties are not helped by the fact that the major independent variables are really not clearly defined anywhere, so that however convincing the evidence the reader ultimately gives up!

So why did I still choose to review it? Only because in Part 1, which is entitled 'Social Change and Changing Causes of Death in Industrial Societies', there is a really thoroughgoing evaluation of the presently accepted disease models and the many shortcomings which they have in limiting discussion, understanding and research into the complex problems of chronic disease. If only this had been published as a paper!

DOUGLAS HOOPER

The Schizophrenic Syndrome: an Annual Review. Volume 1. Edited by R. CANCRO. London: Butterworth. 1971. Pp. xv + 791. £9.00.

This review draws upon 22 journals for its 51 papers, which are reproduced *in toto* under eight sections. The greater part of the book, in fact, is given over to 'Historical Development, Classification, and Theoretical Considerations' (Section I), 'Biochemical Studies' (Section IV) and 'Therapy' (Section VIII). 'Dynamic Considerations', 'Social, Developmental and Existential Factors', 'Genetic Studies', 'Cognitive and Perceptual Studies' and 'Prognosis' are less strongly emphasized.

Underlying this format it is suspected that Cancro has included papers for three different reasons. In the first place he has selected a number of historical review articles and transcripts of conference addresses by the elder statesmen of American psychiatry, based upon their lifetimes' experience, which at times tend to duplicate one another and to lack topicality. Perhaps the best of these is the article by Grinker, in which over the space of 40 pages he provides an interesting historical perspective for current research and points out recurring problem areas in such a lucid and easy manner that several other contributors are almost entirely pre-empted.

Under the second heading one suspects that Cancro has endeavoured to provide a balanced overall view by deliberately selecting a number of papers which clearly overstate their own case. Heath on biochemical aspects of schizophrenia, Lidz on family dynamics (he, in particular) and Siegler *et al.* on the follies of Laing must all alienate more readers from their viewpoint than they recruit to it, but just as optical illusions provide insight into visual perception, so this arrangement encourages and demands more constant critical appraisal than any procession of uncontroversial platitudes would achieve in a volume of this length.

The third section contains the meat of the book and includes some excellent articles. Particularly memorable is the attempt by Ornitz to relate adult schizophrenia to early infantile autism, and a cross-national study of diagnostic habits by Gurland *et al.* which is the best of the papers endeavouring to point out that diagnosis is something less than scientific. Pride of place, however, must go to Epstein and Coleman's paper on drive theories of schizophrenia, in which they provide one of the most lucid appraisals available of the theories of Mednick, Broen and Storms, Venables, McReynolds, Pavlov, and, of course, Epstein himself. The précis of Pavlov's theory is particularly recommended. The only regret is that this paper comes so early in the book (pp. 17–52), since this height of clarity and depth of understanding is never again reached in the whole remaining 729 pages.

The section on biochemistry, occupying the central 117 pages, deserves a special mention, since this is the only part of the entire book which is not readily understandable by any reader of first-degree standard without specialist knowledge. Phrased otherwise, however, it is a compliment to the editor that 90 per cent of the book will be of interest to and easily grasped by psychiatrists (for whom it is primarily intended), psychologists, general practitioners and even interested laymen, both for the wide overview it gives of the field and for the insight it provides into the strengths and weaknesses of the individual schools of thought. Nonetheless, it is for the biochemical section that next year's review is most eagerly awaited, since it is primarily in this area that the impression is given that real progress is being made.

It will be a disappointment to psychologists that psychological research is scarcely represented even in a volume as eclectic as this. To have included some of the better papers on the operant conditioning of psychotic patients as an aid to rehabilitation would have been in keeping with the general tenor of the book, and it is a matter of regret that no psychometric techniques since the MMPI and the Rorschach have impressed the psychiatric field with their utility, in spite of the tangled and apparently hopeless state of nosology and classification which bedevils many of the papers included in the review and all but precludes comparisons between them. Does no one wish to help eliminate such complaints as schizomania, phasophrenia or schizanoia?

Finally, there are two small quibbles with the presentation of this book. First, there are instances where whole lines of print are juxtaposed or omitted and all but precipate unbalanced episodes in those of us ill-equipped to tolerate such stress, and secondly, in a reader of such length which few will read from cover to cover, it would be of enormous assistance if the excellent list of contents at the beginning of the book could be augmented by an index, especially as succeeding volumes are published.

S. A. LOCKWOOD

Personality Assessment. By RICHARD I. LANYON and LEONARD D. GOODSTEIN. London: Wiley. 1971. Pp. xii + 267. £4.00.

Readings in Personality Assessment. Edited by LEONARD D. GOODSTEIN and RICHARD I. LANYON. London: Wiley. 1971. Pp. vii + 792. £5.52.

These are companion volumes but, like Hall and Lindzey and Lindzey and Hall, they can be considered and used as separate books. First, *Personality Assessment*. It begins with a chapter, short but pithy, on the history of personality assessment, going on to chapters on Concepts and Definitions, Rational–Theoretical Approaches to Assessment, Empirical and Internal Consistency Approaches, Behaviour Samples and Biographical Data, Reliability and Validity, Response Distortions, Problems of Application, and finishes with two very up-to-date and forward-looking chapters on Criticisms of Personality Assessment and New Directions. The only criticism I have of the arrangement and order of the chapters – and I know that this is arguable – is that I would have preferred the behaviour samples and biographical data to have come earlier in the book. The only other small complaint – and this is essentially a reviewer's – is that one has to read every word because the authors don't waste any. Perhaps this is because one of the authors was an engineer before he came into clinical psychology.

The parts I particularly liked were those on reliability and validity and the first-rate discussion on clinical versus actuarial prediction. The whole book could be read by those people whom British Association paper-givers are asked to bear in mind, 'the intelligent educated laymen and scientists from other disciplines'. The penultimate chapter of the book, devoted to the improper usage of personality assessment devices and the moral issues involved, should be obligatory reading for journalists and the over-enthusiastic pedlars of personality tests. The book has almost certainly taken a long time to compile, but it will stand for a very long time, even though research findings overtake the authors; the basis, the approach and temper of the whole book are *so* rational.

The reader will want to know how it compares with Hall and Lindzey and whether it should take its place. The reviewer's answer is a firm No, as the Hall and Lindzey is concerned entirely with theories of personality. Nor does it take the place of Sarason's *Personality: an Objective Approach* or Vernon's *Personality Assessment: a Critical Survey*, both of which are quite different books from Lanyon and Goodstein's. Sorry, all are needed in a booklist for students.

The plan of the main text is reflected in that of the companion volume, the 'Readings'. So, for example, articles by Galton, Jung and Taft, amongst others, appear in the first chapter, 'History of Personality Assessment'; articles by P. E. Vernon, Allport, P. A. and L. R. Goldberg, Husek, Edwards and Meehl (two) appear under appropriate chapter headings, parallel to the chapters in the main text. I was delighted to see Cureton's 'Validity, Reliability and Baloney' reprinted.

The only thing I did not like about the two volumes is the silly book-cover, which looks like a modernistic illustration of a snook being cocked. Very inappropriate, most misleading, but do not be put off by it.

DENIS MCMAHON

Readings in Extraversion–Introversion. Volume 1: *Theoretical and Methodological Issues.* Volume 2: *Fields of Application.* Edited by H. J. EYSENCK. London: Staples Press. 1970 and 1971. Pp. 416 and 354. £4.00 and £4.95.

These volumes bring together a set of previously published papers that give a background and some applications for Eysenck's theory of extraversion. The first volume has sections on history and on measurement, and then turns to seven correlational and to eight genetic studies. The second volume deals with the role of extraversion in fields of education, industry, social behaviour, psychiatry and crime. Most of the papers were originally published in the 1960s, although there are a few earlier ones. Each book is divided into sections prefaced by a brief and not always informative editor's introduction. There is also a longer essay at the beginning of the first volume, and a third volume relates extraversion to the basic psychological processes (and has been already reviewed in this *Journal*, 1972, **63**, p. 144).

The detailed work that has gone into the development of this theory of extraversion and the breadth of its application are both sometimes forgotten. It would be good therefore to be able to regard these collections as in some sense definitive. But we are told in the introduction to the psychiatric section, for example, that those papers were chosen to fulfil the criteria of being 'interesting, unusual, and not easily obtainable'. It is also a pity that the plea for 'more and better work' is reiterated over most of the sections. That need must be widely recognized anyway, while the reason is not exactly clear why it should be said that 'lost opportunities abound in all the fields surveyed in this book, but perhaps more so in the field of social psychology than in any other' (p. 139). Reclaiming those opportunities is more important than bemoaning their loss.

Despite all this, these are useful if not definitive sources, especially for the kind of teaching that is increasingly arranged to give detailed attention and criticism to original papers – at least it is if the papers can be bought by the students at a reasonable price.

L. B. BROWN

Gerontological Psychology. By JAMES H. BARRETT. Springfield: Thomas. 1971. Pp. xv + 165. $8.00.

This book contains an 11-page appendix abstracting the results of a survey carried out by the author and his students upon the living conditions, opinions and behaviour of elderly people ('gerontos'). This would have been worth publishing as an article, but the rest of the book is little more than a statement of how mistaken are the views that society is assumed to have about old people; this plus a generous helping of gems such as 'the writer's mother, at ninety years of age, still eats hot chili, pizza and fried pork!' Whilst agreeing with the author that work in the field is sparse, this does not seem an adequate reason for ignoring it completely. Not a book to merit the attention of psychologists.

ANTONIA WHITEHEAD

Br. J. Psychol. (1972), 63, 4, *pp.* 653–655
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EDITORIAL ACKNOWLEDGEMENT

The Editor, Assistant Editor and Co-editors wish to acknowledge the help they have received in the past year from the following persons who have acted as referees for papers submitted for publication in the *British Journal of Psychology*.

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J. Richard Eiser *Department of Psychology, University of Bristol, England*
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Winter 1972, approx. 250 pp., £3.50

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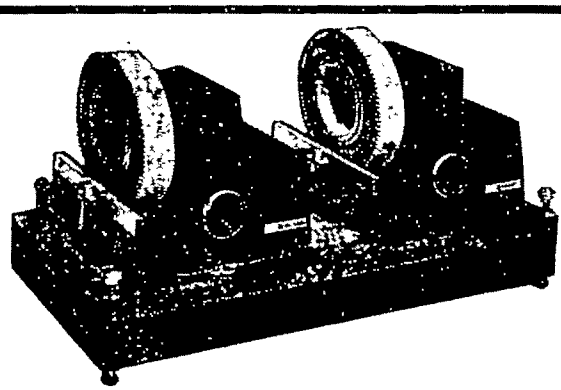

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